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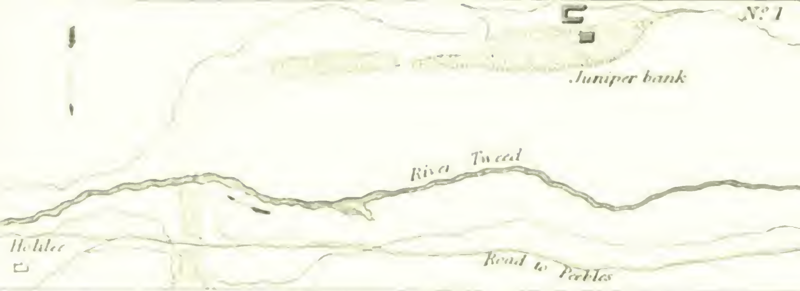

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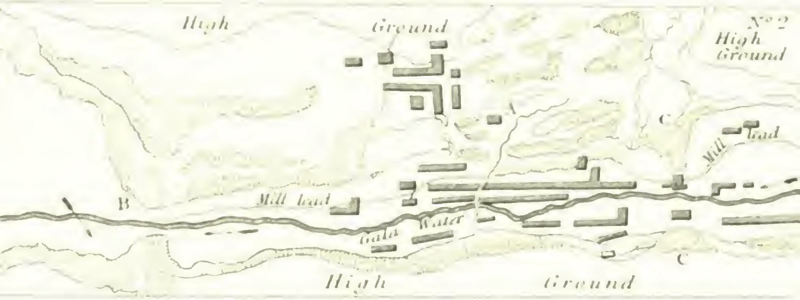

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XVI.
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VI. Illustrative of Dr. Draper's Paper on the Electro-motive Power of Heat.

VII. Illustrative of Mr. Smee's Paper on the Production of Electrotypes.

ERRATA.

Vol. xii. p. 608, line 2 from the bottom, *for* Manhand, *read* Marchand.

Vol. xv. p. 451, last line but three, *for* increasing indefinitely, *read* increasing or diminishing indefinitely.

——— p. 452, line 14, *for* $\cos m i = \sqrt{-1} . \sin m i$, *read* $\cos m i + \sqrt{-1} . \sin m i$.

——— p. 453, line 28, *for* ρ, a, *read* $\rho, a_{,}$.

——————, line 29, *for* a, *read* a_{r}.

——— p. 454, line 21, *for* (23.), *read* (33.)

——————, lines 22 and 23, *for* $e^{e x}$, *read* $e^{\epsilon x}$.

Vol. xvi., p. 32, line 15 from the bottom, *for* 1-2554th, *read* 1-3554th.

——— p. 108 line 19 from the bottom, *for* Haller, *read* Harvey.

——— p. 478, line 2, *for* M.R.S. *read* M.R.I.

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
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[THIRD SERIES.]

JANUARY 1840.

I. *On the Theory of Substitutions in Chemistry, proposed by* M. Dumas. *In a Letter to* M. Pelouze *from* M. Berzelius*.

YOU will perhaps permit me to return once more to M. Dumas's theory of substitutions, especially after the new development that this skilful chemist has lately given it, a development which, according to him, threatens to overturn the theory of chemistry in general, and especially electro-chemical notions. You remember that in a preceding letter, I declared my opinion, that the application which has been made of the theory of substitutions, in considering chlorine, which takes the place of hydrogen, as acting the same part as this latter, is contrary to the principles of the science, and I quoted some examples on this subject, which I think prove it in an unequivocal manner. In begging you to communicate the contents of this letter to the *Académie des Sciences*, I hoped that M. Dumas would have the goodness to explain under what point of view he regards the theory in question. He has, indeed, complied with this request, and in a manner which, with the exception of some details foreign to the question, has entirely satisfied me.

In the *Comptes Rendus* of 1838, ($1^{ère}$ semestre, p. 699 and the following pages,) M. Dumas has disclaimed this explanation of the theory of substitutions: "If I am made to say that chlorine takes the place of hydrogen in such a way that the former acts the same part as hydrogen, an opinion is imputed to me," says M. Dumas, "against which I protest, for it is opposed to all that I have asserted on these matters. The theory of substitutions merely asserts the simple affinity between hydrogen and the chlorine which takes its place in equal volume. It is an empirical law which deserves our attention only as long as it holds good, and if any one has made it of more importance it is not my fault."

---

* From the *Ann. de Chim. et de Phys.*, vol. lxi. p. 137.

This assertion of M. Dumas is in perfect harmony with my way of thinking; what has led to a misstatement of his opinion is probably the improper denomination of *theory of substitutions*, for an empirical law is not a theory, and the word 'substitution' has long since been used in chemistry to signify the replacing [*remplacement*]* of one body by another which acts the same part as this body, and M. Dumas has lately changed this word for another, which is very well chosen, *metalepsie*, signifying replacement [*remplacement*]*.

In the *Comptes Rendus* of 1839, (1^ère^ semestre, p. 909), M. Dumas has more lately given a new explanation of this theory, but quite opposed to that just mentioned. He made the beautiful discovery that crystallizable acetic acid $C^4 H^6 O^3 \dot{\underline{H}}$, exposed to the light of the sun in an atmosphere of dry chlorine, is gradually decomposed, and that an equal volume of chlorine completely takes the place of the hydrogen. A new acid composed of $C^4 H^6 O^3, \dot{\underline{H}}$, is the result of this action, some of whose properties he has described.

The conclusions which M. Dumas draws from this fact are the following: the chlorine in taking the place of the hydrogen atom for atom, *acts the same part as the hydrogen*; an acetic acid is the result of this, which differs only from the other in containing six atoms of chlorine instead of six atoms of hydrogen, and on this account he gives it the name of *chloracetic acid.* It possesses the same properties as the ordinary acid, so that if we know the properties of the latter, we equally know those of chloracetic acid. This is caused by the properties of a body depending rather on the type of the composition than on the particular characters of the element which has been exchanged. In organic chemistry, says he, there exist certain types, which are preserved even when equal volumes of chlorine, bromine, or iodine have taken the place of the hydrogen which they contain. He infers from this that there are two great leading principles in chemistry, which are *isomorphism* in mineral chemistry, and *substitution* in organic chemistry. He supposes that these principles originate from the same cause, and that in due time they may be generalised under one common expression.

M. Dumas adds that neither isomorphism nor the theory of substitution harmonizes with electro-chemical opinions, and as these latter have not, according to him, the property of yielding to facts or of explaining or predicting them, he dispatches the electro-chemical theory by saying, "It must be admitted there is nothing in it."

---

* As precision in the use of terms is very important in this subject, is not *metalepsy* objectionable as signifying either *participation* or *succession*?

With regard to the practice of rendering the French *remplacer*, *remplacement*, by the English 'replace', 'replacement', which answer to the French *replacer*, &c., and have an entirely different meaning, it is a slovenly shift, which ought always to have been resisted, as causing constant ambiguity and confusion. Can chemists find any authority for such a perversion of both languages?—R. T.

This is certainly a complete revolution in the state of chemistry.

When questions of such importance to science are treated of, the love of truth should ensure a mature examination of the propositions of the author of such a revolution, for the purpose of engaging us on his side if he is right, and of opposing him if he is wrong.

One of the great advantages of the theory of substitutions over electro-chemical opinions seems to be, that the type of composition preserves the same properties after the exchange of hydrogen for chlorine.

Let us then examine the few properties of chloracetic acid which M. Dumas has described, and compare them with those of acetic acid. It will be seen that these two acids differ infinitely more from each other than do formic acid and acetic acid.

ACETIC ACID.	CHLOROACETIC ACID.
Smell strong and pungent.	Scarcely any smell.
Liquid at common temperatures.	Solid at common temperatures.
Solidifies at about 50° Fahr., boils at about 212°.	Solidifies at about 187° Fahr. Boils at about 392°.
Specific gravity 1·06.	Specific gravity 1·617.
Potash salt deliquescent.	Potash salt unalterable in the air.
Æther very fluid, odour æthereal; floats on water.	Æther oleaginous, odour like mint; sinks in water.

These two acids, therefore, have no other resemblance than that which belongs to acids in general, and it might be stated that chloracetic acid resembles sulphuric acid as much as it does acetic acid. Thus the type does not lead to an anticipation of the particular properties of an acid in which hydrogen is replaced by* chlorine.

With respect to the composition of chloracetic acid, M. Dumas is of opinion that it is formed of three atoms of oxygen and of one atom of a radical composed of four atoms of carbon and of six atoms of chlorine. In examining into this composition, it is natural to inquire if other bodies which have an analogous composition are not known, and whether M. Dumas's opinion is not equally applicable to them, for it is evident that any explanation not applicable to similar cases ought to be rejected.

---

* "*remplacé par.*" See preceding notes.—R. T.

Chloracetic acid is composed of carbon combined with chlorine and oxygen. There exists a numerous class of similar combinations; it is divided into three subdivisions according to the radicals combined with the chlorine and with the oxygen; these shall be considered in rotation.

1. *Simple electro-positive radicals.* — This subdivision is very numerous and well known: it consists of the basic muriates of the ancient theory of the nature of hydrochloric acid. The general opinion, which has not been contested, is that these bodies are composed of a chloride and an oxide. They are readily decomposed and recomposed.

In applying the new theory of M. Dumas to the compound $\mathrm{Pb}\ \underline{\mathrm{Cl}} + 3\,\dot{\mathrm{P}}\mathrm{b}$, it should be expressed by $\mathrm{Pb}^4\,\mathrm{Cl}^2 + 3\,\mathrm{O}$, in which $\mathrm{Pb}^4\,\mathrm{Cl}^2$ is a radical composed of lead and chlorine.

I am persuaded that M. Dumas would protest against this extension of his views; but if they are true, however, it is requisite to do so, and to be consistent, we must go still further, and consider the salts of the oxacids as oxides of a radical composed of that of the acid and also of the base, sulphate of potash, for example, as $\mathrm{KS} + 4\,\mathrm{O}$.

2. *Simple electro-negative radicals.*—The number of combinations known of this class is very limited. They are to be considered under two different points of view. M. Henry Rose, who discovered the combination of chromium belonging to this class, considers it as composed of $\mathrm{Cr}\ \underline{\mathrm{Cl}}^3 + 2\,\dddot{\mathrm{C}}\mathrm{r}$, and this manner of viewing it has been generally adopted. It has since been proposed to consider it as chromic acid in which an equivalent of chlorine is substituted for an equivalent of oxygen. This manner of viewing it is more simple, but it is easy to prove that it is not correct, for the chloride of chromium may be replaced by other chlorides*, and we have several examples of salts formed of an atom of a chloride and of two atoms of chromic acid; but the preceding hypothesis admits only a single relative number of atoms of chlorine and of oxygen, although several are known to exist. Consequently, the only admissible explanation in this class of bodies, is that of considering them as composed of a chloride and of an oxide.

The following are the only combinations of this species hitherto discovered:—

Radicals.	Formula.	Authors.
Sulphur	$\underline{\mathrm{S}}\,\underline{\mathrm{Cl}}^3 + \dddot{\mathrm{S}}$	Regnault.
Do.	$\underline{\mathrm{S}}\,\underline{\mathrm{Cl}}^3 + 5\,\dddot{\mathrm{S}}$	

---

* "*se laisse remplacer par d'autres chlorures.*"

Radicals.	Formula.	Authors.
Carbon	$\underline{C}\,\underline{Cl}^3 + \overset{\cdots}{\underline{C}}$*	Dumas (chloracetic acid.)
Do.	$C\,\underline{Cl}^2 + \ddot{C}l$	J. Davy (phosgene gas.)
Molybdenum ...	$Mo\,\underline{Cl}^3 + 2\overset{\cdots}{Mo}$	H. Rose.
Tungstenium ...	$2\,W\,\underline{Cl}^3 + \overset{\cdots}{W}$	Bonnet.
Do.	$W\,\underline{Cl}^3 + 2\,\overset{\cdots}{W}$	H. Rose.
Chromium	$Cr\,\underline{Cl}^3 + 2\,\overset{\cdots}{Cr}$	Do.

You observe then that the chloracetic acid of M. Dumas is merely a combination of oxalic acid with a chloride of carbon, corresponding to the oxalic acid itself. Its acid characters will be hereafter considered.

3. *Compound Radicals.*

Radical.	Formula.	Authors.
Acetyle	$2\,C^4\,H^6\,\underline{Cl}^3 + C^4\,H^6\,O^3$	Malaguti (chloric æther.)
Formyle	$\underline{C}^2\,\underline{H}^2\,\underline{Cl}^3 + C^2\,H^2\,O^3$	Laurent (acetate of methylen with chlorine).
Benzoïle	$C^{14}\,H^{10}\,\underline{Cl}^2 + 2\,C^{14}\,H^{10}\,O^3$	Liebig and Whœler (chloride of benzoïle).
Elayle (†)	$C^2\,H^4\,\underline{Cl} + C^2\,H^4\,O$	F. d'Arcet (chloretheral).
$C^3\,H^4$	$C^3\,H^4\,\underline{Cl}^2 + C^3\,H^4\,O^2$	Kane (methylic chloral).
$C^6\,H^3$	$C^6\,H^3\,\underline{Cl}^3 + C^6\,H^3\,O^2$*	Laurent (chlorophénésic acid).
$C^6\,H^4$	$C^6\,H^4\,\underline{Cl}^2 + C^6\,H^4\,O^2$*	Id. (chlorophénésic acid).
$C^7\,H^8$	$C^7\,H^8\,\underline{Cl}^2 + C^7\,H^8\,O^5$*	Piria (chloride of salicine).
$C^{14}\,H^{10}$	$C^{14}\,H^{10}\,\underline{Cl}^5 + 4\,C^{14}\,H^{10}\,O^5$*	Id. (Id.)
$C^{14}\,H^6$	$C^{14}\,H^6\,\underline{Cl}^3 + C^{14}\,H^6\,O^3 + \underline{NH}^3$	Id. (chlorosamide).

The combinations marked with an asterisk possess the property of uniting with bases without abandoning the chloride combined with them, resembling in this respect the vegeto-sulphuric and vegeto-nitric acids, &c. &c. into the composition of which many different substances may enter. These combinations are five in number: observe then that the chloracetic acid is not a solitary instance of a compound of this kind, and that in order to explain its properties, it is by no means requisite to isolate it from other analogous combinations, and on its account to imagine new views contrary to the general system of chemical knowledge.

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† The name which I have given to the substance $C^2\,H^4$ in the oil of the Dutch chemists and in the æthereal salts of platina.

The chloric æther of M. Malaguti may be considered in two different points of view. As obtained directly with æther and chlorine, it is very probable that it is composed, as above mentioned, of an atom of acetic acid and of two atoms of chloride of carbon, corresponding to the radical of this acid. But when, for example, benzoate of ethyle is decomposed by chlorine, it appears that the substance produced, and which remains in combination with the benzoic acid, has the formula which I gave in a preceding letter,

$$C^2 H^6 O, + 2 C\underline{Cl}.$$

I will also add some examples of combinations of oxichlorides with other bodies; but they are confined to the oxichlorides of carbon. For the sake of brevity,

E will be = $C^4 H^{10}$, ethyle;
Me = $C^2 H^6$, methyle;
F = $C^2 H^2$, formyle.

$(C\,\underline{Cl}^2 + \ddot{C}) + (S\,\underline{Cl}^2 + \ddot{S})$	A. Marcet and myself (sulphuret of carbon treated with nitro-muriatic acid).
$(C\,\underline{Cl}^2 + \ddot{C}) + \dot{E}\,\ddot{C}$	Dumas (chloroxicarbonic æther).
$(C\,\underline{Cl}^2 + \ddot{C}) + \dot{Me}\,\ddot{C}$	Do. (chlorocarbonate of methyle).
$(C\,\underline{Cl}^2 + \ddot{C}) + F\,\underline{Cl}$	(Chloral).
$(C\,\underline{Cl}^2 + \ddot{C}) + F^2\,\underline{Cl}$	Kane (methylic chloral).

In the last the radical may also be considered as $C^4 H^4$, that is to say, that of the citric, tartaric and succinic acids.

In order then to avoid the revolution which threatens electro-chemical views, it will be seen we have only to place the symbols of the formula of chloracetic acid in an order rather different from that of M. Dumas; and by this slight change the new combination is placed in a class of previously known bodies. An epoch has arrived in which a glimpse of a chemical theory of organic combinations has been perceived; but if instead of allowing it to develop as experience extends, it is wished to base it upon isolated facts, viewed without regard to their relations with the system of general knowledge, and on explanations which do not harmonize with the principles of the science; and if, besides, it be concluded that this want of agreement should make us reject as erroneous principles which are otherwise well founded, the truth will never be arrived at.

This is nearly all which I have thought necessary to say upon this occasion for the defence of electro-chemical opinions; but before I conclude, I will add a few words relative to the discussion on the organic metamorphoses produced by heat, a discussion in which MM. Dumas, Peligot, and Payen have given an opinion contradictory to mine.

M. Dumas thinks that I have deceived myself, because he has found an ethylic combination which includes metamorphosed citric acid. The existence of this combination proves neither more nor less than the existence of the other metamorphosed combinations.

M. Payen has shown that the amylate of lead, heated to 266° Fahr. yields no more water when heated to between 266° and 356°, without being partially decomposed. This is precisely what I have myself proved. I did not examine what took place below 266°. With regard to this I referred to the data contained in the memoir published by this skilful chemist upon starch.

M. Peligot, who, I know not why, considers as an attack upon him the explanation which I have given of an observation made by him, and the accuracy of which I have verified, M. Peligot endeavours to prove that I have advanced inaccurate facts. I am persuaded that M. Peligot obtained the results which he quoted in opposition to my views; but I must beg of him to believe as much of mine. The question is then reduced to determining the conditions under which a body metamorphosed by heat is reproduced by the action of water, and those under which it is not. A careful study of organic metamorphoses will, perhaps, discover these conditions. I shall perhaps at some future period return to this subject. Your devoted friend,

JAC. BERZELIUS.

Stockholm, July 15, 1839.

Additional Note on Oxichlorides with Compound Radicals.

By M. BERZELIUS[*].

THE excellent work of M. Malaguti on the reaction which chlorine exerts on æthers, affords no less than seven new examples of this kind of combination.

1. *Chlorinated sulphuric æther* of M. Malaguti is composed, as we have shown, of

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[*] From the *Ann. de Chim. et de Phys.*, vol. lxi. p. 340.

$$\begin{array}{rl}
\text{1 at. of acetic acid ...} = & 4\,C + 6\,H \qquad + 3\,O \\
\text{2 at. of acetic chloride} = & 8\,C + 12\,H + 12\,Cl. \\
\hline
(\dddot{A} = C^4\,H^6\,O^3 \text{ and } A\,\underline{Cl}^3 = C^4\,H^6\,\underline{Cl}^3)\ \dddot{A} + A\,\underline{Cl}^3 = & 12\,C + 18\,H + 12\,Cl + 3\,O
\end{array}$$

which contains three times the number of simple atoms, in the formula of M. Malaguti = $C^4\,H^6\,Cl^4\,O$.

2. *Sulphuretted æther*, $C^4\,H^6\,S^2\,O$, contains, in tripling the number of simple atoms,

$$\begin{array}{rl}
\text{1 at. of acetic acid ......} = & 4\,C + 6\,H \qquad + 3\,O \\
\text{2 at. of acetic sulphuret} = & 8\,C + 12\,H + 6\,S \\
\hline
(A\,S^3 = \overset{'''}{A}) \quad \dddot{A} + 2\,\overset{'''}{A} = & 12\,C + 18\,H + 6\,S + 3\,O.
\end{array}$$

3. *Chlorosulphuretted æther*, = $C^4\,H^6\,S\,Cl^2\,O$, the number of simple atoms being multiplied by 6, contains two atoms of acetic acid, two atoms of acetic sulphate, and two atoms of acetic chloride, which may be represented by the combination of an atom of No. 1. and an atom of

$$\text{No. } 2 = (\dddot{A} + 2\,\overset{'''}{A}) + (\dddot{A} + 2\,A\,\underline{Cl}^3).$$

It is probable that in treating the acetic oxichloride (No. 1) by sulphuret of lead, we shall obtain No. 2 or No. 3, as the decomposition may be more or less complete; just as when

$$Bz\,\underline{Cl}^3 + \dddot{Bz}\ (Bz = C^{14}\,H^{10})$$

is treated by sulphuret of lead, we obtain $\overset{'''}{Bz} + 2\,\dddot{Bz}$.

4. *Chlorinated acetic æther*, $Cl^5\,H^{12}\,C^{14}\,O^4$, gives, on multiplying the number of simple atoms by $1\frac{1}{2}$,

$$\begin{array}{rl}
\text{2 at. of acetic acid ...} = & 8\,C + 12\,H \qquad + 6\,O \\
\text{1 at. of acetic chloride} = & 4\,C + 6\,H + 6\,Cl \\
\hline
A\,\underline{Cl} + 2\,\dddot{A} = & 12\,C + 18\,H + 6\,Cl + 6\,O
\end{array}$$

This combination is proportional to those of sulphur, chromium, molybdenum, tungsten, and benzoyle, with chlorine and oxygen.

5. *Chlorinated formic æther*, $C^6\,H^8\,Cl^4\,O^4$, of which if we triple the number of atoms gives

$$\begin{array}{rl}
\text{2 at. of formic acid ......} = & 4\,C + 4\,H \qquad + 6\,O \\
\text{1 at. of formic chloride} = & 2\,C + 2\,H + 6\,Cl \\
\text{2 at. of acetic acid.........} = & 8\,C + 12\,H + \qquad + 6\,O \\
\text{1 at. of acetic chloride ...} = & 4\,C + 6\,H + 6\,Cl \\
\hline
F = C^2\,H^2)\ (\underline{\dddot{F}\,Cl}^3 + 2\,\dddot{F}) + (A\,\underline{Cl}^3 + 2\,\dddot{A} = & 18\,C + 24\,H + 12\,Cl + 12\,O
\end{array}$$

6. *Chlorinated camphoric æther*, $C^{14} H^{20} Cl^{4} O^{4}$, of which we triple the number of simple atoms, gives

2 at. of camphoric acid ... $= 20\,C + 28\,H + 6\,O$
1 at. of camphoric chloride $= 10\,C + 14\,H + 6\,Cl$
2 at. of acetic acid .... $= 8\,C + 12\,H + 6\,O$
1 at. of acetic chloride ...... $= 4\,C + 6\,H + 6\,Cl$

$$(Ca = C^{10} H^{14})\ (Ca\,\underline{Cl}^{3} + 2\,\dddot{Ca}) + (A\,\underline{Cl}^{3} + 2\,\dddot{A}) = 42\,C + 60\,H + 12\,Cl + 10\,O$$

7. *Chlorinated benzoic æther*, $C^{18} H^{16} Cl^{6} O^{3}$, multiplied by 3 gives

2 at. of benzoic acid ......$= 28\,C + 20\,H + 6\,O$
1 at. of benzoic chloride ...$= 14\,C + 10\,H + 6\,Cl$
1 at. of acetic acid ....$= 4\,C + 6\,H + 3\,O$
2 at. of acetic chloride......$= 8\,C + 12\,H + 12\,Cl$

$$(Bz\,\underline{Cl}^{3} + 2\,\dddot{Bz}) + 2\,A\,\underline{Cl}^{3} + \dddot{A}) = 54\,C + 48\,H + 18\,Cl + 9\,O$$

8. *Chlorinated œnanthic æther*, $C^{18} H^{28} Cl^{8} O^{3}$, does not contain the radical of œnanthic æther. M. Malaguti has found that the alkalis, in destroying the acetic oxichloride, combine with a new acid, which he analysed, and found to be composed of $C^{14} H^{22} Cl^{4} O^{2} + \dot{\underline{H}}$, the radical of which consequently is $C^{14} H^{22}$. Expressing this radical by the symbol x, the acid in the anhydrous state will be composed of

1 at. of the acid of the radical $x = 14\,C + 22\,H + 4\,O$
1 at. of chloride of the same $x = 14\,C + 22\,H + 8\,Cl$

$$x\,\underline{Cl}^{4} + \ddddot{x} = 28\,C + 44\,H + 8\,Cl + 4\,O$$

It is therefore one of those oxichlorides, the acid of which combines with the bases without separating the chloride. If, as M. Malaguti supposes, and as is probable, this body enters into the composition of chlorinated œnanthic æther, this latter contains

2 at. of acetic acid .... $= 8\,C + 12\,H + 6\,O$
4 at. of acetic chloride...... $= 16\,C + 24\,H + 24\,Cl$

3 at. of the acid $\ddddot{x}$ .... $= 42\,C + 66\,H + 12\,O$
3 at. of the chloride $x\,\underline{Cl}^{4}$... $= 42\,C + 66\,H + 24\,Cl$

$$2\,(2\,A\,\underline{Cl}^{3} + \dddot{A}) + 3\,(x\,\underline{Cl}^{4} + \ddddot{x}) = 108\,C + 168\,H + 48\,Cl + 18\,O$$

which makes two atoms of acetic oxichloride for three atoms of the oxichloride or radical $C^{14} H^{22}$, and six times the number of simple atoms indicated in the formula of M. Malaguti.

With regard to the methylic acetate, it appears that it furnished the same combinations as the ethylic formiate, as indeed would be foreseen by theory.

Chloropyromucic æther appears to contain, according to the last experiments of M. Malaguti, ethylic oxide, combined with an acid which is composed of an acid and of a chloride. When it is saturated with bases the acid does not separate from it. Any conjecture as to the composition of the acid and of the chloride united with it would at present be premature.

Stockholm, August 20, 1839. BERZELIUS.

II. *Experiments on Mechanical Exosmose with reference to determining the Existence of any difference of Magnitude between liquid and gaseous Particles.* *By* JULIUS JEFFREYS, *M.R.C.S. late of the Hon. East India Company's Bengal Medical Service.*[*]

EXPERIMENTAL inquiries into the molecular constitution of bodies, being in their nature very difficult, no means of bringing the position or the relative magnitude of their particles under observation have, so far as I am aware, suggested themselves, if we except a faint light which of late electricity, crystallography, and optics have beautifully thrown in among them.

The subject having been of too much interest and importance to be neglected, much speculation has supplied the place of experiment. In physical inquiries, however, hypothesis, valuable in connexion with experiment, is dangerous to philosophy without it, and I should not venture a word of the former, but in its relation to the latter.

The experiment I have to relate may be called, in the absence of any other term, mechanical exosmose. It was performed indeed in 1829, some years before this word was introduced, to denote the interesting phænomena brought to light in connexion with the diffusion of fluids, and it served, I think, to determine something regarding the relative magnitude of liquid and gaseous particles. Though it did not lie in my power at the time to subject to trial more than one *gaseous* fluid, the experiment not only went to compare the size of its particles with liquid heterogeneous ones, but with such as are homogeneous also. It appeared to establish in a manner sufficiently conclusive, a considerable difference between the magnitude of particles of the same body in its liquid and gaseous state.

Moreover, though limited to a few subjects, a wider object was attained, since it served to point to a method of entry upon a new and interesting field of inquiry.

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[*] Communicated by the Author.

The phænomena of exosmose which science has of late years made known to us, especially through the valuable experiments of Professor Graham, differ from that of the experiment under consideration in being of an elective character; the transmission varying indeed with the density of gaseous fluid, but yet taking place spontaneously, or in a manner that scarcely admits of a mechanical explanation. Whereas in the case under consideration the fluids were in a state of so great compression within the vessels, that those which passed through their pores were forced out chiefly in a mechanical way.

The material of the vessels was an aluminous composition of considerable density made into bottles of a spheroidal shape, and into these the substances experimented on were condensed.

The means for the experiments offered themselves in a manufactory of factitious mineral waters I was led to establish in the East Indies, at the suggestion of friends, by which a course of experimental pursuits, commenced from the time of my entry into that country, were diverted from a philosophical into a commercial channel. Works, however, were thereby established, as in the case of many of our manufactures here, yielding apparatus which might be employed in scientific experiments, and which for the sake of philosophy alone never would have been constructed.

Earthy minerals may be compounded in a great variety of mixtures, both as to proportion and to mechanical division, which upon being exposed to heat shall undergo various kinds of hardening and condensation; and there are many which pass into different degrees of vitrification, though few can be turned to any useful purpose, for reasons curious in their kind, but which it would be out of place to detail here.

Of the few useful vitrifiable earthy combinations, some in which silica predominates, with a lime or gypsum flux, undergo little condensation up to a certain temperature, when a partial fusion suddenly takes place, and a vitrified and sometimes beautiful texture is obtained where the heat is nicely under command. Of such compounds I had several varieties, but they are not of a nature suited to the objects of the experiment; and I refer to them only that in any repetitions of it compositions of this nature might be avoided.

Another class of vitrifiable mixtures may have a body chiefly aluminous, and one of these combined with a felspatic earth and a small proportion of iron oxide produced the ware which *yielded* the experiments. This ware underwent, from a dull red up to the time it was at a full white heat, a regular progressive contraction of its parts, by which time it had lost one fourth or one fifth in length. Commencing with pores so open that they would transmit water or gas with little resistance, it became at the heat of fused cast iron as close and dense in texture as glass; and when cooled, was equally impermeable by fluids under any pressure, exerted during any length of time.

Such a substance in its passage through every intermediate degree of density, from that at which all fluids were allowed to pass through it, to that at which they were all confined, might be expected to arrive at some one, at which it would detect any difference in the size of the molecules of two fluids pressing against it for passage, by detaining the one but transmitting the other.

The results were curious. Vessels formed of this composition thick enough to bear the pressure within, were filled with water, containing a small quantity of mineral alkali, and into which was condensed about twelve times its bulk of carbonic acid gas under a pressure of nearly eight atmospheres; their mouths being effectually secured so that no leakage whatever took place through them. The alkali varied from half a drachm to a drachm and a half of carbonate in twenty ounces. Such vessels as were in their most porous state, having been only hardened at a full red heat, allowed both the water and the gas to be ejected by the repulsive force through their pores, the liquid being projected in every direction in the form of a fine rain or cloud, until the pressure within was relieved.

Such vessels again as were in a properly effective condition, having been exposed to their maturing heat, retained their contents without any apparent loss for a year; and when placed near a fire would burst with a violent explosion, without allowing any exudation to take place.

A third kind of the same composition, apparently intermediate as to density between the two already noticed, allowed the gas alone to make its escape through the pores, rather quickly at first, but as the pressure lessened more gradually, until an equilibrium was nearly established, the whole or nearly the whole of the liquid being retained.

In a fourth kind a density was attained much above the last, but short of that of the entire impermeability of the second kind described, and this texture yielded results very curious in themselves, and especially so when contrasted with those last described. Here instead of the gas escaping, and the liquid being retained, the reverse took place. In process of time, varying from two to four months, every drop of the water was forced out, so that when the vessel was opened it was as dry within as when it came out of the furnace; yet the gas was retained highly condensed, and made its escape with a loud report.

On one occasion a violent explosion took place from a blow which I had reason to suppose would not have been sufficient to cause it, had alkaline solution been present to restrain the elasticity of the gas. In one case the outer surface of the ware was frosted over with the efflorescent alkaline carbonate, which had passed through it in a state of solution : the water was all gone, the vessel being as before perfectly dry within, yet the gas apparently all present.

The power that effected the transmission of the water and alkali being the elastic force of the gas, I could conceive it to act in no other way than mechanically, and to indicate therefore some difference in the size of the elastic and the liquid molecules; the former being too large to find passage through the pores of this closer ware, while the latter, both aqueous and alkaline, all escaped. So far the gaseous were heterogeneous to the liquid particles; and the difference of size indicated might be supposed to depend upon a difference of their original magnitude, and not of magnitude arising from their liquid and gaseous condition. But it will be observed that the alkaline *carbonate* travelled out with the water. Here therefore was carbonic acid in a liquid state, forced out through apertures which refused exit to the same matter in the state of gas. By this it would appear that the atoms of matter are indeed invested, as has been supposed, with an æthereal atmosphere elastic in itself, but forcibly drawn around the atom, so that it can find passage only where there is space for its æther to accompany it, and that this æthereal investment being much larger around gaseous than around liquid particles, apertures through which the latter may be forced may nevertheless be too small for the passage of the former. It appears to me difficult otherwise to explain the phænomena of this experiment.

Again, in that case in which the reverse took place, namely, an exit of the*gas* and a retention of the *liquid*, and which was a result most persons would expect, inasmuch as it is commonly more easy to make vessels water-tight than air-tight, a sufficient explanation may be given which shall not be incompatible with the case just recorded, and which may remove the apparent incongruity of the two experiments. In this case, though the pores were free enough to give passage both to the water and to the gas, the density was still sufficient to detain the water during the time the gas kept extricating itself, and constantly occupying the pores of the ware to the exclusion from them of the water. Though the chief force, the mechanical one, acted upon both fluids, a little might turn the scale in favour of the relief taking place by a departure of the one and not of the other. Now quite enough to decide the point in favour of the exit of the gas might be found in the well-established diffusive tendency of gases shown in the phænomena of electric exosmose, by which the gas on the one hand would seek a departure into the atmosphere through the ware, while on the other hand the cohesion of the liquid particles would favour their remaining together within it. This process went on until the gas was nearly all discharged, the small remaining quantity being detained by the affinity of the alkali and the water.

In the instance of great porosity first noticed, the results explain themselves at once. Here there was not closeness of texture sufficient for even such a detention of the contents as would enable the gas and liquid to part, and the process of exosmose to remove the former; but both fluids were propelled through the pores, the water in fine rain which formed into a mist.

Atmospheric air was not tried with water, on account of the difficulty of retaining it condensed into water while the vessel is being secured; though with apparatus devised for the purpose the experiment might easily be made, if ware of the peculiar closeness were procurable. The impregnation of water with hydro-sulphurous acid gas, which was being conducted on a large scale at the time, would have enabled me to try it, had the machinery constructed for that purpose been capable of exercising a pressure of more than two atmospheres, which was not sufficient for the effect.

Of the compositions of which the various kinds of stoneware and china are manufactured in England, there are probably some which by a certain heat would be brought to the points of density suited for the experiments. Specimens of Staffordshire earthen and China ware, however, (all of which are absorbent,) upon being treated with greater heat than those at which they had been manufactured, did not pass into any state of great density, but after a certain approximation of their parts, a decomposition of them commenced, attended with a swelling or enlargement of the whole, and an increase of porosity. I also doubt if the body used for the Derby porcelain would pass at any heat into the suitable texture; and from experiments made on oriental china from Canton, I do not think it could be conducted to the requisite point of closeness without an opposite result taking place to the opening of the pores of Staffordshire ware by intense heat, namely, a closing up of all pores by a vitrification of the body; the Chinese ware being evidently very siliceous, and corresponding in its transitions with mixtures I have already referred to.

There is a brown stoneware manufactured in a large pottery at Belper in Derbyshire, which, from certain qualities, I should judge to be formed of a body likely to yield, at different degrees of heat, the several approximations of parts suited for these experiments, provided it were protected from a glazing of the surface, which would cause an imperfect result, though no glazing is impermeable by fluids under pressure. And it is proper to remark, that although I have described only four degrees of density, they were selected out of several yielding intermediate results; which would be requisite before the heat for the experiments were obtained. These remarks may not appear irrelevant, since they may serve as some guide to a person desirous of repeating or extending the experiments.

The familiar fact that water may be retained in a porous earthen pitcher for a length of time without any escaping excepting by evaporation, although the lower part is under the pressure of a foot or more of the fluid, while such a pressure would suffice to force air quickly through the pores, would commonly be explained by supposing a greater minuteness of parts in the case of air than of water; but the above experiments would prove the contrary.

The right explanation is to be found in the case of the liquid, in the cohesive attraction between the particles which indispose the liquid to that breaking up into minute portions requisite before it can enter the pores, and where it has entered the pores in the capillary adhesion to the substance of the ware by which the liquid refuses to leave its outer surface and run down; while in the case of the gaseous fluid a mutual repulsion favours the separation of its particles, and there is no considerable attraction between them and the earthy surfaces to resist their passage out; the greater subtilty of the air over the water being due to these causes and not to its being composed of minuter particles. In some measure related to this subject I had to notice some interesting facts connected with the transmission of saline liquids through woody fibre, but the space of this paper does not admit of their being detailed here.

JULIUS JEFFREYS.

III. *On Photometry in connexion with Physical Optics.* *By* R. Potter, *Esq., B.A., F.C.P.S., Medical Fellow of Queen's College, Cambridge*[*].

THAT Photometry should have excited some attention amongst scientific men, could scarcely fail of proving a cause of satisfaction to the writer of the present paper, who had several years ago urged its great importance in laying an experimental foundation in Physical Optics. If the subject had been approached in a philosophical spirit, with a real desire to find out the truth according to the principles laid down by Lord Bacon, that satisfaction must have been the writer's privilege. Very different however has been the notice it has called forth.

The dazzling fruits of Fresnel's genius had so blinded the scientific world, that it has been held to be presumption to examine minutely the accuracy of his results, although his constitution of mind must have indicated the need of this, to the most cursory reader of his various papers. We there see the zeal of the advocate to carry his point, overruling his judgement, and leading him to make objections against the theory he opposed, which were frequently either incorrect in their foundation, or were of trivial importance.

It matters little to say such and such things are in themselves improbable or unintelligible; for if the theory give results in accordance with natural phænomena, we are bound to receive it, although many of its consequences should seem strange and even inadmissible at the first view: thus, I conceive, no one would be justified in rejecting the undulatory theory of light, or any other, if its results were in accordance with experiments.

The fashion of pinning their faith on Fresnel's sleeve having become general amongst the influential in learned societies, and amongst the most eminent in mathematical attainments, it was natural that minds of smaller calibre, or reputations of smaller weight, should feel it a readier and surer way to honours and distinction to follow in the tide, without venturing to dispute the judgement of their superiors or to question their infallibility. Under such circumstances the mere analyst must find himself floating in the tide with those who claimed a more profound knowledge of experimental philosophy, and who were sure to applaud the aid he rendered to the common cause in symbols and formulæ, of which they were frequently as little able as willing to investigate the accuracy.

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[*] Communicated by the Author.

It is thus that almost any analysis, which professed to support the undulatory theory of light, has been hailed as a magnificent achievement without its minute bearings being ever looked into, provided the general case showed a *primâ facie* accordance with some known facts. In no place has this been more prominent than in the Cambridge Philosophical Society.

He is a young man and inexperienced who has so little knowledge of human nature as to suppose that in this state of matters any scientific truth, of however important a bearing, which did not fall in with the popular and fashionable prepossessions, must be decried, and its discoverer would naturally be held out as a factious and refractory pretender in the republic of science.

The scientific public must sooner or later awaken and perceive the liberty which has been taken with its confidence, and there must sooner or later arise a time of return to sound philosophizing in physical optics, when the maxims of Lord Bacon will be acknowledged as the only sure guide.

My objections to Fresnel's formulæ for the intensity of light reflected and transmitted by transparent bodies, although founded on laborious and careful experimental researches, have been treated as though other men's guesses were more worth than my experiments. I shall, however, before I close this paper, bring corroborative evidence of the accuracy of my results which fortunately exists in print amongst the labours of Bouguer and of Sir William Herschel. Dr. Faraday also has given some photometric measures in his Bakerian lecture on the manufacture of glass for optical purposes. If the refractive indices of the heavy optical glass had been given, we should have had a good test of Fresnel's formulæ from the experiments with those glasses. The angle of incidence which Dr. Faraday employed does not furnish an *experimentum crucis* for common glass.

The subject of photometry has been discussed by Professor Lloyd in his report on Physical Optics, read before the 'British Association for the Advancement of Science;' by Professor Powell, in a paper read before the Newcastle meeting of the Association, and lately, by Professor Forbes, in a paper read before the Royal Society of Edinburgh, of which an abstract has been printed under the title "Memorandum on the Intensity of reflected Light and Heat. By Professor Forbes. (From the Proceedings of the Royal Society of Edinburgh, March 18, 1839)."*

As Professors Lloyd and Powell did not think it necessary to make themselves acquainted with the subject they under-

[* Inserted in L. and E. Phil. Mag. for December 1839, vol. xv. p. 479.]

took to discuss, their observations do not call for any further notice in this place. With Professor Forbes, however, I have a much graver controversy.

In regard to Professor Forbes's 'Memorandum,' we must strive to find his meaning in some curious passages. In speaking of Fresnel's formulæ for the intensity of reflected light, he says "scarcely any attempt has been made towards its verification by direct experiment." Before we assent or dissent from the Professor's assertion, we should know what he means by *an attempt at verification.* Attempts, and successful ones too, have been made to find the quantity of light reflected at various incidences by transparent bodies, by the celebrated Bouguer*, a century ago, and by myself latterly†, in a series of experiments which occupied me several months.

As Fresnel's formula will not give either Bouguer's quantities or mine, our labours must be very unsatisfactory, having done nothing towards that desirable verification. Professor Forbes's paper will probably supply this desideratum. A little further on he says, "It occurred to me, about the end of 1837, that the anomalies of photometrical observations being nearly as unsatisfactory as ever, some light might be thrown upon this important subject by ascertaining the law in the case of heat, the intensity of which we have no difficulty in measuring."

With respect to his actual experiments he says, "I have this winter resumed the subject. I have had an apparatus constructed for securing sufficient accuracy in determining the angle of incidence, and I have used reflecting surfaces, both transparent and metallic; the former are wedges of plate-glass, by means of which reflection from the first surface only may be observed, and the latter are plane specula of steel and silver. The prosecution, however, of these apparently simple experiments, has been attended with unforeseen difficulties; and although the relative proportions of heat at different angles of incidence are now pretty well determined for glass in several cases, I am not prepared to say whether the absolute amount is exactly the same as Fresnel's formula would give, assigning to heat its proper refractive index. It is satisfactory, however, to know, that the approximation to it is much greater than direct photometrical measures have yet given, with the single exception of two experiments of M. Arago already referred to; and that I have reason to believe that the experimental law which Mr. Potter has given from direct observation in the case of light, represents my results much less accurately than the theory of Fresnel." This undulatory passage, with its ambiguous sign prefixed in the "unforeseen difficulties," shows that the methods are not likely to furnish results accurate enough for testing important laws of nature. It is not to be wondered at that the Professor, in much variation of results, should find some which were '*satisfactory*.' As a practical answer I here give some comparisons between my own results and those of others. The most striking and most essential difference between Fresnel's theoretical formula and my experimental one is, that according to the former the whole incident light should be reflected at 90° incidence, or rather, that this is the limit to which the intensity of reflection tends at very high incidences; my formula gives only a quantity varying from about 72 to 76 rays reflected out of every 100, as this limit for the ordinary kinds of crown plate and flint glass. Hence we have a difference which cannot be hidden in errors of experiment. This was one of Bouguer's particular points. In speaking of the light reflected by water, he says (p. 135), "dans l'incidence pour ainsi dire infiniment petite la lumière réfléchie est les trois quarts de l'incidente ou de la directe." We must note here that he measured his angle of incidence from the surface and not from the normal, and that a part only, instead of the whole, was reflected at the highest possible incidences. Below I give Dr. Faraday's results with crown, plate, and flint glass, at 45° incidence (the only angle he used) with my own at 40° and 50°, from the before-named papers, as well as some calculated results.

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* See Bouguer's Traité d'Optique or Priestley's History of Vision.

† See Edinburgh Journal of Science for 1830.

From Dr. Faraday's results.

Crown glass, No. 3, reflected $\frac{1}{24{\cdot}2}$ = 4·13 of every 100 rays.
" 7, " $\frac{1}{23{\cdot}3}$ = 4·29 "
" 6, " $\frac{1}{22{\cdot}1}$ = 4·52 "
Plate glass, No. 2, " $\frac{1}{26}$ = 3·85 "
" 8, " $\frac{1}{25{\cdot}8}$ = 3·88 "
Flint glass, No. 9, " $\frac{1}{19{\cdot}5}$ = 5·13 "

From my own results.

Incidence.	Crown glass, reflects. of every 100.	Plate.	Flint.
40°	4·94	4·78	5·29
50°	5·68	5·92	6·73

Calculations by my own formula:

Incidence.	Crown.	Plate.	Flint.
40°	4·767	4·778	5·320
45°	5·205	5·243	5·884
50°	5·810	5·882	6·656

Calculations from Fresnel's formula from Sir David Brewster's paper, Phil. Trans. 1830:

Incidence.	Crown glass, reflects. of every 100 rays.
40°	4·910
45°	5·366
50°	6·136

It appears from the above that Dr. Faraday's experiments, making every allowance for different kinds of glass being used in the different experiments, do not yield that desirable verification which Professor Forbes found amongst his results. However it will be seen that we are not to seek for *experimenta crucis* at the above angles of incidence, or at any rate, with such low refracting substances. Dr. Faraday's experiments with the heavy optical glasses would have furnished them if we had had the refractive indices of those glasses; for in high refracting bodies the discordance of Fresnel's formula with experiments is palpable, for it gives results frequently one-half more, to twice as much as experiment.

In the Professor's next sentence I have to complain of a want of candour. My first introduction to physical optics was the discovery by experiments, of the law of reflection by metals, of a nature quite different from the suppositions of the scientific world, as well as of myself previously. Sir Isaac Newton had stated his opinion, such as had been received ever since; Bouguer most likely had it presented to him in his experiments with mercury, but blinded by the commonly received theory, could not see it: for he complains, that after all his precautions the fine dust fell so quickly on his mercury as to hinder the reflection at the higher incidences. Sir William Herschel had examined his splendid specula by photometry without falling upon it. Is it to be wondered at, that I should feel greatly proud of this discovery, my first effort in physical optics?

Professor Forbes says, "With respect to reflection at the metals, I believe I may assert that I have verified the remark of Mr. Potter, that metallic reflection is *less* intense at the higher angles of incidence. I have attempted to ascertain whether it reaches a minimum, and then increases up to 90° of incidence, as Mr. Maccullagh supposes; but I have not obtained decisive results. The quantity of heat reflected by the metals is so much greater than Mr. Potter's estimate for light, as to lead me to suspect that his photometric ratios are all too small, which would nearly account for their deviation from Fresnel's law."

Here my discovery comes to be merely a "remark." I think it should pass for a discovery amongst Professor Forbes's remarks.

The comparison of the former and the latter part of this sentence is a curiosity. In the first part he considers that he has experimentally verified my law, that metals reflect less light at higher incidences, and in the second he finds reason to suspect that my 'ratios' are all too small, which would nearly account for their deviation from Fresnel's law. Now, by Fresnel's formula more light would be reflected at higher incidences. How are we to understand the Professor's meaning of the word *verification*?

I consider Bouguer's observation that he found the dust fall so fast on his mercury as to hinder the reflexion at higher incidences, to be more proof of the correctness of my law, than Professor Forbes's experiments with steel and silver mirrors. It is well known that silver will not polish so as to make telescopic mirrors, and steel is very difficult to polish for that purpose. If the Professor's mirrors are the labours of the cutler and silversmith, I would not value the results they give at a straw, however skilfully and carefully made. With opticians, the most difficult thing they attempt, is to obtain a perfectly flat surface, and so essential in my experiments did I esteem it, that I used my mirrors as the oval mirror in a Newtonian telescope, and proved their accuracy by the distinctness with which double stars were exhibited in it. I stated with respect to the steel mirror, of the figure of which I was less certain than of the others, as follows: "As I had only ground and polished it in the common way for flat surfaces, I was not certain that it might be truly plane, and thought necessary to prove it on some astronomical objects. Accordingly, on the 19th February, with it and a 5½ inch speculum of my own workmanship, of about 50 inches focal length, and with a power of 100, I saw α geminorum beautifully and distinctly defined; and with a power of 150 saw γ leonis to be double at the first view, which I think will be allowed to be a sufficient test of its surface being nearly plane."

Professor Forbes will perceive that my conception of the mode to be pursued in order to discover laws of nature was a good deal different from his own.

With respect to the quantity of light reflected by metals being greater than I had found it, I have need only to give Bouguer's results, and that of Sir William Herschel, side by side with my own, and then to leave the scientific world to judge of the weight to be allowed to the Professor's experiments. Bouguer, in a preliminary discussion, says he found 754 rays to be reflected by mercury of every 1000 at 11½° inclination to the surface, and in another experiment he found 703 reflected. When afterwards he is speaking more particularly on the subject, he says he found at 21° degrees inclination in two experiments 637 and 666 reflected of every 1000. Sir W. Herschel (Phil. Trans. for 1800.) found that his specula reflected 67262 of every 100 thousand rays at an incidence nearly perpendicular.

In my paper on the reflective powers of metals, published in the Edinburgh Journal of Science for 1830, one speculum gave 69·45 rays reflected of every 100 incident at 20° incidence; another set of experiments gave 68·61 at 10° incidence, and another 66·42 at the same incidence; the deterioration in the successive sets being attributed chiefly to my rubbing the speculum too severely, with the intent to prevent being deceived at the higher incidences by any film remaining on the surface.

In the beginning of his memorandum he refers to a paper by Mr. Green, B.A., Fellow of Caius College, in this University, which appears in the forthcoming volume of the Camb. Phil. Trans. which he says accords with Fresnel's formula. This is an example of my statement that any *primâ facie* case is sufficient with the advocates of the undulatory theory. Mr. Green finds that the intensity of the light reflected at the polarizing angle of a transparent substance, when the incident light is polarized in a plane perpendicular to the plane of incidence is represented by the formula

$$\frac{(\mu^2-1)^4}{4\,\mu^2\,(\mu^2+1)^2+(\mu^2-1)^4}$$

for glass and $\mu = 5\cdot 1$ this becomes $\frac{1}{40}$ very nearly, which is more than one-half the light reflected at the perpendicular incidence even taking the undulatory formulæ, which give $\frac{1}{25}$, and if such a proportion still existed no one would say that light polarized in the plane perpendicular to the plane of incidence vanishes at the polarizing angle.

To test this further I will use an experiment brought forward by Mr. Airy, in the fourth volume of the Camb. Phil. Trans. in a paper to which Mr. Green refers. It is there stated, and easily verified, that when Newton's rings are formed by placing a piece of glass on diamond, the rings disappear at the polarizing angle for the glass when examined by light polarized perpendicularly to the plane of incidence. Hence there is not light reflected from the glass surface of the thin plate of air, which could interfere with the light from the diamond surface.

Now in Newton's rings, formed between two glasses and seen by transmission, the light twice reflected causes rings which are easily visible, although one portion of the interfering light is less than $\frac{1}{600}$th part of the other by the undulatory formulæ, and about $\frac{1}{300}$th by my experiments; also, in the former experiment, the polarizing angle for glass ($56\frac{1}{3}°$ nearly) is approaching towards the polarizing angle for diamond (about 68°), therefore, the whole light reflected by the diamond would be only a small fraction of the incident light, and that by the glass, according to Mr. Green's formula, would bear a considerable ratio to it, and therefore the rings should be very distinct, in place of which none are to be seen at all; so that Mr. Green's formula neither represents the facts nor agrees with Fresnel's.

Professor Forbes also says, in the second sentence of his memorandum, "but the chief evidence for the truth of this remarkable law rests on the indirect observation of the change of the plane of polarization of an incident ray after reflexion." He here alludes to a series of valuable experiments made by Sir David Brewster, and published in the Phil. Trans. for 1830. If we look over Sir David's table at page 75, we see the results of the experiments on the change of the plane of polarization after reflexion by diamond, affording errors of observation, although, from the nature of the experiment necessarily large, (averaging $46\frac{1}{2}'$) yet all lying on one side of the calculated results. When the theory is true we naturally expect the errors of observation to fall sometimes on one side and sometimes on the other side of the calculated place. Hence, it is clear, that the undulatory theory, even in this vaunted case, does not give the accurate results.

Queen's College.

IV. *Observations on the Blood Corpuscles or Red Disks of the Mammiferous Animals. By* GEORGE GULLIVER, *F.R.S., Assistant-Surgeon to the Royal Regiment of Horse Guards*[*].

A COMPLETE history of the blood corpuscles of the mammalia would be a very acceptable addition to anatomical science, and probably no less advantageous to zoology. To accomplish this work, however, the co-operation of so many circumstances is necessary, that the contributions of different observers are especially desirable. I have long been engaged in the inquiry; and, as I have been favoured with the liberal permission of the Council of the Zoological Society[†] to avail myself of the advantages of their collection, I propose to give an account of the blood corpuscles of such animals as I have had an opportunity of examining. Of many of these particles I believe no description has hitherto been published; and some others which have been previously described I have examined anew with a view to more accurate comparisons. I am not aware that the peculiar form of the corpuscles in certain species of the genus *Cervus* has been hitherto observed in any class of animals.

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[*] Communicated by the Author, Nov. 23, 1839.

[†] In returning my best thanks on this occasion to the Council, I cannot

The general results appear interesting to me in many points of view, particularly in respect to certain differences in the size of the corpuscles in the same individual at different periods of existence, as well as in nearly allied species of the same genus; and the resemblance, on the other hand, of the blood disks in some groups of the animal kingdom. The connexion, too, between the size and form of the blood particles and the respiratory organs, I have found to be remarkable. But before entering into conclusions it will be necessary to give the observations in detail, reserving a summary arrangement of them, and the deductions which they appear to warrant, for a future section of this memoir, when an opportunity will be afforded of reference to the labours of others in this field.

The instrument made use of in these observations is a compound microscope, with an achromatic object-glass of one-eighth of an inch focal length, made by Ross, and furnished by him with a micrometer eye-piece divided into spaces corresponding to 1-4000th of an inch. The magnifying power afforded is exactly 800 diameters with a clear definition.

If one space and a quarter of this micrometer were occupied by a single globule, this would of course measure 1-3200th of an inch; if three equally-sized particles lying in a line, and touching at their edges, covered three spaces and a half, the diameter of each of these would be 1-3429th,—if four spaces, 1-3000th of an inch. Now these measurements are mentioned because they are very frequently obtained from the average-sized human blood disks, which are to be distinguished from the remarkable varieties which Mr. Bowerbank has observed among them, and which I have also witnessed, though I think in a less degree than he mentions, in man and various animals. In the human blood corpuscle, the diameter last mentioned is not so common as the two former. In the absolute accuracy of any micrometer applied to objects so extremely minute, it is difficult to place implicit reliance; but

avoid acknowledging the kindness and urbanity of Mr. Ogilby, who entertained my application with his usual zeal in the promotion of zoological inquiries. I have also repeatedly been indebted to the kind assistance of Mr. Youatt, the excellent medical superintendent of the Gardens.

in the relative exactness of the instrument I am disposed from long experience to put much confidence, which is of the greatest importance where the results are to be obtained chiefly by comparisons.

The corpuscles were examined thinly spread on glass and quickly dried, also floating in their own serum, and diluted when necessary with weak saline solutions, or sugar and water, or with urine. The objection to these substances is, that they all more or less alter the figure of the globules, generally rendering many of them cup-shaped and diminishing their size slightly. Indeed, the disks kept in their own serum often appear a little smaller in a very short time after they have been removed from the vessels, as if they possessed some degree of contractility. It might be supposed that the particles rapidly dried on glass would shrink a little; but this is not the case, for they retain a remarkably clear and regular outline, and are commonly, to a small extent, larger than those of the same blood exposed to the air in their own fluid.

In some instances there was certainly a slight enlargement in the dried corpuscles, as compared with those seen in their own serum immediately after they were taken from the animal. In the greater number of trials, however, the sizes of the wet and dry disks corresponded accurately. In most cases the measurements were repeated by Mr. Siddall, an experienced micrographer, with another instrument by Ross, so as to avoid as much as possible accidental inaccuracies. The measurements are always expressed in fractions of an English inch.

As the corpuscles are very liable to change in size and form from very trivial causes, the extreme measurements in no case include those large or small particles which occur but sparingly, and which, perhaps, are not identical with the common red disks. Neither the large white globules nor the granulated particles are estimated, because, independently of their spherical shape, the former are almost uniformly larger and the latter smaller than the blood disks. As noticed by Hewson, the common corpuscles become mulberry-shaped when, from incipient putrefaction, their colouring matter begins to dissolve in the serum. But I have observed the granulated particles in great numbers, both in their serum and in the dry state*, in blood examined immediately after it was obtained from the veins of various animals, particularly young kittens. The nature of these particles is worthy of further and special inquiry. They are to be found plentifully during digestion; but, in their deep-red colour and chemical properties, they differ remarkably from the granules observed in the chyle.

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* I gave Mr. Owen specimens of these granules in September last, mentioning this fact to him, as he considered the granulated appearance to be the effect merely of drying.

1. Dog-faced Baboon, hardly half-grown, (*Simia Cynocephalus*). Blood from the right ventricle of the heart. Corpuscles generally 1-3192nd of an inch in diameter; extreme sizes 1-4000th and 1-2666th of an inch, the largest more frequent than the smallest.

2. Green Monkey, adult, (*Cercopithecus Sabæus*). Average-sized corpuscles 1-3200th to 1-3000th of an inch in the dry state, 1-3600th in their own serum. Extreme sizes 1-4000th and 1-2666th. Thickness of the corpuscles as seen on their edges 1-12,000th to 1-10,000th of an inch, i. e. a rouleau of five or six filled two of the micrometer spaces. The blood examined was obtained from both ventricles of the heart, from the vena cava, and vena portæ, no appreciable difference appearing in the corpuscles.

3. White Monkey, adult, (*Macacus radiatus*, albino var.). Corpuscles from the right ventricle generally 1-3200th of an inch in diameter.

4. Rhesus Monkey, old male, (*Macacus Rhesus*). Size of disks very variable from 1-4572nd to 1-2666th of an inch: most common diameter 1·3200th. Blood of both ventricles examined.

a. In a young male the same measurements were obtained.

5. Capuchin Monkey, adult, (*Cebus Capucinus*). Most common diameter of corpuscles 1-3428th and 1-3200th of an inch dried; in their serum, exposed to the air some hours, 1-4000th was a more frequent diameter. Extreme diameters 1-4572nd and 1-2910th of an inch. Blood from both ventricles of the heart examined.

6. Mangabey Monkey, adult male, (*Cercocebus Æthiops*). Common sizes 1-3200th and 1-3000th. Extreme sizes, also common, 1-4000th and 1-2666th. Blood examined from both ventricles of the heart, from the portal and renal veins.

7. Grivet Monkey, (*Cercocebus griseo-viridis*). Most frequent sizes 1-3200th and 1-3000th. Extreme diameters 1-4000th and 1-2666th of an inch. Blood from both ventricles of the heart.

8. Slow Lemur, full-grown, (*Loris tardigradus*). Most common size 1-3552nd. Extreme diameters 1-4000th and 1-3000th. Blood from a prick of the fore-hand.

9. Common Bat, (*Vespertilio murinus*). Blood from right ventricle, Common diameter of corpuscles 1-4365th to 1-4000th of an inch.

10. Common Squirrel, adult male, (*Sciurus vulgaris*). Cor-

puscles from the right ventricle, dried, all about 1-4000th of an inch in diameter.

11. Water Rat, adult, (*Mus amphibius*). Blood from both ventricles, examined dry; average-sized corpuscles 1-4000th and 1-3600th of an inch diameter.

12. Common brown House Rat, a male, nearly full-grown, (*Mus decumanus*). Common size of corpuscles from right ventricle 1-4000th to 1-3554th of an inch.

13. Common Mouse, (*Mus musculus*). 1-4000th and 1-3600th common sizes. Extreme diameters 1-5333rd and 1-3000th. Blood from heart and vena cava.

a. Fœtus, about half an inch long, from the uterus of the above mouse. 1-2666th the most frequent size, and many smaller corpuscles from 1-5333rd to 1-4000th, the latter being numerous. Many of the corpuscles more globular than usual, that is, more like spheres than disks,—a common appearance in the fœtal blood of mammals at an early period.

From this and several other observations, the comparatively large size of the blood corpuscles in the fœtus becomes manifest. The fact is important, because it is connected with the comparative condition of the respiratory organs, and, moreover, illustrates the difference between the blood of the parent animal and that of the fœtus; so that if there be any communication between the vessels of the placenta and uterus, the blood of the mother and fœtus is at all events different. I have examined the fœtal blood of the human subject, obtained from the heart, placenta, and umbilical vessels, with the same result as above; and it appears to me to be of considerable interest*.

14. Common Guinea Pig, adult male, (*Cavia Cobaya*). Corpuscles from the right ventricle, generally measuring from 1-4000th to 1-3200th of an inch.

15. Domestic Rabbit, adult female, (*Lepus cuniculus*). Most common diameter 1-4000th; extreme sizes 1-5000th and 1-3200th of an inch.

16. Golden Agouti, adult male, (*Dasyprocta aurata*). 1-3600th the most common diameter. Extreme sizes 1-5333rd and 1-3200th. Blood from a vein of the ear.

17. Acouchi, full-grown, (*Dasyprocta acouchi*). 1-3600th a common diameter, as was also 1-4000th. Extreme sizes 1-4572nd and 1-3200th.

18. White-whiskered Paradoxure, adult, (*P. Leucomystax*). Size of corpuscles very variable; most commonly 1-4365th and 1-4000th. Extreme diameters 1-6000th to 1-3200th. Blood from a prick of the tail.

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* The accurate Hewson' observed the same fact in a fowl and in a viper. (Exp. Inquiries, part iii. p. 39.)

19. Paradoxure Gennet, adult, (*Paradoxurus typus*). Most frequent size 1-5331st. Extreme diameters 1-6600th and 1-4572nd. Size of corpuscles more regular than in the preceding. Blood from a prick of the tail.

20. Gray Ichneumon (*Herpestes griseus*), adult male. Most frequent diameter 1-4572nd of an inch. Extreme sizes 1-6000th and 1-3554th. The dried disks had more distinct central marks, or appearance of nuclei, than usual. Blood from a prick of the tail.

21. Domestic Dog, old mongrel, (*Canis familiaris*). Common diameter of corpuscles 1-4000th to 1-3200th of an inch.

a. Fox-hound Puppy, one day old, a bitch. 1-3000th and 1-2666th the most common diameters of the corpuscles. In the blood of the vena portæ they were generally of the latter size.

b. Fox-hound Puppy, twelve days old, a bitch. Most common diameter of the corpuscles 1-3000th and 1-2885th of an inch. Extreme sizes 1-4000th and 1-2666th.

c. Mongrel Puppy, four months old, a bitch. All the following diameters common, viz. 1-3693rd, 1-3554th, 1-3429th and 1-3200th.

22. Australasian Dog, aged four or five years (*Canis Australiensis*). Most common size of corpuscles 1-3200th and 1-3000th; 1-4000th and 1-2666th were seen, but rarely.

23. Fox, old bitch, (*Canis Vulpes*). The following sized corpuscles were frequent, viz. 1-4572nd, 1-4365th, 1-4000th, and 1-3653rd; the third diameter (1-4000th) was the most common. Besides blood from the heart, corpuscles were examined from the cava and splenic veins.

24. Red American Fox, half-grown dog, (*Canis Vulpes*, var. *Americanus*). Common-sized corpuscles 1-4000th and 1-3693rd of an inch in diameter. A few were observed of 1-5333rd; and 1-3200th was rather a frequent diameter.

25. Black American Fox, a dog, about two-thirds grown, (*Canis argentatus*). 1-4000th of an inch most common diameter of corpuscles: extreme sizes 1-5333rd to 1-2666th. (This animal and the succeeding one came to England together from Hudson's Bay.)

26. White Arctic Fox, about two-thirds grown, (*Canis lagopus*). Same sizes as in the preceding.

The measurements in the genus Canis were all made from dried corpuscles obtained from the right ventricle of the heart.

27. Domestic Cat, adult female, (*Felis Catus*). Blood from right ventricle. Most common diameter of disks 1-4000th of an inch; a few were 1-4572nd.

a. A Kitten one day old, female. The common size of disks was from 1-4000th to 1-3564th, and 1-3000th to 1-2666th were frequent sizes. Corpuscles in the blood of vena portæ and right ventricle of the heart apparently identical.

b. A Kitten sixteen days old. The most frequent size of corpuscles 1-4000th. From Mr. Siddall's observation.

28. Serval, adult female, (*Felis Serval*). 1-4000th of an inch the most common diameter of the corpuscles. They were very irregular in diameter, 1-5000th and 1-3000th being frequent sizes. Blood from all the chambers of the heart examined, as well as from the renal vein.

29. Asiatic Leopard, adult male, (*Felis Leopardus*). Disks very variable in size; 1-4800th the common diameter, many varying from 1-5333rd to 1-3200th. The blood was obtained from the ventricles of the heart, from the renal vein and artery, and from the splenic vein, no difference being apparent in the corpuscles.

30. Lynx, nearly full-grown, male, (*Felis Caracal*). Most common diameter of corpuscles 1-4800th to 1-4365th of an inch. Very variable in size, extending from 1-6000th to 1-4000th. Blood from both ventricles examined.

31. Norway Lynx, about two years old, (*Felis Lynx*). Most common sizes 1-4365th to 1-4000th. Extreme diameters 1-5333rd to 1-3554th. Blood from both ventricles.

32. Coati Mondi, old male, (*Nasua fusca*). 1-3200th the most frequent diameter. Extreme sizes 1-4572nd and 1-2666th: the former extreme most common.

33. Red Coati Mondi, adult female, (*Nasua rufa*). 1-4000th and 1-3554th the most common diameters of the corpuscles. Extreme sizes 1-5333rd and 1-3200th of an inch. Blood from both ventricles and from the abdominal aorta, in this and the preceding species.

34. Horse, gelding, aged three years, and another aged five (*Equus Caballus*). Blood from jugular vein. The average-sized corpuscles in both 1-4800th and 1-4572nd of an inch.

a. Gelding, twenty-six years old. Same size generally, but more irregular, many being seen as large as 1-4000th of an inch. In this and the preceding, when collected into rouleaux, the edges of the corpuscles were from 1-12,000th to 1-14,000th of an inch, that is, a rouleau of six or seven disks occupied two spaces of the micrometer.

35. Ass, old female, (*Equus Asinus*). Most common diameter of corpuscles 1-4000th of an inch. Blood obtained from the jugular vein.

36. Dromedary, full-grown male, (*Camelus Dromedarius*). Disks oval. Long diameter: the most frequent was 1-3200th of an inch, and the following were seen, viz. 1-4000th,

1-3000th and 1-2666th. Short diameter, 1-6000th, the most frequent, and the following were seen, viz. 1-6600th, 1-6400th, and 1-5333rd, and 1-4800th. Thickness of the edges of the disks 1-16,000th to 1-12,000th of an inch. A very few circular corpuscles, about 1-6400th of an inch in diameter, were observed. Blood obtained from a prick in the skin.

37. Vicugna, adult male, (*Auchenia Vicugna*). Disks oval. Long diameter most frequently 1-4000th of an inch; and the following long diameters were observed: 1-5333rd, 1-3200th, 1-3000th. Short diameter; the most frequent 1-6400th; also many varying from 1-8000th to 1-5333rd of an inch. A few circular corpuscles observed, as in the Dromedary. Blood obtained from skin of the leg.

38. Paco, male, nearly full-grown, (*Auchenia Paco*). Disks oval. Long diameter generally 1-3200th of an inch; some 1-4000th, others 1-2666th of an inch. Short diameter, most commonly 1-6400th and 1-6000th; some of 1-8000th, and 1-5333rd. A few circular particles observed, as in the Dromedary and Vicugna. Blood obtained by pricking the ear.

39. Guanaco or Wild Lama, adult female, (*Auchenia Glama*). Blood obtained from the ear. Disks oval, and not differing appreciably from those of the Paco.

Thus in the Vicugna, Paco, and Wild Lama, the blood corpuscles are elliptical*, as the interesting discovery of M. Mandl has shown them to be in the Dromedary. In the Vicugna the disks are rather smaller than in the Dromedary. They all appear to be very thin or flat in relation to their size, their edges in the Dromedary being not more than 1-16,000th or 1-20,000th of an inch thick. I could see no indication whatever of a nucleus in any of the corpuscles; and those of the Dromedary were subjected to the action of water, acetic acid, and various reagents, with a view to determine this point.

40. Goat, adult female, (*Capra Hircus*). Common diameters of corpuscles 1-6400th and 1-6665th of an inch; the following sized globules are also very frequent, viz. 1-7108th, 1-5333rd, and 1-6000th. Blood from a wound in the neck.

41. Cashmere Goat, old male, (*Capra Hircus*, var.). Average-sized corpuscles 1-7200th to 1-5858th. The corpuscles very irregular in size; all the following sizes were very frequent, viz. 1-8000th, 1-6400th, 1-6000th; there were also a few disks 1-5333rd of an inch in diameter. Blood from the vena cava and from the renal artery.

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* As announced in the Phil. Mag. for December 1839, and in the Dublin Medical Press, No. 47. Some observations on the blood and pus of these animals were read at the Royal Med. Chir. Society, Nov. 26, 1839.

42. Common sheep, adult, (*Ovis Aries*). Corpuscles remarkably irregular in size, most commonly 1-6000th and 1-5333rd of an inch. All the following measurements taken from corpuscles which were abundant: 1-8000th, 1-5142nd, 1-4800th, 1-4221st. Blood from right ventricle, and from vena portæ.

43. Bearded Sheep, an old male, (*Ovis tragelaphus*). The most common diameter of corpuscles 1-6000th and 1-5333rd of an inch; extreme sizes, 1-6400th and 1-4000th, the former much more frequent than the latter.

44. Philantomb Antelope, adult male, (*Antilope Philantomba*). Average-sized corpuscles 1-6000th and 1-5333rd of an inch. A large number 1-4365th. Extreme diameters, 1-6400th and 1-4200th. Blood from a prick of the ear; disks examined dry, in their own serum, and diluted with urine. A very few distinct oval or spear-shaped corpuscles were seen, both in the wet and dry specimens.

45. Gazelle Antelope (*Antilope Dorcas*). Most common sizes 1-5333rd and 1-4800th. Extreme diameters 1-6000th and 1-4000th. Thickness of the edges of disks 1-16,000th of an inch. Blood from a vein of the ear.

46. Calf at the seventh month of utero-gestation, (*Bos Taurus*). Most of the corpuscles 1-4000th and 1-3428th of an inch. Extreme diameters 1-6000th and 1-3200th. Blood from jugular, umbilical, and portal veins.

a. Cow, adult, giving milk. Size very variable, from 1-5333rd to 1-3555th. Average size, 1-4268th and 1-4000th of an inch diameter. Blood from jugular vein.

b. Brahmin Cow, adult, (*Bos Taurus*, var.). 1-4572nd and 1-4800th very common sizes. Extreme diameters 1-6000th and 1-3557th.

47. Fallow Deer, full-grown buck, (*Cervus Dama*). Average-sized corpuscles 1-4572nd of an inch. Extreme measurements 1-6000th and 1-3200th. Several corpuscles 1-5333rd. Blood from jugular vein.

a. A buck Fawn, four or five days old. Average-sized corpuscles 1-4365th of an inch. Extreme diameters 1-4000th and 1-5333rd. Blood from jugular vein.

48. Mexican Deer, adult female, (*Cervus Mexicanus*). *Circular corpuscles* generally 1-6000th of an inch in diameter; *oblong corpuscles*, most frequently from 1-3200th to 1-2400th long, and from 1-12,000th to 1-8000th broad, at their gibbous part.

The blood of this animal contains a large quantity of lunated or crescentic corpuscles, besides many of the ordinary circular form. The singular lunated corpuscles are remarkably distinct and characteristic, and I believe unlike any hitherto described in the animal kingdom. They are generally acutely pointed at the ends and gibbous in the middle; sometimes they are not curved, and then, to use a botanical term, they present a lanceolate figure. A more particular description of them, with drawings, is in preparation.

49. Napu Musk Deer, adult female, (*Tragulus Javanicus**). Average size of disks 1-12,000th of an inch. Extreme diameters 1-15,000th and 1-9600th. Blood examined from a prick of the ear, also from a small vein of the leg.

Hence the blood disks of this beautiful little animal are smaller than those, hitherto described, of any other mammal. They were remarkably distinct, and very well seen in dry specimens, also diluted with serum or urine.

50. Pig, about half-grown, (*Sus Scrofa*). Common diameters 1-4000th and 1-3728th. Extreme sizes 1-5333rd and 1-3200th, and the corpuscles generally very variable. The blood examined was arterial and venous mixed.

51. Elephant, male, apparently an adult, (*Elephas Indicus*). Most frequent diameters of disks 1-3000th, 1-2666th, and 1-2462nd of an inch. Other sizes, 1-3329th, 1-2910th, 1-2823rd, and 1-2286th. Blood obtained from a vein in the ear.

Consequently the blood corpuscles of this animal are larger than any hitherto described in the mammalia, being considerably larger than in man. But a reference to the comparative magnitude of the blood disks of the horse and the bat, and some species of *Mus*, will show how little relation there is between the size of the animal and the diameter of the red particles. In the mouse for instance they are larger than in the horse.

52. Rhinoceros, full-grown male, (*Rhinoceros Indicus*). Common size of disks 1-4000th and 1-2554th. Many were observed with diameters of 1-4572nd and 1-3200th of an inch, with of course every intermediate gradation of size. Blood from a prick in the nose.

53. Common seal (*Phoca vitulina*), 1-3200th the most frequent diameter. Extreme sizes 1-3554th and 1-2666th. The disks very regular in size. Blood from prick of caudal fin.

The particles are slightly larger than in the adult human subject, and approach in appearance to corpuscles of the monkey's blood.

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* See Phil. Mag. for Dec. 1839, and Dublin Medical Press, No. 47. *Moschus Javanicus* of Pallas. Since my observation of the singular blood-disks of this animal was published, Professor Owen has given an account of them in another species (*M. pygmæus*), in which they seem to be equally small, as he remarks he had anticipated.

54. Common Otter, full-grown, (*Lutra vulgaris*). Most common sizes 1-3600th and 1-3200th. Extreme diameter, 1-4572nd and 1-2910th of an inch. Blood from right ventricle, and from renal artery and vein.

55. Kangaroo, adult male, (*Macropus Bennettii*). Most common diameters 1-3600th and 1-3432nd of an inch; and the following sizes were rather frequent: 1-4000th, 1-3200th, and 1-3000th. Blood from a prick at the end of the tail.

56. Flying Opossum, adult female, (*Petaurus Sciurus*). Average-sized disks 1-3600th. Extreme diameters 1-4800th and 1-3000th of an inch. Blood from a prick in the nose.

57. Ursine Opossum, adult male, (*Dasyurus Ursinus*). Most frequent sizes 1-3428th and 1-3600th. Extreme diameters 1-4365th and 1-3000th. Thickness of the edges of the disks from 1-10,000th to 1-12,000th of an inch. Blood from a vein in the ear.

58. Another species, (*Dasyurus viverrinus*). 1-4000th a very common diameter of the disks; 1-4800th and 1-3554th were also the sizes of several of the disks. Blood from the left ventricle.

59. The Perameles, (*Perameles lagotis*). Common sizes 1-4572nd and 1-4000th of an inch. Several disks were seen of the following diameters: 1-4800th, 1-3428th, and 1-3200th. Blood from a prick in the ear.

Thus the blood disks of these five Australasian animals agree in form and size with the red particles most common in other mammals.

Since the foregoing paper was printed I have examined the blood of some other mammiferous animals. I subjoin the notes in the order they happen to have been made. All the observations will soon be systematically arranged, so as to exhibit in some measure the relation between the blood corpuscles and the organization of the animal. The blood particles of the different species of the genus Cervus, Antilope, and the congeners of the Napu Musk Deer, appear to me to be especially deserving of further observations; and it is very probable that any physiologist who may prosecute the inquiry will be rewarded with some interesting results.

60. White-fronted Lemur, adult male, (*Lemur albifrons*). Size of corpuscles very variable, from 1-4800th to 1-3000th. The most frequent diameter 1-3600th. Blood from both ventricles, from the splenic and portal veins, and the disks apparently identical in all.

61. Sambur Deer, adult buck, (*Cervus hippelaphus*). Dry; size very variable, 1-4000th and 1-3600th most common.

Extreme diameters, rather frequent, 1-4572nd and 1-3200th. Blood from a prick of the nose.

62. Moose Deer or Elk, adult hind, (*Cervus Alces*). Average-sized corpuscles, 1-4000th and 1-3764th. Extreme diameters 1-5333rd and 1-3200th. Blood dried, from a prick of the nose.

63. Barbary Deer, adult hind, (*Cervus Barbarus*). Dry; most frequent diameters 1-4800th. Extreme diameters 1-5333rd and 1-4365th. Blood from the upper lip.

64. Wapiti Deer, full-grown hind, (*Cervus Wapiti*). Dry; 1-4363rd most common size. Extreme diameters 1-5333rd and 1-3554th. Blood from the upper lip.

Carefully compared with the blood of the preceding, and the corpuscles generally pretty regular in size, and certainly larger than in the Barbary Deer.

65. Hog Deer, adult male, (*Cervus porcinus*, albino var.). Common size of corpuscles 1-5333rd and 1-6000th. Many oblong lunated corpuscles, corresponding in size and form to those observed in the blood of the Mexican deer.

66. Gnu, adult female, (*Antilope Gnu*). Dry; corpuscles varying much in size, 1-4800th most common. Extreme diameters 1-6000th and 1-4000th. Blood from prick of upper lip.

67. Indian Antelope, male about two-thirds grown, (*Antilope cervicapra*). Dry; most frequent diameter 1-4800th and 1-5000th. In their own serum the corpuscles were very irregular in size and shape, and certainly smaller than when dried. The extreme sizes of the dried disks 1-6000th and 1-4000th; in their serum or diluted with urine 1-8000th and 1-5000th.

68. Giraffe, adult male, (*Camelopardalis Giraffa*). Most frequent size of corpuscles 1-4572nd. Extreme diameters 1-5333rd and 1-4000th. Blood from a prick of the lip.

69. American Buffalo, full-grown female, (*Bos Bison*). 1-4000th of an inch, a very common diameter of the corpuscles. Extreme sizes 1-4572nd and 1-3554th. Blood examined dry, from the nose.

70. Weasel-headed Armadillo, adult male, (*Dasypus sexcinctus*). Most frequent size of corpuscles 1-3429th, and the following common, viz. 1-3692nd, 1-3552nd, 1-3368th, 1-3330th. Extreme diameters 1-4000th and 1-3000th. Blood from a prick of the ear.

71. Lesser American Flying Squirrel, adult, (*Pteromys volucella*). Dry; 1-3600th the most common size of disks, and 1-4000th not uncommon. Extreme diameters 1-4800th and 1-3428th. Blood from a prick of the ear.

72. Palm Squirrel, adult, (*Sciurus palmarum*). Dry; most commonly 1-3692nd, extreme sizes 1-4800th and 1-3000th. In their own serum or in urine they seemed contracted, the corpuscles often cup-shaped, bent, or shrunk, ranging from 1-4800th to 1-4000th. Blood from a prick of the tail.

73. Furnier's Capromys, adult, (*Capromys Furnieri*). Dry; common diameters 1-3530th and 1-3429th. Extreme sizes 1-4000th and 1-3000th. Blood from a prick of the ear.

74. Bandicoot Rat, adult, (*Mus giganteus*). Corpuscles in their own serum very irregular in size, most frequently 1-4000th of an inch, and many from 1-5333rd to 1-3200th. There was evidently considerable shrinking of the corpuscles while under examination, as observed in several trials. In dried specimens the disks were generally from 1-4000th to 1-3600th. Extreme sizes 1-4800th and 1-3200th. Blood from a prick of the tail.

75. Hoary Marmot or Whistler, an old animal, (*Arctomys pruinosa*). Dry; most commonly 1-3600th. Extreme sizes 1-4000th and 1-3000th. Blood from a prick of the upper lip.

Regent's Park Barracks, Nov. 22, 1839.

V. *On the Employment of Carbon in Voltaic Combinations. By* Mr. John Thomas Cooper, *Lecturer on Chemistry, &c. &c.*

To Richard Phillips, Esq., F.R.S., &c.

Dear Sir,

IT occurred to me on reflecting upon the use of the platinum as employed by Mr. Grove in the construction of his very energetic voltaic combination*, as it was only to conduct the electricity from the decomposing nitric acid, that any cheaper substance which conducted electricity, and upon which nitric acid had no action, might be employed with equal advantage, and probably supply the place of the more expensive material. With these views I was induced to make trial of *charcoal*, and the other forms of carbon, viz. plumbago, and a peculiar kind of carbon which is frequently met with as an incrustation in the retorts in which coal is decomposed for the purpose of gas lighting; and was gratified on making the experiments in finding my anticipations fully realized. In order to show the comparative value of each of the substances, I here subjoin the results of some of the experiments made with acids of the same strength, and with amalgamated zinc cylinders, each presenting to the action of the dilute sulphuric

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[* See Lond. & Edinb. Phil. Mag., Oct. 1839, vol. xv. p. 287.]

acid as nearly an equal surface as it was possible to obtain *. The figures represent the volumes of gas obtained by the voltameter in equal times.

Thick platinum foil soldered to the zinc.	Well-burned charcoal clamped to the zinc.
3·4	2·8
3·4	3·0
3·4	3·1
3·6	3·2
3·6	3·25
3·5	3·25
3·75	3·25
3·5	3·4
3·5	3·25
9)31·65	9)28·50
Mean 3·517 cub. inch.	Mean 3·17 cub. inch.

Plumbago clamped to the zinc.	Hard carbon from gas retorts clamped to the zinc.
3·2	2·6
3·2	2·8
3·3	3·3
3·2	3·4
3·2	3·4
3·6	3·45
3·6	3·5
3·7	3·6
3·5	3·4
9)30·5	9)29·45
Mean 3·4 cub. inch.	Mean 3·27 cub. inch.

From the above results it will be evident that the platinum appears to possess a trifling advantage over the other substances; this, however, I am inclined to ascribe to the more perfect contact of the platinum with the zinc by soldering than to its superior qualities as a conductor of electricity; and when the difference of expense is taken into consideration in the construction of large and extensive combinations, the application of the above-mentioned substances must be regarded as of great importance to the chemist, and to those who may have occasion to employ such combinations as electro-motors, seeing that either with the charcoal, plumbago, or hard carbon,

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* The battery consisted of four zinc cylinders, each having 6¼ inches surface, and the pipeclay cups, 1 inch in diameter and 1¼ inch long, which gave the above quantity of gas in two minutes.

the purity of the nitric acid is a matter of indifference, strong commercial acid answering every purpose.

I remain, dear Sir, very truly yours,
JOHN THOMAS COOPER.

82, Blackfriars Road, London, Dec. 10, 1839.

P.S. I have found within these few days that some kinds of common coke, such as is very brilliant, close-grained, and has a columnar fracture, is equally good with the other varieties of carbon, and admits of being selected of almost any variety of form and size. J. T. C.

VI. *On Derivation of Coexistence:* Part I. *Being the Theory of simultaneous simple homogeneous Equations.* *By* J. J. SYLVESTER, *F.R.S. & R.A.S., Professor of Natural Philosophy in University College, London*[*].

Art. (1.) WE shall have constant occasion in this paper to denote different quantities by the same letter affected with different subscribed numerical indices.

Such a letter is to be termed a "Base."

Every character consisting of a base and an inferior index I call an argument of the base, viz. the first, second, or *n*th argument, according as 1, 2, or in general (n), be the number subscribed.

Art. (2.) I use the symbol PD to denote the product of the differences of the quantities to which it is prefixed (each being to be subtracted *from* each that follows); thus

PD $(a\ b\ c)$ indicates $(b-a)\ (c-a)\ (c-b)$.
PD $(o\ a\ b\ c)$ indicates $a\ b\ c\ (b-a)\ (c-a)\ (c-b.)$
PD $(o\ a\ b\ c \ldots l)$ indicates $a\ b\ c \ldots l \times$ PD $(a\ b\ c\ \ldots\ l)$.

Art. (3.) For want of a better symbol I use the Greek letter ζ to denote that the product of factors to which it is prefixed is to be effected after a certain symbolical mannner. This I shall distinguish as the zeta-ic product.

The symbol ζ will never be prefixed except to factors, each of which is made up of one or more terms, consisting solely of linear arguments of different bases, i.e. characters bearing indices below but none above.

I am thereby enabled to give this short rule for zeta-ic multiplication: "Imagine all the inferior indices to become superior, so that each argument is transformed into a *power* of its base; multiply according to the rules of ordinary algebra; after the multiplication has been *done fully out* depress all the indices into their original position; the result is the zeta-ic product*."

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[*] Communicated by the Author. Part I. appeared in L. & E. Phil. Mag. Dec. 1839, vol. xv. p. 428.

Thus for example $\zeta(a_r . b_s)$ is the same as simply $a_r . b_s$, but $\zeta(a_r . a_s)$ represents not $a_r . a_s$ but a_{r+s}.

So in like manner

$$\zeta((a_h - b_k)(a_l - b_m))$$
$$= a_{h+l} - a_h . b_m - b_k . a_l + b_{m+k}$$
$$\zeta((a_1 - b_1)(a_1 - c_1)(b_1 - c_1))$$

= the *depressed product* of $(a-b)(a-c)(b-c)$

= the *depressed* value of $a^2(b'-c')+b^2(c'-a')+c^2(a'-b')$

i.e. $= a_2 . b_1 - a_2 . c_1 + b_2 . c_1 - b_2 . a_1 + c_2 . a_1 - c_2 . b_1$.

Art. (4.) We shall have occasion in this part to combine the two symbols ζ, PD: thus we shall use

ζ PD $(a_1\, b_1)$ to denote $\zeta(b_1 - a_1)$

ζ PD $(a_1\, b_1\, c_1)$ to denote $\zeta((b_1 - a_1)(c_1 - a_1)(c_1 - b_1))$.

Art. (5.) For the sake of elegance of diction I shall in future sometimes omit to insert the inferior index when it is unity; but the reader must always bear in mind that it is to be *understood* though not expressed.

I shall thus be able to speak of the zeta-ic product of such and such bases mentioned by name.

Art. (6.) We are not yet come to the limit of the powers of our notation. The zeta-ic product of the sum of arguments will consist of the sum of products of arguments, each argument being (as I have defined) made up of a base and an inferior index. Now we may imagine each index of every term of the zeta-ic product *after it is fully expanded* to be increased or diminished by unity, or each at the same time to be increased or diminished by 2, or each in general to be increased or diminished by *r*. I shall denote this alteration by affixing an (*r*) with the positive or negative sign to the ζ. Thus

$\zeta(a_1 - b_1)(a_1 - c_1)$ being equal to $a_2 - a_1 . c_1 + b_1 . c_1 - b_1 . a_1$

$\zeta_{+1}(a_1 - b_1)(a_1 - c_1)$ is equal to $a_3 - a_2 . c_2 + b_2 . c_2 - b_2 . a_2$

$\zeta_{-1}(a_1 - b_1)(a_1 - c_1)$ is equal to $a_1 - a_0 . c_0 + b_0 . c_0 - b_0 . a_0$.

In like manner ζ PD $(a\, b\, c)$ indicating

$$b_2 . a_1 - b_2 . c_1 + c_2 . b_1 - c_2 . a_1 + a_2 . c_1 - a_2 . b_1$$

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[*] It is scarcely necessary to add that an analogous interpretation may be extended to any zeta-ic function whatever. Thus

$$\zeta(a_1 + b_1)^2 = a_2 + 2\, a_1\, b_1 + b_2$$
$$\zeta \cos(a_1) = 1 - \frac{a_2}{1.2} + \frac{a_4}{1.2.3.4}, \text{ \&c.}$$

$\zeta_{\pm r}$ PD $(a\ b\ c)$ indicates

$$b_{2\pm r} \cdot a_{1\pm r} - b_{2\pm r} \cdot c_{1\pm r} + c_{2\pm r} \cdot b_{2\pm r} - c_{2\pm r} \cdot c_{1\pm r}$$
$$+ a_{2\pm r} \cdot c_{1\pm r} - a_{2\pm r} \cdot b_{1\pm r}.$$

I shall in general denote ζ_{+r} PD $(a\ b\ c \ldots l)$ *actually expanded* as the zeta-ic product of $a_{,} b_{,} c_{,} \ldots l$ in its rth *phase.*

Art. (7.) *General Properties of Zeta-ic Products of Differences.*

If there be made one interchange in the order of the bases to which ζ is prefixed, the zeta-ic product, in whatever phase it be taken, remains unaltered in magnitude, but changes its sign.

Art. (8.) If in any *phase* of a zeta-ic product two of the bases be made to coincide, the expansion vanishes.

Art. (9.) Let $\int_1$ be used, agreeably to the ordinary notation, to denote the sum of the quantities to which it is prefixed, $\int_2$ to denote the sum of the binary products, $\int_3$ of the ternary ones, and so on.

Thus let $\int_1 (a_{,}\ b_{,}\ c_{,})$ or $\int_1 (a\ b\ c)$ indicate $a_{,} + b_{,} + c_{,}$
and $\int_2 (a_{,}\ b_{,}\ c_{,})$ or $\int_2 (a\ b\ c)$ indicate $a_{,}\, b_{,} + a_{,} c_{,} + b_{,} c_{,}$
and $\int_3 (a_{,}\ b_{,}\ c_{,})$ or $\int_3 (a\ b\ c)$ indicate $a_{,}\ b_{,}\ c_{,}$
we shall be able now to state the following remarkable proposition connecting the several phases of certain the same zeta-ic products.

Art. (8.) Let $a, b, c, \ldots l$, denote any number of independent bases, say $(n-1)$; but let the arguments of each base be periodic, and the number of terms in each period the same for every base, namely (n), so that

$$\begin{array}{ll}
a_r = a_{r+n} = a_{r-n} & a_n = a_o = a_{-n} \\
b_r = b_{r+n} = b_{r-n} & b_n = b_o = b_{-n} \\
c_r = c_{r+n} = c_{r-n} & c_n = c_o = c_{-n} \\
\cdot \quad \cdot \quad \cdot \quad \cdot & \cdot \quad \cdot \quad \cdot \quad \cdot \\
l_r = l_{r+n} = l_{r-n} & l_n = l_o = l_{-n}
\end{array}$$

r being any number whatever. Then

$$\zeta_{-1}\,\text{PD}\,(o\ a\ b\ c \ldots l) = \zeta\Big(\int_1 (a\ b\ c \ldots l)\ .\ \zeta\,\text{PD}\,(o\ a\ b\ c \ldots l)\Big)$$
$$\zeta_{-2}\,\text{PD}\,(o\ a\ b\ c \ldots l) = \zeta\Big(\int_2 (a\ b\ c \ldots l)\ .\ \zeta\,\text{PD}\,(o\ a\ b\ c \ldots l)\Big)$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\zeta_{-r}\,\text{PD}\,(o\ a\ b\ c \ldots l) = \zeta\Big(\int_r (a\ b\ c \ldots l)\ .\ \zeta\,\text{PD}\,(o\ a\ b\ c \ldots l)\Big)$$

This proposition admits of a great generalization*, but we have now all that is requisite for enabling us to arrive at a proposition exhibiting under one *coup d'œil* every combination and every effect of every combination that can possibly be made with any number of coexisting equations of the first degree, containing any number of *repeated*, or to use the ordinary language of analysts, (variable or) unknown quantities.

Art. (9.) For the sake of symmetry I make every equation homogeneous; so that to eliminate n repeated terms, no more than n equations will be required.

In like manner the problem of determining n quantities from n equations will be here represented by the case in which we have to determine the *ratios* of $(n + 1)$ quantities from n equations.

Art. (9.) *Statement of the Equations of Coexistence.*

Let there be any number of bases $(a\ b\ c \ldots l)$, and as many repeated terms $(x\ y\ z \ldots t)$, and let the number of equations be any whatever, say (n). The system may be represented by the *type* equation

$$a_r \,.\, x + b_r \,.\, y + c_r \,.\, z + \;.\;\;.\;\;. + l_r \,.\, t = 0.$$

In which r can take up all integer values from $-\infty$ to $+\infty$. The specific number of equations given will be represented by making the arguments of each base *periodic*, so that

$$a_r = a_{\mu n+r} \quad b_r = b_{\mu n+r} \quad c_r = c_{\mu n+r} \;.\;\;.\;\;.\; l_r = l_{\mu n+r},$$

μ being any integer whatever.

Art. (10.) *Combination of the given Equations.— Leading Theorem.*

Take $f, g, \ldots k$ as the *arbitrary* bases of new and absolutely independent but periodic arguments, having the same index of periodicity (n) as $a\ b\ c \ldots l$, and being in number $(n - 1)$, i. e. one fewer than there are units in that index.

The number of *differing* arbitrary constants thus *manufactured* is $n \,.\, (n - 1)$.

Let $\mathrm{A}\,x + \mathrm{B}\,y + \mathrm{C}\,z + \;.\;.\;.\; + \mathrm{L}\,t = 0$ be the general *prime* derivative from the given equations, then we may make

$$\begin{aligned}\mathrm{A} &= \zeta\,\mathrm{PD}\,(o\,a\,f\,g \ldots k)\\ \mathrm{B} &= \zeta\,\mathrm{PD}\,(o\,b\,f\,g \ldots k)\\ \mathrm{C} &= \zeta\,\mathrm{PD}\,(o\,c\,f\,g \ldots k)\\ &\;.\quad.\quad.\quad.\quad.\\ \mathrm{L} &= \zeta\,\mathrm{PD}\,(o\,l\,f\,g \ldots k).\end{aligned}$$

Art. (11.) Cor. (1.) *Inferences from the Leading Theorem.*

Let the number of equations, or, which is the same thing, the index of periodicity (n), be the same as the number of repeated terms ($x\ y\ z \dots t$), then one relation exists between the coefficients: this is found by making the ($n-1$) new bases coincide with ($n-1$) out of the old bases. We get accordingly, as the result of elimination,

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* See the Postscript to this paper for *one* specimen.

$$\zeta \, \mathrm{PD} \, (o\ a\ b\ c \dots l) = 0.$$

Art. (11.) Cor. (2.) Let the number of equations be one more than that of the given bases, there will then be two equations of condition. These are represented by preserving one new arbitrary base, as λ. The result of elimination being in this case

$$\zeta \, \mathrm{PD} \, (o\ a\ b\ c \dots l\ \lambda) = 0.$$

Ex. The result of eliminating between

$$\begin{aligned} a_1 . x + b_1 . y &= 0 \\ a_2 . x + b_2 . y &= 0 \\ a_3 . x + b_3 . y &= 0 \end{aligned}$$

is $\zeta \, \mathrm{PD} \, (o\ a\ b\ \lambda) = 0$

i. e. $\lambda_3 . b_2\, a_1 - \lambda_3 . b_1\, a_2 + \lambda_1 . b_3\, a_2 - \lambda_1 . b_2\, a_3 + \lambda_2 . b_1\, a_3$
$- \lambda_2 . b_3\, a_1 = 0,$

from which we infer, seeing that $\lambda_3\ \lambda_2\ \lambda_1$ are independent,

$$\begin{aligned} b_2 . a_1 - b_1 . a_2 &= 0 \\ b_3 . a_2 - b_2 . a_3 &= 0 \\ b_1 . a_3 - b_3 . a_1 &= 0, \end{aligned}$$

any *two* of which imply the third.

In like manner, in general, if the number of equations exceed in any manner the number of bases or repeated terms, the rule is to introduce so many *new* and *arbitrary* bases as together with the old bases shall make up the number of equations, and then equate the zeta-ic product of the differences of zero, the old bases and the new bases, to nothing.

Art. (12.) Cor. (3). Let the number of equations be *one* fewer than the number (n) of bases or repeated terms; the number of introduced bases in the general theorem is here ($n-2$). Make these ($n-2$) bases equal severally to the bases which in the type equation are affixed to $z, u \dots t$, then

$$\begin{aligned} \mathrm{C} &= 0 \\ \mathrm{D} &= 0 \\ &\dots\dots\dots \\ &\dots\dots\dots \\ \mathrm{L} &= 0, \end{aligned}$$

and we have left simply

$$\zeta \, \mathrm{PD} \, (o\ a\ c\ d \dots k\ l)\, x + \zeta \, \mathrm{PD} \, (o\ b\ c\ d \dots k\ l)\, y = 0.$$

In like manner we may make to vanish all but A and C, and thus get

$$\zeta \, \mathrm{PD} \, (o\ a\ b\ d \dots k\ l)\, x + \zeta \, \mathrm{PD} \, (o\ c\ b\ d \dots k\ l)\, z = 0,$$

and similarly

$$\zeta\,\mathrm{PD}\,(o\,a\,b\,\ldots\,k)\,x + \zeta\,\mathrm{PD}\,(o\,b\,c\,\ldots\,l)\,t = 0.$$

$$\text{Hence}\quad \left.\begin{array}{c} x \\ y \\ z \\ \cdot \\ \cdot \\ \cdot \\ t \end{array}\right\} \text{ are severally as } \left\{\begin{array}{l} \zeta\,\mathrm{PD}\,(o\,b\,c\,\ldots\,l) \\ \zeta\,\mathrm{PD}\,(a\,o\,c\,\ldots\,l) \\ \zeta\,\mathrm{PD}\,(a\,b\,o\,\ldots\,l) \\ \cdots\cdots\cdots\cdots \\ \cdots\cdots\cdots\cdots \\ \cdots\cdots\cdots\cdots \\ \zeta\,\mathrm{PD}\,(a\,b\,c\,\ldots\,o) \end{array}\right.$$

This is the symbolical representation as a *formula* of the remarkable *method* discovered by Cramer, perfected by Bezout and demonstrated by Laplace for the solution of simultaneous simple equations.

Art. (13.) Cor. (4.) In like manner if the number of repeated terms be two greater than the number of equations, we have for the relation between any *three* of them, taken at pleasure, for instance, x, y, z,

$$\zeta\,\mathrm{PD}\,(o\,a\,d\ldots l)\,x + \zeta\,\mathrm{PD}\,(o\,b\,d\ldots l)\,y + \zeta\,\mathrm{PD}\,(o\,c\,d\ldots l)\,z = 0.$$

And in like manner we may proceed, however much in excess the number of repeated terms (unknown quantities) is over the number of equations.

Art. (14.) *Subcorollary to Corollary* (3.)

If there be any number of bases $(a\,b\,c\,\ldots\,l)$, and any other two fewer in number $(f\,g\,\ldots\,k)$

$$\left.\begin{array}{l} \quad\zeta\,\mathrm{PD}\,(a\,f\,g\,\ldots\,k) \times \zeta\,\mathrm{PD}\,(b\,c\,\ldots\,l) \\ +\,\zeta\,\mathrm{PD}\,(b\,f\,g\,\ldots\,k) \times \zeta\,\mathrm{PD}\,(a\,c\,\ldots\,l) \\ +\,\zeta\,\mathrm{PD}\,(a\,f\,g\,\ldots\,k) \times \zeta\,\mathrm{PD}\,(b\,c\,\ldots\,l) \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ \cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ +\,\zeta\,\mathrm{PD}\,(l\,f\,g\,\ldots\,k) \times \zeta\,\mathrm{PD}\,(a\,b\,c\ldots) = 0, \end{array}\right\}\ \begin{array}{l}\text{The cross is used} \\ \text{to denote \textit{ordinary}} \\ \text{algebraical multipli-} \\ \text{cation.}\end{array}$$

a formula that from its very nature suggests and *proves* a wide extension of itself.

In conclusion I feel myself bound to state that the principal substance of *corollaries* (1), (2) and (3) may be found in Garnier's *Analyse Algébrique*, in the chapter headed "Développement de la Théorie donnée par M. Laplace, &c." But I am not aware of having been anticipated either in the fertile notation which serves to express them nor in the general theorems to which it has given birth.

End of Part (2).

[The subject to be continued.]

University College, London, Dec. 9, 1839.

P.S. I shall content myself for the present with barely enunciating a theorem, one of a class destined it seems to the author to play no secondary part in the development of some of the most curious and interesting points of analysis.

Let there be $(n-1)$ bases $a, b, c \ldots l$, and let the arguments of each be "recurrents of the nth order[*]," that is to say let

$$a_\iota = \varphi\left(\cos . \frac{2\pi\iota}{n}\right) \; b_\iota = \psi\left(\cos . \frac{2\pi\iota}{n}\right) \; c_\iota = \chi\left(\cos . \frac{2\pi\iota}{n}\right)$$

$$\ldots\ldots \; l_\iota = \varpi\left(\cos . \frac{2\pi\iota}{n}\right).$$

Let R_ι denote that any symmetrical function of the rth degree is to be taken of the quantities in a parenthesis which come after it, and let ϑ indicate any function whatever. Then the zeta-ic product

$$\zeta\left(\zeta R_\iota (a\, b\, c \ldots l) \times \zeta_\varrho \vartheta \, \mathrm{PD}\, (o\, a\, b\, c \ldots l)\right)$$

is equal to the product of the *number*

$$R_\iota\left(\left(\cos\frac{2\pi}{n} + \sqrt{-1} . \sin\frac{2\pi}{n}\right) \quad \left(\cos\frac{4\pi}{n} + \sqrt{-1} . \sin\frac{4\pi}{n}\right)\right.$$

$$\left(\cos\frac{6\pi}{n} + \sqrt{-1} . \sin\frac{6\pi}{n}\right) \ldots\ldots$$

$$\left.\cos\left(\frac{(2n-1)\pi}{n} + \sqrt{-1} . \sin\frac{2(n-1)\pi}{n}\right)\right)$$

multiplied by the zeta-ic phase

$$\zeta_{\varrho-\iota} \vartheta \, \mathrm{PD}\, (o\, a\, b\, c \ldots l) \; ! \, !$$

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[*] I am indebted for this term to Professor De Morgan, whose pupil I may boast to have been. I have the sanction also of his authority, and that of another profound analyst, my colleague Mr. Graves, for the use of the arbitrary terms zeta-ic, zeta-ically. I take this opportunity of retracting the symbol S P D used in my last paper, the letter S having no meaning except for English readers. I substitute for it Q D P, where Q represents the Latin word Quadratus. On some future occasion I shall enlarge upon a new method of notation, whereby the language of analysis may be rendered much more expressive, depending essentially upon the use of similar figures inserted within one another, and containing numbers or letters, according as quantities or operations are to be denoted. This system to be carried out would require special but very simple printing types to be founded for the purpose.

In the next part of this paper an easy and *symmetrical* mode will be given of representing any polynomial either in its developable or expanded form.

VII. *On the Combinations of Carbon with Silicon and Iron and other metals, forming the different species of Cast Iron, Steel, and Malleable Iron. By* Dr. C. Schafhaeutl, *of Munich.*

[Continued from vol. xv. p. 428.]

THE chemical ingredients of the iron are easily to be ascertained; but the information thus obtained is of no value in investigating the real chemical nature of iron, and can only be used as a preliminary method which must guide or verify further proceedings.

I will only here remark, that by separating silica in the usual way by means of alkalis, there is considerable difficulty in rendering silica insoluble in water when combined with a great quantity of oxide of iron, it requiring a great length of time to drive away the last traces of water and acid from the evaporated solution; and by a quick evaporation, if the residuum is not heated almost to a red heat, the silica either dissolves in a great measure again, or goes through the filter after a most tedious filtering process. It is always a laborious process, causing much loss to separate it afterwards, when the solution contains manganese. On the contrary, if the heat is increased to so high a degree, the separated silica retains a great quantity of oxide of iron, from which it is the most readily freed by treating it, after ignition, with hot chlorohydric acid; I mention this purposely, as Baron Thenard cautions us to use, for evaporating the solution containing the silica, only a moderate heat, in order that the chlorides may not be decomposed.

In ascertaining the exact quantity of carbon, of more than half-a-dozen given directions one only is of real value, which was likewise first used by Berzelius, viz. the burning the iron in a current of oxygen gas, or mixing it with chlorate of potash and chromate of lead, and igniting it in a glass tube after the well-known practice used in the analysis of organic bodies. All other methods give, instead of pure carbon, carbon combined with silicon, or carbon combined with hydrogen, azote, and silicon.

To ascertain the quantity of azote, where the metal is in large quantity, I use Dumas's method, viz. the combustion of iron in a vacuum; but where the quantities are small, I employ the following means. I put into a tube of German glass, from four to five lines wide, and about twelve inches long, shut at one end, a few grains of the body from which I intend to separate the azote, and afterwards about six times its weight of a mixture of caustic potash and caustic barytes; the open end of the tube is then drawn out over the lamp into a short capillary tube, the tube is afterwards bent about five inches from its closed end into a siphon-like figure, the capillary tube is immersed in a test glass until it nearly touches the bottom, holding not more diluted chlorohydric acid than sufficient to fill the drawn-out leg of the syphon to the height of three inches, whilst the other end is gradually heated over a spirit-lamp. The azote forming with hydrogen ammonia is driven out in a very small stream into the acid, and when the decomposition of the iron is completed, the heat is gradually diminished, and the acid ascends in the same ratio into the end of the siphon. As soon as all the acid of the test tube is absorbed, air is streaming through the capillary tube and the acid into the siphon, thus establishing the equilibrium between the interior and exterior of the tube. The heat is afterwards again raised, till the absorbed acid is driven out again into the test glass, and after this operation all the ammonia will be found to be absorbed by the acid. The larger end of the siphon is then cut off and well washed with distilled water, and the quantity of ammonia ascertained by means of a solution of chloride of platinum, added in excess, the liquor of course being evaporated nearly to dryness in the water-bath, and treated and washed with absolute alcohol. The compound of muriate of ammonia and chloride of platinum, insoluble in alcohol, remains, from which the quantity of ammonia is calculated very easily.

The action of acids upon iron and the products afterwards are highly curious and interesting. The products formed by the action of acids upon iron depend first, on the chemical constitution of the iron itself; secondly, on the greater or less division of the mass; thirdly, on the chemical constitution of the acids; fourthly, on the greater or less concentration of the acids; fifthly, on the temperature; and sixthly, on the presence or exclusion of the atmosphere.

The acid whose action I have most studied is the hydrochloric acid, and in some respects nitric acid. I therefore confine my observations to those two only. The specific gravity of the hydrochloric acid used by me was 1·169 to 1·17. The iron was in fragments from the size of a nut to that of a lentil. For the sake of illustrating the action of hydrochloric acid upon iron, I select first a fragment of a steel bar of the highest conversion of the Dannemora iron. A drop of already melted steel adhered with a very broad base on one side. The fragment about the size of a walnut in a temperature of 48 F., was treated in an open wine-glass with chlorohydric acid. The action of the acid was very rapid, but the outside of the bar fragment was more rapidly attacked than the bright crystalline fracture of the steel. That most rapidly attacked was the before-mentioned adhering drop of steel; a light yellow powder was first separated, which partly rose to the surface and partly descended to the bottom; next a gray precipitate was generated, which increased until the acid ceased to act upon the steel. All parts of the steel fragment which showed an incipient melting and silver-white colour, retained this colour during the action of the acid; the other parts of the outside of the fragments showed a blackened granular formation, traversed by white shining needles.

After having taken the fragment from the acid, I filed the surface of the before-mentioned steel drop off, and laid open a grayish surface traversed by a venous network, white and shining, and much more difficult to be attacked by the file than the lower gray and softer parts betwixt the network. On pouring fresh acid again over it, the filed surface was soon covered with a deep black velvety crust, traversed by a silver white elevated network, somewhat similar to the veins in some marbles. The interstices between the network, after removing this black crust, were found to be filled up with small crystalline needles, and all the white places, showing an incipient fusion, which the day before had remained white, disappeared partially on the second day, leaving only a few spots like the remainder of a skin, which covered a similar composition, formed of an aggregation of needles. The faces of the cubical crystals seemed also to consist only of a silvery skin, which being corroded and eaten through by the acid, showed underneath a granulated texture. A gray precipitate was found on the bottom of the glass, distinctly intermixed with a little yellowish granulated powder.

A separated cubical piece of the same bar, treated in the same manner in a wine-glass, was not very rapidly attacked by the acid, but milky streams were observed ascending to the surface. Fresh acid was then substituted. The liquid soon became milky; a copious white precipitate fell, which two hours afterwards assumed a whitish flocky form, and only a few small black flocks seemed to be mixed with copious yellowish flocks.

In order to obtain some further information respecting this powder, which I considered as silica, I used fragments of the size of a pea of a highly-cubical crystallized steel bar, made from burnt English iron, prepared after my method; secondly, a granulated fragment of the exterior of the same bar; thirdly, a small crystallized sponge-like piece from the before-mentioned burnt bar, before its conversion into steel. I put over those specimens, in three different glasses, hydrochloric acid. No. 1. was attacked very violently: the liquid became milky, and a yellow precipitate with a very few traces of black flocks remained. During a space of nine weeks the solution had imbibed so much water from the surrounding atmosphere, that the glasses were nearly overflowing. The acid was then poured off and distilled water poured over the precipitate; the precipitate was now white and flocky, and the most copious of the three samples.

No. 2. was not so violently attacked, and did not become milky: first, fine black scales became separated, adhering to the sides of the glass very regularly; afterwards, the interstices between these black scales or spots became filled up with the before-mentioned yellow powder. The whitish powder appeared very fine, never granulated or flocky, and adhered to the sides of the glass almost entirely. The whitish precipitate of Nos. 1. and 3. fell to the bottom of the glass.

No. 3. was the most rapidly attacked by the acid, and became milky, the same as No. 1. The liquid was, after a lapse of nine weeks, rather turbid and showed the smallest quantity of black scales swimming in the liquid. A few hours after the acid had been poured over it, it became turbid and very yellow, which showed that it had combined the quickest with the oxygen of the atmosphere.

The day after the acid had been poured on No. 2. the surface only began to acquire a yellow colour. In No. 3. the black flocks seemed entirely to have disappeared, and the yellowish powder ascended in part to the surface, and the remainder fell to the bottom. The difference therefore between the granulated exterior of the same bar, and the interior crystallization, seems to consist in this, that the exterior produces less white powder in a very divided state and more carbonaceous scales, or that the granulated parts, be they inside or outside of the bar, contain less of the white powder and more black scales than the crystallized; I therefore infer, that the carbon is here substituted for silicon; and as No. 3. contained more silicon than No. 2., it seems either that the silicon of No. 2. was partially driven away during its preparation by the carbon, or rather that the silicon remained in combination with carbon.

A fragment of Bombay wootz was also very slowly attacked by the acids, and deposited on the sides of the glass the before-mentioned white powder.

21·71 grains of No. 3. left white residuum dried at the heat of boiling water = 0·3437 grains.

A part of a coloured blister of Dannemora steel deposited likewise a *white powder*, and was like the before-mentioned samples.

I took afterwards two pieces of iron from the puddling furnace, one just before the granulated mass began to become coherent, and the other just before the iron was ready for balling or for being made into balls, in order to bring them under the forge hammer. These two samples were treated with acids as before. The first sample, weighing 29·89 grains, separated a gray powder, and left a black skeleton of the iron, which very easily crumbled into powder. The weight of the gray powder was 0·421, and the black skeleton weighed 0·250.

The second sample, which had been forged and consisted of a mixture of grains and fibres, weighing 36·625 grains, left grayish-green powder first, and on the acid being changed for the third time, it deposited white powder and left also a black skeleton, which oxidized very rapidly and was soon converted into a brown powder. Concentrated nitric acid would not act at all on this remainder, but on the addition of hydrochloric acid the powder became of a bright red. The remaining powders together weighed 0·8125, and the black skeleton 0·4531.

We learn from this that the *black remainder* increases with the progressive advancement from cast iron to that of malleable, and that the gray powder belongs to the gray granulation, and the white remainder to the finished fibrous iron.

The relation of the black remainder to the gray appears to be in all cases the same, only that the quantity increases progressively towards the finishing of the puddling.

We gather further from these facts, that the yellow powder appears only on using concentrated hydrochloric acid, and with that species of iron which nearly approaches steel and wrought iron.

At first I considered the yellow powder to consist of *silica* with a small portion of iron, and with the view of ascertaining with certainty the correctness of my opinion, I collected the yellow residuum of the three before-mentioned specimens on a very small filter, and separated as much powder as I possibly could.

The powder retained, after being dried, its light yellow colour, and was neither attacked by acids, except by very *concentrated hydrofluoric* acid; it became white by heating it on a platinum foil, and melted with soda on charcoal before the blowpipe, under effervescence, into a transparent globule of a ruby colour, which retained its transparency after cooling; a circumstance which seems to indicate the presence of silica as well as *sulphur*; the presence of the latter was also ascertained by the smell.

0·41 of this yellow powder was melted in a platinum crucible with carbonate of soda, and the effervescence was very vivid. I separated silica to the amount of 0·115. The solution, from which the silica was separated, saturated with carbonate of ammonia, left alumina with a little silica, which amounted to 0·107; no trace of iron could be detected.

Fourteen grains of filings of English iron, made after my peculiar method for steel, were then dissolved in a test glass with five drams of hydrochloric acid. The iron dissolved rapidly; a dark gray oily skinny scum collected on the surface, and the glass was filled to an inch in height with a whitish gray saline granulated precipitate of protochloride of iron, which, on having removed the acid and substituted distilled water, disappeared completely. The solution, stirred and quickly poured into another vessel, left *gray heavy scales* of the form of the filings on the bottom.

These gray-white scales, well washed, were very slowly attacked by diluted hydrochloric acid; concentrated, it acted rather more powerfully, and the liquid became yellow and milky fourteen hours after the action of the acid had ceased. A few black spots remained on the bottom; a grayish viscid mass floated on the surface, like sulphur, separated by aqua-regia from sulphurets; the sides of the tube were covered by a layer of yellowish-white substance, which I could sublime over a lamp and volatilize. The fumes escaping had, in a degree, the scent of French brandy and fennel oil.

The black viscid mass, slowly ignited on a platinum foil, emitted first fumes of volatilized sulphur; then some sulphurous acid appeared; and lastly, the mass began suddenly to glow like tinder, and burnt without further assistance, leaving a residue of darkish brown powder, which, boiled with muriatic acid, assumed a vivid red colour, was scarcely attacked by aqua-regia, and only dissolved completely after being boiled again in hydrochloric acid. This scaly remainder consisted therefore of

$$\left.\begin{array}{l}\text{Iron and}\\ \text{Sulphur}\end{array}\right\}\text{ a great quantity;}$$

$$\left.\begin{array}{l}\text{Carbon and}\\ \text{Hydrogen}\end{array}\right\}\text{ very little;}$$

probably a sulphuret of carbon mixed with iron, or a carburet of iron mixed with sulphur.

The water, quickly separated from the heavy scales as beforementioned, became, after a little time, clear, a white precipitate having fallen to the bottom. This precipitate, heated first on a platinum foil, gave out sulphur, then a small quantity of sulphurous acid, when the mass began to glow and a *light white powder of silica* remained.

This light precipitate consisted therefore of

Silica,
Carbon,
Hydrogen,
Sulphur,
} without iron,

and was, in all probability, a carburet of silicon mixed with a carburet of sulphur.

The same quantity of steel filings from a razor made by Rogers of Sheffield, gave nearly the same result, with the exception, that the sediment was darker, and there remained more of the white sulphurous precipitate, covering not only the sides, but, in a thick layer, the bottom also of the test tube.

Iron which I made at Axat, in the Oriental Pyrenees, from a mixture of spathose iron and iron glance, differed only from the foregoing specimens in this, that the sediment appeared of a lighter colour and the sides of the tube remained uncovered by the *white precipitate.*

The first decanted liquid of this iron treated with sulphuretted hydrogen let fall a reddish yellow precipitate, soluble in ammonia, leaving white sulphur.

Further, I treated powdered cast iron from the Maesteg iron-works, near Neath in South Wales, with caustic potash in a test tube. After the evolution of ammonia had ceased, the mass was dissolved in distilled water, and a part of the iron was found remaining. Half of this powder was dissolved in hydrochloric acid; hydrogen escaped, and a whitish gray flocky precipitate remained. The other half of the remaining iron being again melted with caustic potash, and ammonia was again disengaged, leaving also a black granulated mass of iron, which was rather tough under the hammer, and afterwards being likewise dissolved in hydrochloric acid, a perfectly *white precipitate* remained. The acid was removed and distilled water substituted, until no trace of hydrochloric acid was to be found. I considered it to consist of sulphur and silica; but on heating it carefully over a spirit lamp, a very volatile vapour was disengaged, having some distant resemblance to the smell of cyanic acid gas. A little white powder remained on the bottom, and the sides of the test tube were covered by a dew of a perfectly transparent liquid. Having poured a few drops of distilled water into the tube, and afterwards a drop of solution of nitrate of silver, a white precipitate fell which retained its colour on exposure to the rays of the sun; a proof that it was not chloride of silver.

[To be continued.]

VIII. *A few Observations on the Authenticity of the Passage in the Treatise of Boetius* de Geometria *on Numerical Contractions. By* J. O. Halliwell, *Esq., F.R.S., F.S.A., F.R.A.S. &c.*[*]

I VENTURE to add the following remarks to what I have previously written[†], in consequence of a postscript by M. Libri, who wishes for more substantial evidence on the point of authenticity than has hitherto been produced. Such crude arguments as I am able to furnish must not by any means be considered as the result of a strict or lengthened inquiry, but are rather intended to show that the question is well worthy of much greater attention than has yet been paid to it.

M. Libri was the first who conjectured that this passage might be an interpolation, and with some justice; for it may be reasonably asked, why does not Boetius allude to the new system in his treatise on arithmetic. Again, from the abacal system employed in that treatise, I should be inclined to think that the articulate and composite divisions were certainly not introduced until after that period.

In the library of Trinity College, Cambridge, there is a very beautiful quarto MS. *of the eleventh century* on vellum, containing the treatise of Boetius *de Geometria* plentifully illustrated with neat diagrams. This manuscript, one of the most ancient in this country, does *not* contain the disputed passage. M. Libri has challenged me to produce such an evidence; *accipe si vis*—the manuscript may be found under the press mark R. xv. 14, and is briefly mentioned at p. 99 of Bernard's Catalogue, No. 491.

It would, perhaps, be scarcely fair to make its extreme singularity an argument against its authenticity, but we may be permitted to argue on the probability or improbability of such a passage being written at so early a period. Is it likely, that at a time when the arithmetic of the West was a mere geometrical adaptation of quantity, when Boetius himself recognised the Roman abacal system, and when it required no inconsiderable depth of foresight to appreciate the advantage of an arbitrary system of digital characters, that Boetius would have inserted so extended an innovation in a treatise written expressly on a science that has no immediate relation with that into which the improvement was introduced? At any rate it is a fair subject for discussion, whether we could reasonably suppose any writer fully acquainted with the merits of both systems and giving superior attention to the worst, if written previously; and if otherwise, why is no reference made to the treatise conducted with the common Roman notation? These considerations are almost sufficient of themselves to throw a doubt on its genuineness, although we must wait for the discovery of more direct evidence before any definite conclusion is broached. I know not what the forthcoming work of M. Chasles may contain, but every one interested in these matters must be well aware how greatly we are indebted to that able writer, and will readily leave the discussion of this point in his hands.

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[*] Communicated by the Author.

[†] See p. 447 of the present volume.

IX. *On the coloured Films produced by Electro-chemical Agency and by Heat.* *By* R. Warington, Esq.*

THE following paper is intended as an answer to a memoir of the late Prof. Nobili on this subject, the translation of which appeared in the first volume of the Scientific Memoirs, p. 94, under the title of a "Memoir on colours in general, and particularly on a new chromatic scale deduced from metallochromy for scientific and practical purposes." As this paper contains a great detail of matter on the physical characters and properties of colours and coloured films, and as it is only with respect to the chemical part of the subject that I propose treating it, it will be necessary to extract such sentences as refer more immediately to the questions at issue. I should not have ventured to attack a memoir coming from so high an authority as Professor Nobili, but from its having as yet elicited very little notice, and the views taken being so startling to the chemist and so perfectly original, I am induced to offer some practical experimental remarks on the subject. The memoir naturally resolves itself into two distinct subjects; namely, the nature of the coloured films produced through the medium of electro-chemical agency; and secondly, those produced by means of heat; these it is my intention to treat of separately and distinctly in their order.

Professor Nobili dates his *discovery* of the electro-chemical appearances as far back as the year 1826, and the account of them was laid before the French Institute in November 1828, and by their advice the distinctive appellation of metallochromy was adopted. The method by which these appearances are produced is thus described, p. 94: "A plate of platina is laid horizontally at the bottom of a vessel made of glass or china. A platina point is vertically suspended over this in such a manner that the distance between the point and the plate may be about half a line. A solution of acetate of lead is next poured into the vessel, so as not only to cover the plate, but to rise two or three lines higher than the point. The plate and the point are now brought into communication, the former with the positive and the latter with the negative pole of an electric pile. At the moment when the voltaic circuit is closed, a series of rings similar to those formed at the centre of the Newtonian lenses is to be seen on the surface of the plate precisely under the point." The same process has been adopted to produce the films, the investigation of which will be detailed in an after part of this paper. The voltaic powers consisted of two small batteries, constructed on the principles of Professor Daniell's, and the effects were produced in small platina dishes or capsules for the convenience of investigation. We must now pass on to another part of Professor Nobili's memoir, in order to collect the facts and their explanation. At p. 106, we find, "The appearances which constitute the chromatic scale are due to the electro-negative elements of the solution (oxygen and acid), which being transferred by the current to the positive pole, are then spread out into thin transparent films, from which all the colours of the scale arise. The electro-positive elements (such as hydrogen and the metallic bases) are, on the contrary, transferred to the negative pole, and then deposited in layers which never produce the colours of thin plates." Again, at page 109, Professor Nobili adds, "I will not undertake to say by what species of affinity or force it is that these elements" (oxygen and acid) "are attracted to each other and spread out into thin films on the platina. It is certain however, that they attach themselves to the platina without oxidizing it in the slightest degree. We must not suppose that this happens because platina is a metal difficult to be oxidized. Iron and steel belong to the class of metals most easily oxidized, and yet it is well known that they will bear to be covered with electro-negative layers without becoming rusted. My electro-chemical experiments, multiplied and varied in a thousand ways, leave no room for reasonable doubt on this point: they show that oxygen and certain acids may adhere to the surface of metals without producing the slightest chemical change in them. This is a novel state for oxygen and the acids, and is distinguished from their ordinary combination by the three following peculiarities: 1st, the metal retains, beneath the deposited layer, its natural brilliancy; 2ndly, this layer produces the phænomenon of the coloured rings in all its beauty; 3rdly, instead of oxidizing the metal, these electro-negative elements contribute to secure it against oxidation in every part to which they are applied. A fact so unprecedented is interesting to chemistry, and is entitled to particular attention, as tending to enrich the science by the introduction of new ideas."

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* Communicated by the Author.

In a foot-note the following theory is offered, "that the electro-negative elements disposed in thin layers on the surface of the metals are at too great a distance from the molecules of these substances to enter into combination with them." These extracts, although of no great length, will yet put the reader in possession clearly of Professor Nobili's views of the subject, and enable him to appreciate the bearing of the following experiments.

The "unprecedented fact" then "so interesting to chemistry" is entitled to particular attention, as it tends to enrich the science, and introduce new ideas. What is this fact? it is the production of coloured films at the positive pole (the poles being of platina) of an electric circuit, the connecting medium being a solution of acetate of lead. These coloured films, Professor Nobili states, consist of oxygen and acid precipitated, as it were, upon the surface of platina, iron, or steel, without producing any oxidation of these metals, and the correctness of which statement the multiplicity of the experiments, he states, places beyond a reasonable doubt.

Some of these splendidly coloured films, produced as stated before, were well washed with distilled water and acted upon by dilute nitric acid, which did not remove them, but appeared to dilute, as it seemed, the intensity and brilliancy of the colours: the solution was decanted and evaporated to dryness to remove all excess of acid, and on being tested gave indications of lead. Muriatic acid instantly destroyed all trace of the coloured films with evolution of chlorine gas and the formation of a curdy or crystalline chloride of lead. Another experiment was made by heating the films to redness and then acting upon them by dilute nitric or acetic acid, which dissolved them rapidly and yielded the usual indications of the presence of that metal. It would appear from these experiments that these electro-chemical appearances are therefore nothing more than extremely thin films of peroxide of lead or of red lead spread out on the surface of the platina, but from their great tenuity it is likely that they may modify in a degree the action of the acids used. I may be allowed to remark here, that if the voltaic arrangement is too powerful, or the solution of acetate of lead too strong, when the poles are brought into too close proximity, brown oxide of lead is thrown down in a powdery form. Now it is a well-known fact, which may be found in almost every work on chemistry, that peroxide of lead is precipitated from solutions of that metal by means of a voltaic current, platina forming the poles *. It is much to be regretted that Professor Nobili did not during the long period of his experiments put this matter to the test of chemical investigation.

The circumstance of lead in a high stage of oxidation being deposited in aggregated films at the positive pole of a voltaic circuit, will bear out an observation of Prof. Schœnbein, made at the last meeting of the British Association at Birmingham, that he believed that peroxide of hydrogen was sometimes formed during the decomposition of water, for that the volume of hydrogen gas eliminated was often more than double that of the oxygen. It will also afford a reason why iron should assume the inert or inactive state in voltaic combinations, particularly in an experiment exhibited by Prof. Schœnbein, of heating one extremity of an iron wire so that it was covered with a coating of oxide, in the form of a coloured film, and thus becoming inactive, while the end which had not been submitted to a similar operation was attacked with energy.

We now pass on to the consideration of the second division of our subject, the nature of the coloured films produced on the surface of metals by means of heat. This is a part of the subject more familiar to all parties, whether scientific or not, for every one must have noticed the beautiful colours produced on the polished steel bars of an ordinary stove, or on the surface of a copper tea-kettle. In the arts its applications are numerous and highly interesting, and the processes of tempering steel, the formation of the beautiful bronzing powders, of rose copper, and a variety of others, are too well known already to need anything more than a slight allusion to them.

Before entering on the analysis of the subject I must put you in possession, as briefly as possible, of Professor Nobili's views of the question. At p. 108, he states, " As to these colours, the most generally received opinion is, that they depend on a principle of oxidation. Berzelius calls the metallic

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* I was not aware until the morning of the day on which this paper was read before the Mathematical Society, that this matter had been investigated by Prof. Schœnbein, and published by him in the *Bibliothèque Universelle* for May, 1837, and that this investigation had succeeded a hint to that effect from Professor Faraday, in volume x. of the Lond. and Edin. Philosophical Mag.: for this information I am indebted to Mr. Brayley.

layer which is thus coloured a *suboxide*." In a foot-note at p. 110, we read, "Berzelius was more sensible of the difficulty," (of accounting for these coloured films) "perhaps, than any one else: but would not an open avowal have been better than the attempt to evade it by the adoption of the term *suboxide*, which is quite as vague and undefined as the *principle of oxidation*, for which it was offered as a substitute?" This is rather strong language to be used against such an authority as Berzelius, every one of whose statements is backed by investigation, and brought by Prof. Nobili, who does not adduce a single experiment in proof of his statements. But to proceed: "I have always entertained some doubts as to the correctness of this explanation; because each degree of oxidation has a colour peculiar to itself, and in no way related to that variety of tints of which we speak. I was also struck by the well-known practice of giving steel a violet colour in order to secure it from rust." "Were this tint, as it is presumed to be, the effect of oxidation, it would, in my opinion, instead of preventing, serve only to accelerate oxidation." "But this is not all; the superficial colours of which we speak are changeable, and belong evidently to the same class as those produced by thin plates. Now the pure metals are, from their opacity, incapable of this species of coloration. Can they acquire that capacity in their first degree of oxidation by becoming suddenly transparent in consequence of their union with a small quantity of oxygen? The hypothesis far exceeds the bounds of probability, and the phænomenon requires to be otherwise explained." Again, "Confining myself in this place to the colours produced on metals by the action of fire, I do not hesitate to say, that I think their origin now placed beyond the reach of doubt. It may be safely laid down as a general proposition, that the oxygen of the atmosphere produces them, not, as is supposed, by oxidizing the surface of the metal, but by becoming fixed in the form of a thin plate or film, similar to those of the electro-chemical appearances." Professor Nobili then gives a detail of the production of the colours by heat, and observes, that as long as the colours are seen there is no oxidation, but that when the metal loses its brilliancy and lustre it has become oxidized, and that if removed from the heating medium before this effect takes place, the oxygen will cover the metal and adhere as a varnish.

In Berzelius's System of Chemistry, under the heads of the various metals and the action of heat upon them, he states distinctly, that copper, lead, and tin form protoxides, and that palladium forms a suboxide; so that out of four metals which produce coloured films by heat, only one is stated to form a suboxide. The next point is, that each degree of oxidation produces colours peculiar to itself; this is offered as a reason for doubting the principle of oxidation. No person will imagine that each tint on the surface of the metal is owing to a different oxide, but that it arises, as experiment proves, from the films of the oxide being of various thicknesses, and that the surface of the bright metal below reflects the light through the film, as is the case with the oxide of lead produced by the electro-chemical agency; and that the brighter the metallic surface the more vivid and brilliant are the colours produced. With respect to the preserving power of a film of oxide on the surface of the metal, it is well known; and the principle on which it acts is the exclusion of moisture and carbonic acid from contact with that surface*. The next point is the capacity of metals to become transparent when united with oxygen, and here I think the evidence is satisfactory indeed. Copper, tin, antimony, titanium, zinc, and iron all occur in the mineral form transparent or translucent on the edges, and this in mass: of course, if reduced to thin films, such as are produced by heat, they would be perfectly so.

The same beautiful display of colours on the surface of metals may be produced by iodine: the manner in which I have formed these is simply to place a very small piece of iodine on the centre of a disc of metal,—copper or silver succeeds extremely well,—and cover the surface with a flat dish, so as to prevent the vapour of iodine from being dissipated: the circles of coloured films are very distinct and brilliant. The application of the thinnest or pale yellow of these films on silver by M. Daguerre, to the production of his pictures, is now well known. Sulphur and the application of heat produce similar effects. The process of oxidation by heat, and the disappearance of the metallic lustre, which Prof. Nobili attempts to use as an argument, are accounted for very simply thus; that the application of the heat being continued, the film of oxide gradually becomes thicker, until at last it becomes opake; but in the course of my experiments I have had these coloured films peel off from the surface of the metal with all their transparency and all their beautiful tints unimpaired. The same effect has taken place, by long-continued electro-chemical action, with the oxide of lead.

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* M. Zumstein, in August 1820, fixed a polished iron cross on the summit of Monte Rosa, in the Alps; and on again visiting it in 1821, it was found neither rusted nor corroded, but had merely acquired a tarnish of the colour of bronze, owing to the extreme dryness of the air at that elevation.

While arguing at page 110 against the probability of the principle of oxidation, Prof. Nobili observes that the violet tint on steel, "does not, perhaps, consist solely of oxygen, as it does when the metals are pure. Steel is a carburet of iron, and the oxygen of the air on being precipitated on this compound, becoming combined with the carbon, in some manner or other might form the layer in question." Now this is a most extraordinary statement. After arguing so long and so energetically against all thoughts of the oxygen uniting with the metallic surface, it is here hinted that, in some manner or other, the oxygen will take the carbon away from the iron, with which it was in combination, unite with it, and form, not a film of oxygen, but of carbon and that element, I suppose carbonic oxide or carbonic acid; and yet the theory advanced as an explanation for these phænomena, supposes that the electro-negative elements disposed in thin layers are at too great a distance from the molecules of the metal to enter into combination with them. These statements surely are contradictory. Allusion is made to the iridescent surface of the specular iron ore, and they are successfully imitated by electro-chemical means as given in the first part of this paper, but it must be borne in mind, that by this means lead in a high stage of oxidation is produced, and not a film or films of oxygen alone.

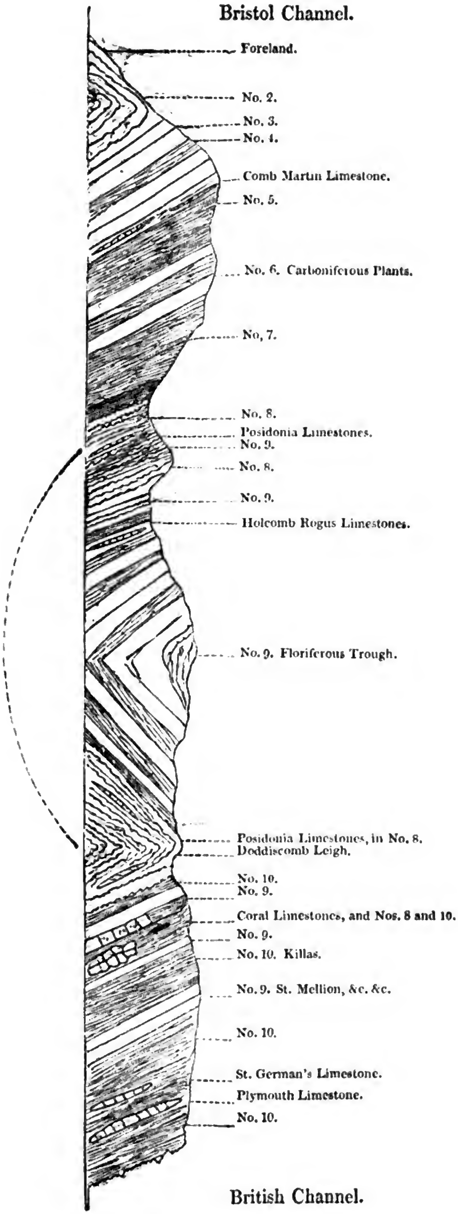
To sum up the whole of this subject in a few words, then, it appears: 1st, that the appearances called electro-chemical are not films of oxygen and acid, but lead in a high stage of oxidation thrown down on the surface of the metal by means of a voltaic combination acting through a medium formed by a solution of acetate of lead; 2ndly, that these colours owe their varied tints to the varying thickness of the precipitated film, and that the light is reflected through them from the polished metallic surface below; 3rdly, that the colours produced on the surface of metals by the application of heat are owing to the formation of thin films of oxide of the metal in consequence of exposure to the air during the process; that this does not involve the necessity of any one oxide being always formed, as this must vary according to the affinity of the metal used for oxygen, under the influence of a raised temperature; 4thly, that the opacity of the metal is not in the slightest degree an argument against the transparency of the oxide, as we have both in nature and art numerous cases which place this question beyond a doubt; 5thly, that we can produce analogous appearances by substituting other elements for oxygen, such as iodine, chlorine, bromine, sulphur, phosphorus, carbon, &c.

X. *On the Geology of Devon and Cornwall, with reference to a paper read before the Geological Society on December 4th, 1839. By the Rev.* D. Williams, *F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

As I do not consider the substance and spirit of my paper on Devon and Cornwall, which was read at the meeting of the Geological Society on the 4th inst. is fairly reported in the Athenæum of the 7th, I request you will favour me with an opportunity of righting myself with your readers, and of reporting progress since my communication which was published in your last October Journal (vol. xv. p. 293). I feel assured that I am not intentionally misrepresented in the Athenæum; the abstract however imputes to me (as I hastily read it before I left London) that I hold mineral characters to be everything and organic evidences nothing, in determining the relative ages of strata. Now it was very distinctly read by the Secretary, Mr. Darwin, that I did not consider the law proposed by Dr. Smith to be of any value in classifying the rocks of the earth in remote localities, if it did not suppose a final and universal extinction of genera and species; and in as much as some plants and animals would probably be enabled by the Creator to survive mutations which would be death to others, I considered that a classification of the older rocks should be regulated by some per-centage test, such as Mr. Lyell had applied to the tertiaries, rather than by a more restricted rule. I quote from memory, not being able to refer to either the Athenæum or to my paper. I stated that I unequivocally believed in the extinction of genera and species, severally at distant epochs, and therefore did *not* believe that the Posidonia and Goniatite, which I discovered in some trashy lentiform limestones in Devonshire, were specially created for the mountain limestone alone, when I knew it could be proved to demonstration, that those Posidonia limestones of Devon, and all their associated rocks, not only bore no lithological resemblance to any of the mineral types of any portion of the great English coal-field, but that they underlaid the coral limestones of South Devon, and the whole of the slates of Cornwall. I exhibited sections evidencing the actual supraposition of the Cornish killas on the floriferous series, No. 9, and the Coddon grit, No. 8, and I pointed out the localities. In all fairness then it remains for gentlemen to disprove those facts, instead of requiring me to show what I believe to be an impossibility, viz. the identity of the plants I found in the great floriferous outlier on the south and


Bristol Channel.
Foreland.
No. 2.
No. 3.
No. 4.
Comb Martin Limestone.
No. 5.
No. 6. Carboniferous Plants.
No, 7.
No. 8.
Posidonia Limestones.
No. 9.
No. 8.
No. 9.
Holcomb Rogus Limestones.
No. 9. Floriferous Trough.
Posidonia Limestones, in No. 8.
Doddiscomb Leigh.
No. 10.
No. 9.
Coral Limestones, and Nos. 8 and 10.
No. 9.
No. 10. Killas.
No. 9. St. Mellion, &c. &c.
No. 10.
St. German's Limestone.
Plymouth Limestone.
No. 10.
British Channel.

south-west of Callington, with those which had been procured from the fine culm shales near Bideford. I believe it to be an impossibility, on account of the same coarseness of the matrix in the great outlier, as exists everywhere, that I have seen, in the rocks of the floriferous series generally, where I have never met with clearly defined specimens, except in the finer culm-shales just mentioned. All I am prepared to prove then is, that about St. Mellion and Pillaton, between Callington and Plymouth, *plants in the same imperfect condition are found in precisely the same slates and shales, which are parted by thick beds of the same sandstone, and in intimate association with that singularly characterized and unique formation the Coddon Hill grit*; there *is the same triple association of the same rocks*, and in the *same order of succession*, that we witness in the base line of the floriferous series along the north and south borders of the trough, and where on earth could they come from if not from the same sources which supplied the constituents of the same rocks elsewhere in the same county? The only deviation on the S. and S.W. of Callington from the normal types of the floriferous series elsewhere, is frequent intercalations in it of undoubted killas, and beds of a composite or neutral character, constituted of moieties of killas and Coddon Hill grit, or of killas and floriferous, round the confines of the outlier seen not only in repeated alternation, but in other instances, their wedge-shaped extremities interlocking into each other like the teeth of a rat-gin. Here, as elsewhere along the confines of these two vast formations, Nos. 9 and 10, whether we advance towards the floriferous area on the one hand, or towards the killas on the other, we distinctly observe the one becoming thinner and evanescent as the other augments into unity and fulness.

Nature has manifestly conducted her operations of deposition and elevation in this region on a vast scale, and if her works be not regarded in their just proportions, we never shall arrive at the truth: thus as we explore the confines of Nos. 9 and 10, we are startled almost at the vastness of the ties and adjustments by which they are indissolubly united, till we reflect that they are only in a ratio to the magnitude and dimensions of their respective masses; that it is only the same transition and alternation on a larger scale, that we observe throughout Exmoor between the several members from No. 2 to No. 9, on a smaller; for while I hesitate as to the diameter of No. 9; No. 10, I repeat, is upwards of eight miles, measured according to Professor Playfair. Thus again, if we take a *coup d'œil* view of this country from one channel to the other, we have nothing more than one great wave (of probably some far extended undulation) consisting simply of two convex arcs inclosing a central trough, apparently the result of the same system of forces acting on a vast floor of matter, successively and regularly accumulated; either an overlying mass, or a fractured section of an original continuation of the Cambrian and Silurian deposits; for if we compare the precipitous and vertical cliffs of Nos. 2, 3, 4 and 5, of Exmoor (in echellon arrangement beetling over the deep tide way of the Bristol Channel) with the carboniferous limestone and secondary rocks of the opposite coast of Wales, we have all the evidences of an enormous fault.

But what are the results if we compare the positive testimony afforded by gradation, alternation, succession, and conformable supraposition, with that afforded by organic remains? The Petherwin fossils near Launceston, and those of the coral limestones of South Devon, I include without doubt or hesitation in a lower horizon, or subdivision of the floriferous series, No. 9, *above* the Posidonia limestones; so that if we suppose the ratio of extinction of vegetable species not to have been governed by the causes which effected that of marine zoophyta and testacea, the exceptions afforded by the Posidonia limestones are but as dust in the balance of organic evidence; and in this respect alone, geologists will involve themselves in inextricable difficulties and contradictions, if they reject the maximum and rely on the minimum amount of organic evidence. I repeat the fact, that the lower floriferous, and Coddon Hill grit series are overlaid in the south by the slates of Cornwall, which comprise in their ascending terms, first, the St. Germans, and lastly, the Plymouth limestones; so that it appears to me, that the great consecutive series from No. 2 to 10, evidences a transition of organic type, in progress, as it were, from the grauwacké *towards* the carboniferous limestone; that the latter, or its coal-field, is not represented here at all, but that the coarse slaty and red arenaceous beds which overlie the Plymouth limestones, extending thence to Rame Head on the south, probably do appertain to the early period of the Old Red Sandstone proper.

The relations of the floriferous, No. 9, to the coral limestones, and killas, No. 10, are explained with the greatest clearness and simplicity at and around Chudleigh; to aid my brief description I refer your readers to the accurate and faithful sections of Mr. De la Beche. (See Report, Plate IV. fig. 7 and 8). But why that able observer should assign a different position to the many other groups of coral limestone of South Devon, is to me unaccountable, because they are all manifestly of the same age and order with the Chudleigh limestones, viewed stratigraphically, zoologically, or mineralogically, and are seen under precisely the same associations, for I do not remember a single exception to the fact of the floriferous, Coddon grit, and killas, being either interstratified with, or underlying and overlying them: so that nothing can be predicated of the Chudleigh, that may not equally be affirmed of the coral limestones everywhere, if the parallel ridges immediately north and south of Chudleigh be the same floriferous, No. 9, and to doubt it is to doubt the plainest evidence of the senses: the controversy is at an end, for we trace them here continuously, from the culm-field; and I ask any fair and indifferent geologist merely to compare the rocks on the right bank of the Teign near Chudleigh-bridge, with those on the left, where a cutting for the road to Newton affords a good section of the west extremity of the Ugbrook ridge; and if he does not pronounce their perfect identity, the same dull olive-coloured sandstones parted by the same black shales, I will no longer advocate what I know to be the truth, and allow error to maintain the ascendant. No one doubts that chalk is chalk, or oolite oolite, or lias lias, elsewhere! The Chudleigh reef of limestones, which is lost under Haldon to the eastward, and cuts out near the Teign to the west, is a great alternation between the two floriferous ridges just mentioned, the three sequents dipping together at about the same angle to the south; while a careful examination of the coral limestones shows them to be based here immediately upon thick *black culmy* beds, and higher up to be parted by Cornish killas, beds of Coddon Hill grit and volcanic ash, with *plants.* The Creator has been so explicit here, that his works cannot be misinterpreted, if the laws recorded on these tables of stone be read without prejudice or control.

On discovering in the month of May last, at Doddiscomb Leigh, five miles north of Chudleigh, the Posidonia limestones (as everywhere else), included in the Coddon Hill grits, and together constituting the anticlinal axis of the south border of the trough to the east of Dartmoor (thus manifestly underlying the Chudleigh series, a fact confirmed by good cuttings and natural sections along the west bank of the Teign) the scales fell from my eyes—every difficulty and apparent anomaly vanished as if by magic, and the structure of the entire region, from one channel to the other, was presented to my mind's eye in all the grandeur of its simplicity; from Plymouth to Linton it was a simple series of successive emergence. A thousand embarrassing facts on the west of Dartmoor, and elsewhere, at once were reconciled, and the rocks appeared before me, like a cloud of witnesses, to testify that the floriferous series was overlaid by the Cornish killas, and requiring me, as it were, to restore each to his rightful throne.

My long section exhibited at the Geological Society did not perhaps show the south anticlinal axis sufficiently prominent or distinct, for I see by my maps that the Coddon Hill grit, commonly dipping *south*, occupies nearly two miles of country from north to south; and that at and about Doddiscomb Leigh, it is in the same parallel with the great line of fracture on the W. of Dartmoor which ranges by Launceston to Bos-Castle; and that this line continued through Dartmoor will intersect it at Amicomb Hill, between Fur-Tor and Yes-Tor, which Mr. MacLauchlan has determined to be the highest points of elevation in the West of England. Any omission in my section, however, I request may be imputed to my deficiency in tact in getting up a section, and not to any imperfection in the evidences afforded by the country; but in reply to the objection urged by Mr. Murchison, I may state, that the Posidonia limestones being only insulated patches in the Coddon Hill grit, and therefore part and parcel of the mineralogical axis, are quite as likely, in the southern fall, to dip away from the trough, as to dip into it; my section, however, gives the floriferous rocks as the most prominent of the anticlinal, which I still think is very near the truth, and may be explained by supposing them to arch over the subordinate Coddon Hill grits; or still better by the fact, that in the N. of Devon the Coddon grits are divided into an upper and lower, by great wedge-shaped masses of the floriferous rising into prominent hills, viz. south of Barnstaple, and north of Bampton, so that the lower range of these grits may not be exposed here at all.

All I have to say further is, that since the day I picked up the master-key at Chudleigh and Doddiscomb Leigh, I have not met with the least difficulty or embarrassment; nor do I anticipate anything hereafter but additional confirmation, from the conviction that nature will not be, as she has not been, permitted to deny herself; and I again earnestly invite Prof. Sedgwick and Mr. Murchison, or Mr. Weaver, to review the county; for after all, there are no gentlemen to whom I would sooner refer this question than to themselves.

I have the honour to remain, Gentlemen, &c.

Bleadon, near Cross, Dec. 16th, 1839. D. Williams.

XI. *Notices respecting New Books.*

A Treatise on Crystallography, by W. H. Miller, F.R.S., Professor of Mineralogy in the University of Cambridge.

IT is well known to those who have attended to the subject of crystallography, that the classification of crystalline forms introduced by Haüy, as well as the methods of expressing these forms and of calculating their relations, have been in a great measure superseded by other modes of treating the subject. The distinction of *systems* of crystallization proposed by Weiss and Mohs has been generally accepted among crystallographers; and the angles made by faces, edges, and the like, instead of being deduced by means of geometrical reasoning, have been obtained by the more general methods of spherical trigonometry and analytical geometry. Weiss may be looked upon as the person who first introduced this more general mode of calculation; and he has been followed by G. Rose, Kupffer, Köhler, Naumann, Neumann, Grassmann, Hessel, and others, in Germany, and by Mr. Levy, Mr. Brooke, and Mr. Whewell in England. Along with these different modes of calculation, different modes of notation for crystalline forms have also been employed. The old unsystematic notation of Haüy has been modified and retained by Mr. Brooke, Mr. Levy, and Mr. Phillips in England, and by several French writers; while Professors Mohs and Weiss have each introduced his own method of notation.

The notation of Mohs, in itself most superfluously cumbrous and unsymmetrical, has been made the basis of a much improved system of notation by Prof. Naumann; and the symbols of Weiss, which are really the most general, and depend upon a single convention, have been somewhat simplified by Mr. Whewell. In this state of the subject, we turn with great interest to the treatise of Professor Miller, who from his familiarity with analysis is able to give to crystallographical methods all the generality and simplicity of the best school of mathematics, and who likewise, from his acquaintance with special minerals, is not likely to fail in furnishing abundant exemplifications of his general methods. We may state in Professor Miller's own words the selection which he has made of a notation and mode of calculation. "The crystallographic notation adopted in the following treatise is taken, with a few unimportant alterations, from Professor Whewell's memoir on a general method of calculating the angles of crystals, printed in the Transactions of the Royal Society for 1825. The method of indicating the positions of the faces of a crystal by the points in which the radii drawn perpendicular to the faces meet the surface of a sphere, was invented by Prof. Neumann of Königsberg (*Beiträge zur Krystallonomie*), and afterwards, together with the notation, re-invented independently by Grassmann (*Zur Krystallonomie und geometrischen Combinationslehre*). The use of this method led to the substitution of spherical trigonometry for the processes of solid and analytical geometry in deducing expressions for determining the positions of the faces of crystals and the angles they make with each other. The expressions which in this treatise have thus been obtained are remarkable for their symmetry and simplicity, and are all adapted to logarithmic computation. They are, it is believed, for the most part new."

It is not possible for us to give any detailed account of Professor Miller's methods; but we may observe that each face of a crystal is determined by the portions cut off from the three *axes* of the crystal, and is expressed by a *symbol* ($h\ k\ l$) in which the indices depend upon these portions. When several contiguous faces have their intersections parallel, they may be considered as belonging to a *zone*; and this zone is indicated by its symbol [u v w]. Some of the simplest methods of determining the law of derivation of a proposed face consist in referring it to such zones. Thus if we have, given, the symbols of two zones [p q r], [u v w], the symbol of the face common to the two zones ($h\ k\ l$) is known from the equations

$$h = \mathrm{v\,r} - \mathrm{w\,q},\quad k = \mathrm{w\,p} - \mathrm{u\,r},\quad l = \mathrm{u\,q} - \mathrm{v\,p}.$$

The mathematical student of crystallography cannot fail to be delighted with the completeness and symmetry with which, in Professor Miller's Treatise, formulæ of this kind are obtained for each *system* of crystallization; and with the great and instructive variety of examples to which they are applied. It will be found, by attention to these examples, that the methods employed in this work are not only, analytically speaking, the most general and symmetrical, but also practically the most compendious and convenient for the determination of the laws of derivation of any proposed form.

We cannot help thinking, however, which we do with regret, that this book, mathematically so admirable, will be a sealed book to a large body of crystallographical students. It is written with a rigorous brevity, worthy of the ancient mathematicians; a quality, in itself, doubtless, a beauty, but one of those stern beauties which repel, rather than attract, common beholders. There is not a single phrase in which the author shows any sympathy for those of his readers who have not been disciplined in mathematics to the extent which his investigations require. And this requisite discipline is, in truth, not slight; for though the knowledge which he presupposes in his reader does not go beyond the doctrines of spherical trigonometry, no one can follow Prof. Miller's reasonings with any facility, except his habits of mathematical generalization and abstraction have been well matured. And even the method of indicating the positions of the faces of crystals by their *poles* upon a *sphere of projection*, although it much simplifies the calculation, obscures our conception of the relations of the crystalline form; at least it does this when we are first called upon to employ the method, and before it is become familiar to us. This, however, is an inconvenience attendant upon most simplifications of physical problems, and we speak to regret rather than to blame it in the present instance. But perhaps we might venture to express a wish that the practical rules for the calculation of crystals had been separated from the mathematical investigations which contain the demonstrations of the rules. If Prof. Miller would detach from these mathematical reasonings a body of *Precepts*, such as might enable the crystallographer, from proper measurements, to determine the symbols of the faces of any proposed crystal, putting these precepts in such a form that they should be capable of being employed by any person conversant with the processes and symbols of algebra, he would render his work useful to a much wider circle of calculators than will, we fear, now venture to apply his processes. Nor would this addition to the work at all mar the great mathematical beauty of matter and style which all competent judges will allow it to possess.

We cannot conclude this brief notice without expressing our satisfaction, that this subject of crystallography, after being put in so many forms for the last half century, has here assumed a shape which, so far as mathematical simplicity and symmetry go, leaves us nothing to desire, and therefore no reason for further change.

Transactions of the Cambridge Philosophical Society, vol. vii. *Part* I.

These Transactions have a claim upon our notice, not only from their general scientific importance, and especially from their containing the labours of several of our best British mathematicians, but also, in the part now before us, from the peculiar and comprehensive interest of the problems to which most of the memoirs refer. There are *three* great problems which at the present time have a manifest right to the best exertions which mathematicians of the highest class can employ in favour of physical science; and this claim has recently been allowed and acted upon to a great extent by the most eminent mathematicians of England, France, Germany, and Italy. These three problems are, the motion of waves in water; the undulations of the fluid or fluids by which light, heat, and similar phænomena are supposed to be produced; and the molecular forces by which the particles of bodies are held together; and of these, the two latter ones are closely connected with each other. All the papers in the present Part of the Cambridge Transactions, with one exception (the elegant memoir of Mr. Holditch on Rolling Curves), refer to these three problems; which have also been the subject of several investigations in previous parts of the Transactions.

On the subject of the first of these three problems, the motion of waves in water, we have a memoir by Mr. Green, who had in a previous memoir solved the problem of the motion of waves in a canal of small *variable* depth and width; a case which we believe had not been before successfully attacked by any mathematician. In the present memoir Mr. Green employs himself upon two or three other cases of the general problem, and in particular on the motion of waves in a *deep* sea. After solving this case, he adds, "We shall be able to deduce a singular consequence which has not before been noticed, that I am aware of." This consequence is, that any particle of the fluid revolves continually, (he might have added *uniformly*, as his expressions show,) in a vertical circular orbit of which the radius decreases very rapidly as the depth below the surface increases. We may point out to Mr. Green that this conclusion had already been virtually drawn by Laplace in his Memoir on the Tides, published by the Academy of Sciences in 1775. We may, however, observe that this result has acquired a new interest since the experimental researches of the Webers on this subject, with which it is in complete accordance. Mr. Green has in another case (that of a canal with a triangular section) compared his theoretical results with the experiments of Mr. Russell, and finds the agreement much more close than that which is given by Mr. Russell's own empirical formula.

The constitution and motions of the supposed fluid of light and heat form a wider subject of investigation. Ever since it appeared by the great discoveries of Young and Fresnel, that the hypothesis of transverse undulations explains with such marvellous exactness the most complex phænomena of light, mathematicians have been endeavouring to demonstrate the mechanism of such undulations, and to determine their laws under various circumstances.

M. Cauchy in France, Sir William Hamilton and Prof. Maccullagh in Ireland, Prof. Airy, Mr. Green, Mr. Kelland, Mr. Tovey in England, have employed on investigations of this kind all the higher resources of mathematics. In the volume now before us, we have, bearing on this subject, Mr. Green's memoir "On the Laws of the Reflection and Refraction of Light at the common surface of two non-crystallized Media;" and Mr. Earnshaw's "On the Nature of the Molecular Forces which regulate the Constitution of the luminiferous Æther." Mr. Green explains the peculiar starting-point of his researches in this manner. M. Cauchy had considered the luminiferous æther, and the bodies which act upon it, as systems of molecules in which every two particles act upon each other in the direction of the straight line which joins them. But this supposition, Mr. Green says, seems to involve too narrow a restriction; for many phænomena, those of crystallization for instance, seem to indicate certain polarities in the particles, which have never yet been shown to be resolvable into direct attraction and repulsion. Hence he selects for his basis a wider assumption, which may be expressed analytically, and which involves the precarious physical hypothesis of M. Cauchy as a particular case. He obtains from this principle various results, and in the first place this; that in the luminiferous æther the velocity of transmission of waves propagated by *normal* vibrations is very great compared with that of ordinary light. Mr. Green investigates the intensity of the waves reflected at the common surface of two media; and in the case of light polarized in the plane of incidence, obtains precisely the values given by Fresnel. In the case of light polarized perpendicular to the plane of incidence, it appears from the present investigations, that the expressions given by Fresnel are not rigorously true, but are only very near approximations. It appears that the intensity of the reflected wave will never become absolutely null, but only obtain a minimum value;

which value, in the case of reflection from water at the proper angle, is $\frac{1}{131}$ part of the intensity of the incident wave. This minimum value increases rapidly as the index of refraction increases; and thus the quantity of light reflected at the polarizing angle becomes considerable for highly refracting substances; a fact, which has been long known to experimental philosophers.

In Mr. Earnshaw's memoir the ætherial medium is treated as a system of detached particles; and he is led by his investigations to various conclusions, of which the most important are, that the molecular forces which regulate the vibrations of the æther do not vary according to Newton's law of universal gravitation, but that these forces are repulsive, and vary according to an inverse power of the distance greater than two. M. Cauchy, in his "*Mémoire sur la Dispersion de la Lumière*," had inferred from his analysis that "in the neighbourhood of contact, the action of two particles is repulsive, and reciprocally proportional to the fourth power of the distance."

Mr. Kelland, in a memoir contained in the previous volume of these Transactions, (vol. vi. Part I. p. 178) had been led by calculations founded upon the phænomena of the dispersion of light, to conclude that the particles of the æther act on each other with forces varying inversely as the square of the distance. We shall not here pretend to discuss the difference of the results thus obtained by these two mathematicians. But we must notice Professor Kelland's memoir "On Molecular Equilibrium," contained in the volume now under our notice. In this notice Mr. Kelland pursues a train of speculation somewhat similar to that employed in the last century by Dr. Knight, in his "Attempt to explain all the phænomena of Nature by two principles, attraction and repulsion;" and by Boscovich, in his "Theory of Natural Philosophy reduced to a single law of the forces which exist in Nature." Mr. Kelland states his assumption as follows: "I purpose to commence my investigation by retaining M. Mosotti's hypothesis of two systems of particles*, repulsive towards atoms of their own kind, but each respectively attractive towards the atoms of the other. We will call one system of particles *caloric*, and the other *matter*." He then adds the other suppositions by which these two elements are distinguished from each other; the atoms of caloric are distributed through space, the atoms of matter occupy only given positions. In both the density will vary from point to point; but the particles of matter are supposed to be much more widely separated than the particles of caloric; so that a material particle may be considered as a nucleus about which the particles of caloric are collected, forming its atmosphere. But Mr. Kelland afterwards determines the conditions of equilibrium of a system in which the atoms of caloric are repulsive of those of matter; and on the same hypothesis he determines the mutual action of two particles of matter, together with the caloric surrounding them. For the general relations between density of caloric, temperature, cohesion, and attraction of finite masses, which result from these investigations, we must refer to the memoir itself.

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* Taylor's Scientific Memoirs, vol. i. p. 448, and L. and E. Phil. Mag. vol. x. p. 320.

We shall notice, in the last place, Mr. Holditch's memoir "On Rolling Curves." The object of this paper is to determine curves of such a form that, revolving about two centres, one of them may communicate motion to the other, as in the case of the teeth of wheels; with the condition that the curves, in this communication of motion, are to roll upon each other without friction. Euler in the *Acta Petropolitana* had deduced the characteristic property of these curves, but he did not follow out the investigation so as to furnish actual forms of curves; nor has the method of obtaining such curves been pointed out by any previous writer. They are commonly found by a tentative process; but Mr. Holditch thought it worth while to search for rules and forms for their construction; and these he has found and given the present memoir. Some of the results are very curious and novel.

Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology, and as a Guide to the Philosophical pursuit of Natural History. By WILLIAM B. CARPENTER, *M.R.C.S., late President of the Royal Medical and Royal Physical Societies of Edinburgh, &c. &c.* With 240 Figures on Copper and Wood. London, 1839. 8vo, pp. 480.

The science of physiology has been too generally considered by physical philosophers as beyond their pale. The nature of the phænomena which it embraces, and the mode in which it is to be pursued, have been regarded as sufficiently distinct to limit the cultivation of it, with few exceptions, to those who make it a part of their regular professional studies. We cannot but think that such a state of things may be advantageously modified. Men of general science are constantly invoking the aid of the physiologist, for the determination of most important and intricate questions; and too often is it found that this aid is unattainable, in consequence of the exclusive notions of the latter, who, from his want of truly philosophical principles, cannot meet the difficulties which he is expected to solve; and, on the other side, physiologists are too often content with a smattering of knowledge on physics and chemistry, which is more likely to lead them wrong than right. We do not mean to assert that there are not many bright exceptions on both sides; but we maintain that the cause of philosophy would be benefited if the barrier which is supposed to exist between the science of vitality and that of general physics were broken down, and if the cultivators of each were to make themselves acquainted with the *principles* of the other, and with the best mode of pursuing and extending both.

Such appears to have been the object of the author of the volume before us. From the dedication of it to Sir J. Herschel, we infer that he has been trained in the school of physical science; and throughout the work we perceive the influence of those grand principles of inductive philosophy which have been and still are too much neglected by physiologists. All classes of living beings are regarded by him as of equal importance in a scientific view, as furnishing *instances*, by the collection and comparison of which *general laws* may be established. It is thus perceived that what is obscure in one is frequently evident in another; that the life of the simple zoophyte may elucidate, if properly observed, the varied phænomena presented by man; and that the functions of the humblest plant may be traced as fundamentally the same, though gradually becoming more complex, in the ascending scale of the animal as well as of the vegetable creation. Such a work, we cannot but hope, may contribute to excite and facilitate the study of physiology amongst those who make science their pursuit. We need hardly point out, that the connexion between these different branches of knowledge is daily being rendered more intimate, especially by the researches of the geologist and of the organic chemist; the former of whom requires to know those general laws which govern the conformation and distribution of organized beings, while the latter seeks to elucidate the mysteries of vital action, by ascertaining the extent to which the physical properties of matter are concerned in it. One of the most interesting examples of the value of such inquiries which has recently come under our notice, is the discovery of M. Poisseuille, that viscid fluids may be propelled through capillary tubes with much less effort than water or other liquids of aqueous consistence; and that a solution of gum, gelatin, albumen, &c. will pass readily through tubes so small as to resist the passage of water, whatever may be the degree of force employed.

A brief account of the contents of this treatise will serve to display its scope and tendency. The author states himself to have been led to its production by having "felt the want of a treatise which should give a comprehensive view of the science, embracing whatever general principles may be regarded as firmly established, and illustrating them as fully as could be done within moderate limits, yet without distracting the attention by profuseness of detail." It commences with an introduction, which presents a general account of the peculiarities of organized bodies, the elementary structure of plants and animals, and an outline view of the chief natural groups of these kingdoms, intended to facilitate the comprehension of the strictly physiological portion of the work. The first book is devoted to general physiology; and here we are conducted through a profound but lucid investigation into the nature and causes of vital actions, which we particularly recommend to the attention of those who have been accustomed to refer to the "vital principle" as an easy solvent for all difficulties. By comparing the phænomena of vital action with those of the inorganic world, the author shows that they are equally reducible to general laws which result from the properties with which matter has been endowed by the Creator. Of these properties some manifest themselves under the simple conditions which the ordinary changes in the inorganic world supply, and thus perform the actions termed *chemical* and *physical*; whilst others can only be called into play under conditions of a more complex nature, which are only supplied by a living organized system, where many particles being combined by a previously-existing life into one structure, exhibit actions of a peculiar character, dissimilar to any they have heretofore presented, which are denominated *vital*.

The dependence of life upon external stimuli is then pointed out, and a great variety of interesting facts, many of them novel, are collected, relative to the influence of heat, light, and electricity upon living beings. The general laws which have been ascertained to govern the structure and actions of organized beings are then enunciated, and their application illustrated by examples. To pursue their application through the whole range of the animal and vegetable kingdoms is the object of the second book, entitled Special Physiology. Here each function is considered in detail, in the various phases under which it appears in the ascending scale both of the vegetable and animal kingdoms; the fundamental unity which prevails throughout is displayed; and the very extraordinary correspondence which exists between the transitory states of different organs in the embryo condition of the higher classes of each kingdom, and the permanent forms of the same in the lower, is demonstrated in a striking and satisfactory manner. This part of the volume is illustrated by a large number of well-executed figures, which greatly aid the comprehension of the text.

The author appears to us to have fulfilled his design in the most satisfactory manner; and as his work has been received with high approbation by the Medical Press, we can feel no hesitation in recommending it to our scientific readers as the one best calculated to impart to them a sound knowledge of the principles of physiology. We may add, that the clearness of its style, and the simple manner in which the highest truths on this deeply-interesting subject are presented to the student, render it not only a useful, but an agreeable book to any reader of ordinary intelligence.

XII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

Nov. 8, 1839. THE following communications were read:—

On the Determination of the Orbits of Comets, from Observations. By G. B. Airy, Esq. Astronomer Royal.

The author begins by remarking, that the generality given by Laplace to the investigation of the orbits of comets is so complete, and the variations on the method introduced by other writers so numerous, that, as regards generality and facility, the subject may probably be considered as exhausted. The method which is developed in the present Memoir professes to be merely a modification of Laplace's method, directed by considerations of a purely practical nature, which are known to the working astronomer; but which, probably, have not occurred to the distinguished mathematicians, who have laboured on the theoretical difficulties of the problem.

"Every method," the author remarks, "which I have yet seen requires that the observed geocentric places of the comet be reduced to longitude and latitude. The places must, however, in the first instance, be observed in right ascension and declination. Now, the conversion of right ascension and declination into longitude and latitude is one of the most troublesome operations that commonly occurs. It requires the use of 7-figure logarithms, and is liable to errors. An alteration in one original Æ, or declination, requires a complete repetition of the calculations; and when all is done, the elements of the comet's orbit are obtained as referred to the ecliptic; and, for convenience of calculating predicted places, it is generally necessary to refer them back to the equator. For these reasons, it has long since appeared to me desirable that the orbits should be deduced at once from the right ascensions and declinations. Since I have become familiar with the instruments used for observing comets, an additional reason has suggested itself. It is known that on the assumption of a parabolic orbit, the equation given by three complete observations, or by observations which furnish the Æ and declination at a certain time, and their first and second differential coefficients, are one more than are necessary; and, therefore, it rests with the computer to use his discretion in rejecting one of the observations. Now, it often happens that the instrumental or observing errors in right ascension are of an order quite different from those in declination; and, if the method of computation proceeds at once from right ascensions and declinations, the astronomer can at once determine which of the observations ought to be rejected in the calculation, on the score of possible inaccuracy in the observation."

The principal objection which has been made to Laplace's method is the trouble of investigating the differential coefficients of the spherical co-ordinates. It must be avowed, that the process pointed out by Laplace is very laborious; but it may also be asserted, that the principal part of the labour is introduced without any necessity. Three observations, made at proper intervals, are sufficient to give the motion of the comet in either direction, and its two differential coefficients, with an amount of labour that is quite insignificant; or a great number may be introduced by a simple process well known to every computer, and involving very little trouble. In the present paper it is shown, that by adopting for epoch the middle time between the first and second observations, the great mass of the calculations of every kind may be made immediately after the second observation; and the operation, therefore, completed in a very short time after the third.

The author divides his paper into three sections. In the first he gives the "theory," or analytical solution of the problem. On substituting, in the general equations of motion, the right ascension and declination of the comet at the epoch with their first and second differential coefficients, which are given by the observations, he arrives at two equations in which the unknown quantities are ρ and $\frac{d\,\rho}{d\,t}$ (ρ denoting the comet's distance from the earth). On eliminating $\frac{d\,\rho}{d\,t}$ an equation is found of the following form—

$$C \,.\, \rho = \frac{D}{(\rho^2 - E\,\rho + F)^{\frac{3}{2}}} + G,$$

where C, D, E, F, G, are known numerical quantities. The solution of this equation may be obtained with great facility (in respect of the general difficulties of the problem) by the method of trial and error; and the author recommends, that in all cases which admit of it, the equation be formed, and the solution found; not only because the method is comparatively easy, but also because it is perfectly general, no assumption of parabolic, circular, or any other form of orbit, having been made.

The author next proceeds to consider the cases in which the equation fails. These are, first, when the comet is in conjunction with, or in opposition to, the sun; or when the sun, the earth, and the comet, are in the same straight line. In this case the first side of the equation becomes 0 divided by 0; and, as the two equations which involve the first differential coefficient of the comet's distance, taken with respect to the time, also vanish in the same circumstances, the failure is absolutely beyond remedy, and we can only wait until the comet is in a different part of its orbit. Secondly, the equation fails when the apparent path of the comet is directed to or from the sun's place; but in this case, the two equations involving the first differential coefficient of the distance do not necessarily fail; and, in fact, they cannot both fail, excepting under the supposition of the first case; therefore, by using one of them, or a new combination of them together with some new single assumption (as for instance, that the comet is moving in a parabola of unknown perihelion distance), we may still determine the comet's distance. Thirdly, the application of the equation may fail from causes connected with instrumental observations; for as the second differential coefficients of the right ascension and declination both occur on the first side of the equation, and as these coefficients are affected by the whole of the errors of observations, which, if the interval between the observations is short, receive very small divisors, any failure in the instrumental determination will produce a large error in their proportionate values. As it will sometimes occur that the observations made in declination are far more accurate than those made in right ascension, or *vice versâ*, in most cases one of the two equations which contain the comet's distance and its first differential coefficient, will be preferable to the other; and the combination of this with the equation deduced from the assumption of a parabolic orbit, will lead to the elimination of the differential coefficient, and, consequently, give the distance.

Among the various changes to which the comet's apparent path is subject, and of which an arbitrary choice may be made, for the purpose of determining the distance in the cases in which the general equation fails, or becomes unsafe, the author considers the following to be the best:—viz. first, the curvature of the comet's path, produced by the sun's action (or the deflection measured only in the direction perpendicular to the apparent path); second, the acceleration in its path, produced by the sun's action (or the deflection measured only in the direction of its path); third, the deflection in the direction in which both the sun's action on the comet and the sun's action on the earth would cause a change of the comet's apparent place (or the deflection measured along the great circle joining the comet with the sun). These changes are severally considered, and the method of forming the equation proper for each condition explained, and rules deduced for the guidance of the computer in all the particular cases in which the direct method cannot be followed. In these investigations the correction of observed places of the comet for parallax is entirely omitted, as it is most convenient, when ρ is approximately found, to correct the observations for the corresponding parallax, to make the proper alteration in the second differential coefficients, and then to repeat the process of approximation to the value of ρ.

Having given the methods for finding the distance and its differential coefficient, the author concludes his first section with an indication of the process by which the elements of the orbit are computed. In the rules for the selection of the equations on the parabolic assumption, some considerations are introduced which are new and important.

The second section contains remarks on the method of obtaining numerical values of the differential coefficients of the right ascension and declination from the observations. In the use of these quantities, what we have to consider is, not the effect of absolute error in their values, but of proportional error. An error of a single second in the value of the second differential coefficient of Æ may produce an ultimate error as great as would be produced by twenty seconds in the value of the first differential coefficient; or as great as would be produced by ten minutes in the Æ itself. This consideration allows the computer to determine many of the numbers which enter into the equations after the second observation: the method of proceeding is as follows:—

"Adopt for the epoch the middle time between the first and second observations: then the first differential coefficients of α and β (α denoting the right ascension, β the declination) will be obtained accurately by dividing the changes of α and β by the intervening time; and the values of α and β for the epoch will be obtained with sufficient accuracy, by taking the means of α and β for the two observations."

The third and last section of the Memoir gives practical rules for the computation of the observations. The successive steps of the process, from the first observations to the determination of the different elements of the orbit, and the values of the quantities required for predicting geocentric places, are minutely and distinctly stated, so that the ordinary computer will find no difficulty in applying the method.

Extract of a Letter from Professor Schumacher to the Astronomer Royal, relative to the determination of differences of Longitude, by observations of Shooting Stars.

M. Schumacher states that, although observations of shooting stars have long since been proposed by Mr. Benzenberg as a means of determining differences of longitude, no attempt has yet been made to carry the plan into practice. With a view to ascertain the degree of exactness with which such observations can be made, he resolved to make some trials on the night of the 10th of last August. He preferred to observe the *extinction* of the meteor, because its apparition gives warning, and in some measure prepares the observer for the phænomenon. Having given no notice of his intention to other astronomers, he had no expectation of obtaining corresponding observations; but was agreeably surprised when he subsequently obtained them from Bremen, Breslau, and even Königsberg. They did not give very accurate differences of longitude, because the observers at those places had observed the apparition and not the extinction; and because, not having the same object in view, they did not ascertain the equation of the clock with precision. Nevertheless the observations gave approximate differences, and showed that the method is practicable.

LINNÆAN SOCIETY.

April 16, 1839.—Read, "On a Gall gathered in Cuba, by W. S. MacLeay, Esq., on the leaf of a plant belonging to the order *Ochnaceæ*." By the Rev. M. J. Berkeley, M.A., F.L.S.

The gall is remarkable for its very close resemblance in habit and form to some epiphytous Fungi, for possessing a distinct operculum, and, especially, for bursting through the cuticle, which surrounds it in the form of a few laciniæ at the base. Mr. Berkeley pointed out various forms of galls and other productions of insects which have been described as Fungi, but in none is the resemblance so striking as in the present. He regretted that he was not able to throw any light upon the animal by which it is caused, though he was able to state positively that it is an animal production, as in most instances decayed exuviæ were found in its cavity, and in one case a little imperfect grub, which was however unfortunately lost.

May 24, 1839.—The Lord Bishop of Norwich, President, in the Chair.—This day, the Anniversary of the birthday of Linnæus, and that appointed in the charter for the election of Council and Officers, the President opened the business of the Meeting, and in stating the number of Members whom the Society had lost during the past year, gave the following notices of some of them:—

Samuel Brookes, Esq.—Mr. Brookes was devoted to the science of Conchology, and possessed a valuable collection of British and Foreign Testacea. He was the author of an Introduction to the Study of Conchology which appeared in 1815.

The Rev. Martin Davy, D.D., F.R.S., Master of Caius College, Cambridge.

The Rev. Richard Dreyer, LL.B.

John Lord Farnham.

Charles Holford, Esq.

Lawrence Brock Hollinshead, Esq.

John Hull, M.D.—Dr. Hull was ardently attached to the study of Botany, and in the midst of an extensive medical practice, he found occasional moments of leisure to devote to the cultivation of his favourite pursuit. We are indebted to him for the publication of a British Flora in 1799, of which a second edition appeared in 1808; and the Elements of Botany, in 2 volumes, 8vo, in 1800. These works, highly creditable to their author, tended to increase the taste for botanical pursuits.

Matthew Martin, Esq.—Mr. Martin reached the advanced age of 90. He became a Fellow of this Society in 1791.

George Milne, Esq.—Mr. Milne pursued with much ardour the study of Entomology for more than half a century, and his name is familiar to the cultivators of that branch of science in this country. He possessed an extensive cabinet of insects, particularly rich in British and Exotic Lepidoptera. He had retired from London for several years to his native place Johnshaven, Kincardineshire, where he died some months ago at an advanced age.

The Rev. Robert Nixon, B.D., F.R.S.

William Younge, M.D.—Dr. Younge was the early friend and a fellow student of our late distinguished President and Founder Sir J. E. Smith, and the companion of his tour on the continent in the years 1786 and 1787, of which an account appeared in three volumes 8vo, in 1793, and a second edition in 1807. Dr. Younge was elected a fellow of this Society at its first institution in March 1788.

Amongst the Foreign Members occur *M. Frédéric Cuvier*, Member of the Academy of Sciences of the French Institute, the younger brother of the great Cuvier, and eminently distinguished as a systematic zoologist. He was the author of a work on the value of the teeth as affording zoological characters in the class mammalia, and of a number of valuable papers on Descriptive Zoology in the Annales et Mémoires du Muséum. He likewise wrote the principal part of the text to the Histoire Naturelle des Mammifères, a work which he had undertaken in conjunction with Geoffroy St. Hilaire. Among his last productions may be noticed his Mémoire sur les Gerboises et les Gerbilles, printed in the second volume of the Transactions of the Zoological Society of London. He was distinguished, like his brother, for his candour and frankness of character, and a total freedom from those petty jealousies which too often beset men of science.

M. Charles de Gimbernat.

Gaspard Count Sternberg, Founder and President of the Royal Museum of Natural History at Prague, a distinguished patron of science, and author of a valuable original work on Fossil Plants, which were chiefly obtained from his own coal mines in Bohemia, and of an excellent Monograph of the genus *Saxifraga*, illustrated by coloured figures. To him we are indebted for the recovery of the vegetable treasures collected by Hænke in Peru, Cochabamba, and in the Philippines, whither he had accompanied the Spanish voyage of discovery under the celebrated, but unfortunate, Malaspina. These interesting plants have been published by Presl, under the auspices of Count Sternberg, in a work entitled 'Reliquiæ Hænkeanæ.' Count Sternberg was distinguished for his urbanity, hospitality, and an eager desire to promote every useful work. He left his collections and books of Natural History to the Museum already mentioned.

Among the Associates are the following :—

Mr. John Hunneman.—Mr. Hunneman having been long the medium of communication between the botanists of this country and those of Germany, Switzerland, and Russia, our collections have been enriched through his means with a vast variety of new and interesting plants. A curious Mexican genus, belonging to the natural family *Papaveraceæ*, bears his name, and commemorates the services rendered by him to science.

Mr. George Penny.—He was well acquainted with the plants which he successfully cultivated, and was the author of the 'Hortus Epsomensis', and of several papers on Garden Botany in Mr. Loudon's Gardener's Magazine.

Mr. William Weston Young made the drawings for Mr. Dillwyn's valuable work on British Confervæ, and a series of drawings of British birds now in the possession of Mr. Yarrell.

The President also announced that twenty Fellows, five Foreign Members, and two Associates had been elected since the last Anniversary.

At the election, which subsequently took place, the Lord Bishop of Norwich was re-elected President; Edward Forster, Esq., Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under-Secretary. The following five Fellows were elected into the Council in the room of others going out, viz. W. J. Burchell, Esq., J. W. Lubbock, Esq., Hugh Duke of Northumberland, John Forbes Royle, M.D., and William Yarrell, Esq.

NEW SYSTEM OF POSTAGE.

Our experience during the past month of the New System of Postage, though as yet in its incipient and imperfect state, has been most satisfactory and gratifying, in the facility and copiousness of intercourse with our scientific correspondents, however distant. Information, observations, suggestions, corrections, proofs, drawings, inclosures of various kinds, already begin to be interchanged with a freedom which is as delightful as it is new and strange, and therefore not yet enjoyed to its full extent. We shall find, we are persuaded, ample cause for gratitude to Mr. Hill, by whom so great a benefit has been suggested and perseveringly matured; and not less to Mr. Baring, the present Chancellor of the Exchequer, for having faithfully, diligently, and strenuously surmounted every obstacle to the accomplishment of an object, of which he has duly appreciated the importance, as regards not only the commercial, but the moral and intellectual interests of the country.

LETTER BAROMETER.

The equitable system of rating the postage of papers by weight gives rise to the necessity for a ready method of determining what charge any letter or packet may be liable to.

If extreme accuracy be desired, nothing will be found equal to good scales and weights; but as this is seldom essential, and such an apparatus is not very convenient on a writing-table, many contrivances of a less cumbrous nature and less subject to derangement have been produced.

These are generally variations of the steelyard, and of course require the adjustment of a counterpoise for each different case, which is somewhat troublesome and liable to mistakes.

We have lately seen a very simple and ingenious instrument, which avoids these inconveniences. It indicates the weight at once, is not subject to get out of order, and while it occupies but little space, is rather of an ornamental form than otherwise.

The instrument consists of a small tube containing a portion of quicksilver, in which is immersed a rod, furnished on its top with a tablet, on which a letter or even an unfolded sheet of paper may be placed. The rod sinks into the mercury precisely in proportion to the weight placed upon it, and by a graduation on the stem, it is at once seen what the charge of postage will be.

This little contrivance is the invention of John Taylor, Esq., F.R.S., and the instruments are very neatly made and sold, in a variety of forms, by Mr. Lund, No. 24, Fleet Street.

NEW SCIENTIFIC BOOKS.

Scripture and Geology.—On the Relation between the Holy Scriptures and some parts of Geological Science. By John Pye Smith, D.D., F.G.S., Divinity Tutor in the Protestant Dissenting College at Homerton. 1 Vol. 8vo. Jackson and Walford. Of this highly interesting and important work we hope shortly to give an account in some degree worthy of its merits.

Dr. Meyen's Report on the Progress of Vegetable Physiology during the year 1837. Translated from the German by William Francis, A.L.S.

METEOROLOGICAL OBSERVATIONS FOR NOV., 1839.

Chiswick.—Nov. 1. Hazy: rain. 2. Rain. 3, 4. Foggy: rain. 5. Rain: fine. 6. Hazy: rain. 7. Rain. 8. Hazy: rain. 9. Fine: drizzly. 10. Hazy: rain. 11. Clear. 12, 13. Hazy: overcast. 14. Fine: rain. 15. Hazy. 16. Overcast: clear and fine at night. 17. Rain. 18. Heavy rain. 19. Fine: a large halo round the moon at night. 20. Fine: rain. 21. Stormy and wet. 22. Overcast: fine. 23. Clear. 24. Rain. 25. Cloudy: rain: almost a hurricane at night. 26. Clear. 27. Dense fog. 28. Hazy. 29. Heavy rain: 30. Overcast: heavy showers.

Boston.—Nov. 1. Stormy. 2. Cloudy: rain P.M. 3. Cloudy. 4, 5. Rain: rain early A.M. 6, 7. Cloudy. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain A.M. and P.M. 11. Cloudy. 12, 13. Foggy. 14. Cloudy: rain P.M. 15. Cloudy: rain A.M. 16. Fine. 17. Fine: rain P.M. 18. Cloudy: rain early A.M. 19, 20. Fine. 21. Cloudy: rain early A.M.: rain P.M. 22. Cloudy. 23. Fine. 24. Rain. 25. Cloudy. 26. Cloudy: rain early A.M. 27. Fine: rain and snow P.M. 28. Cloudy. 29. Rain: rain early AM. 30. Stormy.

Applegarth Manse, Dumfries-shire.—Nov. 1. Storm of wind with slight showers. 2. Fair: weather moderated. 3. Fair: fine. 4. Rather moist. 5. Clear and cold. 6. Quiet day and cloudy. 7. The same: slight drizzle P.M. 8. Cloudy and moist. 9. The same: rain A.M. 10. Quiet day: moist atmosphere. 11. Calm day: still moist. 12. Showery all day. 13. Mild day throughout: no rain. 14. Drizzly and gloomy: a true Nov. day. 15. Rain all day: heavy P.M. 16. Showery throughout. 17. Fine day and fair. 18, 19. Drizzling day. 20. Very fine day: rain P.M. 21. The same: rain A.M. 22. Fine: one shower P.M. 23. Frosty morning: shower at noon. 24. Rain all day. 25. Showery A.M.: cleared up P.M. 26. Cold and frosty morning: shower snow. 27. The same: frost increasing; more snow. 28. Freezing all day: snow lying. 29. Storm of wind and rain: snow gone. 30. Rain nearly all day.

Days of Month. 1839.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
Nov.		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·714	29·730	29·677	29·50	29·88	29·75	41·2	41·7	39·8	49	42	41·5	47	40	E.	E.	E.	E.	·044	·14	...	...	39
2.	29·618	29·641	29·585	29·30	29·74	29·60	42·7	46·0	40·0	49	42	46	48	43	N.	E.	E.	E.	·044	·30	...	...	41
3.	29·612	29·629	29·518	29·26	29·58	29·57	48·2	49·0	42·7	52	48	47	50	43	NE.	E.	calm	E.	·302	·28	·38	...	41
4.	29·512	29·533	29·503	29·01	29·48	29·43	50·3	51·7	46·2	42	39	50	49	45½	W.	E.	E.	NE.	·263	·11	·34	...	46
5.	29·572	29·768	29·599	28·99	29·49	29·62	47·7	53·3	45·2	50	39	46	47	42	W.	SW.	NW.	N.	·088	·01	·37	...	45
● 6.	29·746	29·763	29·659	29·22	29·62	29·60	45·7	46·5	45·3	51	45	46	47	37	W.	W.	calm	NSE.	...	·09	...	0·16	46
7.	29·624	29·681	29·608	29·23	29·60	29·60	48·3	49·2	45·6	51	47	47	49	42	E.	E.	calm	E.	·116	·10	...	...	46
8.	29·806	29·699	29·623	29·16	29·49	29·41	49·8	52·0	48·5	55	46	50·5	50½	44½	SE.	S.	calm	E.	·033	·05	...	...	48
9.	29·506	29·507	29·446	29·	29·31	29·23	49·3	49·8	48·7	55	50	50	54	46	E.	SE.	E.	SE.	...	·01	·03	...	46
10.	29·378	29·381	28·978	28·85	29·17	29·10	52·4	54·6	48·7	56	50	50	52½	47½	E.	S.	calm	SE.	...	·55	...	...	49
11.	29·248	29·409	29·253	28·70	29·04	29·10	48·7	48·4	47·5	55	38	50	50	39	S.	SW.	calm	SW.	·463	...	·37	...	49
12.	29·418	29·579	29·411	29·	29·24	29·40	47·9	48·7	46·0	55	43	40	49½	38	SE.	E.	calm	SSW.	...	...	...	...	48
13.	29·748	29·944	29·739	29·25	29·63	29·74	48·7	49·4	47·4	51	43	47	49	41	NNW.	E.	calm	SW.	...	...	...	...	48
☽ 14.	29·988	29·984	29·939	29·45	29·70	29·70	47·4	48·0	47·4	55	49	47	48	40	SSE.	SE.	calm	SE.	...	·04	...	1·66	48
15.	29·980	29·978	29·824	29·50	29·70	29·52	52·5	52·8	47·4	56	46	51	54	45½	SE.	S.	calm	SE.	...	·04	·12	...	49
16.	29·886	29·993	29·872	29·34	29·51	29·55	52·8	53·0	51·5	56	48	50·5	56	49	SW.	S.	calm	SSW.	·036	...	·06	...	50
17.	30·044	30·029	29·998	29·43	29·73	29·79	51·8	52·4	50·6	57	51	48	51	43	S.	S.	calm	SSW.	...	·26	...	...	50
18.	29·992	30·280	29·983	29·44	29·73	29·74	54·8	55·3	50·6	37	43	53	52	44	S.	SW.	calm	SW.	·150	·36	·31	...	52
19.	29·992	29·985	29·948	29·39	29·60	29·65	49·4	50·3	47·9	55	38	46	50½	47	S.	SW.	calm	SW.	·305	·08	...	...	50
20.	30·064	30·150	29·990	29·56	29·83	29·63	44·7	49·7	44·6	35	29	42	46	37	W.	NW.	W.	E.	·063	·32	...	...	45
○ 21.	29·472	29·700	29·400	29·13	29·42	29·72	45·2	48·2	42·4	48	36	43	46	39	E.	NE.	SE.	E.	·244	...	·14	0·32	44
22.	29·916	30·152	29·907	29·49	29·91	30·07	42·2	42·7	39·6	48	30	42	45	33	NW.	NE.	calm	SW.	...	...	·03	...	42
23.	30·208	30·272	30·150	29·80	30·00	29·99	38·2	40·5	35·7	46	30	33·5	43	28	NW.	NE.	calm	W.	...	·05	...	...	39
24.	30·156	30·151	29·826	29·65	29·71	29·48	40·3	40·7	37·0	54	40	42	51½	40½	SW.	NW.	calm	WSW.	...	·01	...	...	38
25.	29·680	29·675	29·223	28·97	29·23	29·20	50·7	51·2	40·3	56	36	52	49½	41½	SW.	SW.	calm	NE.	·044	·12	·13	...	45
26.	29·420	29·432	29·410	28·98	29·21	29·21	42·2	42·5	38·2	46	21	41	38	30	SSW.	W.	calm	NNE.	·169	...	·25	...	45
☾ 27.	29·436	29·538	29·420	29·01	29·29	29·38	34·7	44·6	34·6	36	32	31·5	36	30	S.	NE.	calm	NNE.	...	·88	...	...	37
28.	29·546	29·584	29·569	29·15	29·41	29·53	39·2	39·8	33·4	41	32	37·5	34	23	E.	SE.	calm	ENE.	1·460	·06	·28	0·82	37
29.	29·268	29·298	29·159	29·04	29·41	29·24	43·3	44·0	39·4	51	41	45	44	28½	E.	SE.	E.	ENE.	·133	·37	·12	...	39
30.	29·606	29·665	29·621	29·16	29·19	29·47	42·4	42·7	42·3	48	28	43	45½	43	S.	S.	NW.	ENE.	·250	·04	·30	...	38
																						...	
Mean.	29·705	29·771	29·627	29·23	29·528	29·533	46·4	48·0	43·8	50·53	40·06	45·3	47·7	39·6					Sum. 4·207	4·27	3·23	2·96	Mean. 44·7

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[THIRD SERIES.]

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XIII. *An Account of some Experiments made in the South of Virginia, on the Light of the Sun. By* JOHN WM. DRAPER, *M.D., Professor of Chemistry in the University of New York.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just seen in the Journals for the current month, brought out by the British Queen, a letter from Sir J. Herschel to the [British] Association for the Advancement of Science, in reference to some remarkable actions of the different colours of the solar spectrum.

About five years ago, having the advantage of a bright and almost tropical sky, I amused myself with attempting a repetition of Morichini's experiment for the magnetizing of steel, and was led to some results in respect to the chemical action of the sun's rays, which appear to bear very much on the subject of the letter above alluded to. Most of these have been published in the Journal of the Franklin Institute of Philadelphia; but as they do not appear to have been noticed in England, I will ask the favour of a page or two of your excellent Magazine, to give my testimony on a subject, which now appears to excite so much interest.

1. If you pass a beam of the sun's light through a solution of chromate of potassa, it can no longer blacken a piece of sensitive paper; if you converge the light which has thus passed through a stratum of this fluid, by means of a lens, chloride of silver will remain for a long time without much change, in the focus.

The list, which was published in the Journal above named, of solutions possessing this power, is as follows:

Bichromate of potassa.
Chromate of potassa.
Yellow hydrosulphuret of ammonia.
Hydrosulphuret of lime.
Muriate of iron.
Chloride of gold.
Chloride of platinum.

It is to be remarked, that every one of these solutions is *yellow*, but I also found that a great many *vegetable coloured infusions* would in like manner absorb the chemical rays, especially those which have a *yellow* tint.

2. When I exposed pieces of paper covered with a layer of chloride of silver, to a beam which had passed through the red sulpho-cyanate of iron, the paper became of a brick-red colour; if to a beam which had passed through a solution of sulphate of copper and ammonia, it became of a blue brown; and lastly, on exposing a piece in a box which I shall presently mention, for five days, to light which had been acted on by bichromate of potassa, it became perceptibly of a faint yellowish green.

3. It is very probable, that there exist in the sunlight, rays having particular chemical powers.

A beam which has passed through bichromate of potassa, does not appear to cause the union of a mixture of chlorine and hydrogen. I kept such a mixture for several hours in it, and could not perceive any change.

But this same beam can nevertheless enable vegetable leaves to effect the decomposition of carbonic acid. I took a wooden box, about a cubic foot in dimensions, and having removed its bottom, replaced it with a pair of parallel plates of glass, so adjusted that there was an interstice between them of half an inch or thereabouts. Into the trough thus formed, I poured a solution of bichromate of potassa, or any other salt under trial, and the box being raised on one end, served as a closet in which bodies could be exposed to the action of beams that had passed through any given medium.

In this little chamber, its trough being filled with a solution of the bichromate, I placed a mattrass containing water slightly impregnated with carbonic acid, and a few vegetable leaves; after a little while, air bubbles were copiously given off; there had been placed, similarly in all respects, another mattrass in the direct rays of the sun, and when a quantity of gas sufficient for analysis was evolved, it was found that carbonic acid had in both cases been decomposed, though, as might have been expected, in the latter more energetically. The result gave a mixture of carbonic acid, oxygen, and nitrogen: the uniform appearance of this latter body was subsequently traced to the leaves employed.

Plants, also, become green in light that has been submitted to the action of these yellow salts, and therefore deprived of the rays that blacken chloride of silver. I took a number of pea-plants out of the garden, in May 1837, and caused them to vegetate in light modified in this way, and also in light which had passed through sulpho-cyanate of iron, and sulphate of copper and ammonia, &c., but in every instance the leaves became green. It may also be mentioned, that seeds of common cress were caused to germinate and grow under these circumstances; the young plants after reaching a certain size were always green, but those which had grown in the dark had yellow leaves and white stalks.

Professor Silliman states, in one of the early numbers o. his Journal, that he witnessed an explosion of hydrogen and chlorine, caused by the light of a common fire.

4. Ritter was the first who asserted, that the opposite extremities of the spectrum possess opposite powers of chemical action: he states that phosphorus will emit fumes in the red ray, but if the violet be thrown on it, it ceases to smoke: this experiment I repeated often, and under favourable circumstances, but could not make it succeed.

5. I could succeed, however, in showing very beautifully the interference of that class of chemical rays which blacken chloride and bromide of silver, but failed in trying to produce their polarization, for want of proper apparatus. An electric current circulating in a wire does not seem to have any influence on these chemical rays; I found that the same neat magnified image of the wire was obtained, on chloride paper, when it was placed in a beam diverging from a lens, whether the current was made to pass or was stopped.

So much for chemical actions; let me now ask your attention to a mechanical result of solar light, which is very curious.

(*a*). Having made a large air-pump jar very clean and dry, place a few pieces of camphor on the plate of the pump, and exhaust. Carry the pump with its receiver into the sunshine, and very soon you will see all that side which is nearest the sun covered with crystals, but there will be few or none on the side which is furthest from him. With the brilliant sun of Virginia, I have seen this effect take place, and beautiful stellated crystals appear *in four minutes*, literally covering the whole of the upper parts of the jar nearest the sun.

(*b*). Or, make in a tube of half an inch or more in diameter, and upwards of thirty inches long, a torricellian vacuum; pass up through the mercury a fragment of camphor. The tube may now be kept for any length of time in the dark, without anything happening; but bring it into the beams of the sun, and in a few minutes crystallization will happen, on the side next the luminary.

(*c*). Again, paste on the inside of an air-pump jar, a piece of tinfoil an inch in diameter, and having operated as in experiment (*a*) expose this side towards the sun. Crystals will soon form, but the tinfoil will protect the glass in its vicinity, and none will be found within a certain space round the metallic circle.

(*d*). Crystallization is not necessarily connected with these results: the vapour of mercury in a torricellian void is condensed towards the light; so also the dew which settles on the inside of a jar containing water is always on the side nearest the window. The rays of the sun have also the power of decomposing a solution of chloride of gold: the metalline spangles are deposited on that side of the glass which is nearest the light.

Artificial light gives none of these results.

(*e*). Having removed the piece of tinfoil used in experiment (*c*), place it on a little stand in front of the receiver; it will hinder the crystallization taking place in the parts on which its shadow is cast, and also for a certain space in the vicinity.

(*f*). Take a jar that has already been coated with crystals, place the tinfoil before it, and it will remove all those crystals which are within its shadow.

(*g*). Instead of using a piece of tinfoil as in experiment (*c*), make the receiver hot, and rub upon it a piece of resin, so as to leave a transparent circle of that substance; expose to the light, and it will be found that the resin cannot protect the glass.

(*h*). If along the inside surface of a vessel, about to be exposed to the sun, a glass rod be rubbed, rows of crystals will be deposited on the lines which were described by the end of the rod, but the vessel must be very dry for this experiment to succeed.*

Now, can we explain these singular results on any other *known* principle than this; that the side of the jar nearest the sun radiates freely the heat that it receives, back again, whilst radiation is interfered with at the other side; that in point of fact the anterior side is the colder, and the other the hotter? Yours respectfully,

University, New York, Nov. 28, 1839. JOHN W. DRAPER.

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* This result would appear to point to some change of the mechanical condition of the glass, affecting either the radiation from its surface, or that through its substance, or both.—EDIT.

XIV. *On a remarkable Fall of Hail; with Observations on the probable Nature of such Phænomena.* *By* P. J. Martin, *Esq., F.G.S.*

To Richard Taylor, Esq.

Dear Sir,

NOT having met with any notice of the remarkable fall of hail which took place during the storm of thunder and lightning that passed over the counties of Sussex, Surrey, and Middlesex on the evening of the 7th of last July, I beg leave to offer you the following memorandum respecting it, with some short observations on the probable nature of such phænomena, which, if not altogether new, may coincide with and serve to strengthen the opinions of more experienced meteorologists.

Except in the fall of very large hail, the storm above-mentioned did not differ materially from the wide-spreading and grander thunder storms which sometimes gather on our coasts, and pass over the metropolis, from the south-west, after a sultry day or two in the middle of summer, but of which we have not had any very remarkable examples for the last five or six years.

One of these storms generally rises slowly over the South-Downs towards evening, in the form of a flimsy cirrostratus, gradually deepening as the night advances, and engendering denser cumuli as it draws inland. In general it is met by an under current of air from the north-east; but this is not invariably the case, for the under current often comes in at an acute angle with the motion of the gathering clouds, from the south or south-east; and sometimes, as in this instance, there is a dead calm below, whilst the clouds are advancing with considerable rapidity in the higher regions. I may observe, in passing, that a long-continued and steady influx of a warm north-easterly current towards the points of precipitation generally characterizes the grandest of these exhibitions; so that it is common to hear it said here that "a tempest," for so the Sussex people call such a storm, "comes up against the wind."

On the evening in question, the storm gathered on the line of coast between Selsea Bill and Beachy Head, was heavy over Brighton, and seemed to have its nucleus or central range from about Shoreham over Henfield, East Grinstead, and Croydon. On its western verge it passed over Arundel, Pulborough, Horsham, and Dorking, and was noted at Chiswick to be "accompanied with unusually little rain." The hail which I am about to describe was confined to this western extremity, and its ravages did not extend more than about a mile and a quarter wide, whilst in length they reached about twenty miles, viz. from Arundel to the vicinity of Horsham. Hail fell, I believe, further on, quite into Surrey, but the fall of large stones was limited to the space above mentioned. We had been watching the rise, and dissolution into the expanding body of the nimbus of many heavy cumuli from the south and south-west, with some grand explosions of thunder and lightning, when we observed a dense mass approach us in that direction from the Arundel quarter, accompanied by a rushing or rather roaring sound, clearly to be distinguished from the thunder, and attended with a pretty sharp blast of wind. In a few moments hail of the ordinary kind began to fall copiously, and this in a few moments more was intermixed with stones of an enormous size, the slapping of which could be clearly distinguished from the roaring of the mass of other hail on the slated roof of the summer-house in which I and my family had taken shelter. Very little rain fell, and the duration of the hail-storm was about ten minutes, only five of which was occupied by the fall of the largest hail-stones. On its clearing off, the ground was observed to be whitened by the hail, amongst which the large stones lay like tennis-balls amongst marbles; and on measuring some of them, after they had lain several minutes melting on the ground, we found many five, six, and seven inches in circumference. These large stones were more compact in their structure than the smaller ones, and were all of the flattened spheroidal form, and likened by many of the common people in size and shape to their thick old-fashioned watches. A dead calm succeeded to the passage of the storm, and the atmosphere continued to be encumbered with dark clouds, but without any more rain during the night.

The congelation of large drops of rain at the moment of aggregation, and the formation of ordinary hail, and even a considerable accretion of more ice to the original globule in its passage downwards, do not seem to be very difficult of comprehension and explanation. But there is only one way in which I can suppose such masses of ice as these can be suspended long enough in the atmosphere to grow to such enormous sizes, and that is by the assistance of a nubilar whirlwind or water-spout (*Trombe aërienne*) with sufficient power to keep them in its whorl, and to resist the earth's attraction, whilst the concretive action is going on, till their momentum overcomes the suspending power, or till they are thrown beyond the range of its intensity. That such operations are amongst the reciprocal electrical phænomena of the clouds, distinct from, though allied to the water-spout, is, perhaps, well-known; and I was myself once witness to an appearance of this sort, between a higher and a lower cloud, that had the strongly electric aspect before they had resolved themselves into nimbus. It was a bent narrow column of dark vapour, which I could distinctly observe to be in rapid rotatory motion, passing from one cloud to the other, continuing for some minutes, and then gradually disappearing. During this time it emitted no sound, and had no visible connexion with the earth whatever.

The above theory of hail-stones will be further corroborated if we consider the form of the stones in this instance, viz. a sphere flattened at its poles, as the result of a rotatory motion; especially if it be a law, as perhaps it is, *that all solids in rapid gyration acquire per seipsos a rotation on their own axes.*

I am, dear Sir, yours, &c.

Pulborough, Dec. 16, 1839. P. J. MARTIN.

XV. *Notice of certain Meteorological Phænomena observed at Swansea.* *By* J. W. G. GUTCH, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THINKING that the following notice may prove interesting to some of your meteorological readers, I forward it for insertion in your valuable publication.

On the morning of the 20th of November, an unusual rise in the barometer was observable, as sudden as it was great. At 5 p.m. on the 19th, my barometer stood at 29·75; at 9 a.m. on the 20th at 29·99, being a rise of 0·24. At 9 a.m. on the 21st it sunk to 29·99, being a fall of 0·70; and so sudden a rise and fall I have not had occasion to record in my registry, now kept for the last four years. The wind during the whole period was a dead calm; the weather cloudy and hazy, with occasional light showers. A similar phænomenon was observed by my friend Mr. Addison of Malvern. The sudden *fall* of the barometer was noticed by that gentleman on the 20th, and the rise on the 21st, and like mine unaccompanied with wind, and at Malvern no rain fell. The explanation of this sudden rise and fall I am yet to learn, and should be glad if any of your correspondents could elucidate the subject.

On Wednesday, Nov. 6, the most brilliant meteor occurred that has been observed here for a great length of time, illuminating perfectly the principal street of the town: this occurred about 10 p.m.

On Monday, Nov. 10, at 1 p.m., the wind, which had been *perfectly calm* all the morning, suddenly rose, and with great violence blew for a minute or two at a pressure of four pounds to the square foot, and veered at the same moment direct from E., at which the vane had been standing all the morning, to W. continuing from that quarter for the remainder of the day, and immediately on so doing subsiding again to a complete calm.

This morning, Dec. 18, we were visited with a gale of wind surpassed only in violence by that of the 8th of May last. I send you the following table, drawn up from the actual markings of my self-registering anemometer and pluviometer.

Date.	Hour.	Pressure in Pounds on Square Foot.	Amount of Rain in 100dth of an Inch Cistern or Receiver 1 foot square.
Tuesday, Dec. 17.	10 a.m.	⅓	
	11	1	
	11 30 min.	3	
	12	3	
	1	1½	
	2	4	
	3	3	
	4	1⅔	
	5	5	
	6	2	
	7	2	
	8	6	
	9	6	From 9 to 10 0·03
	10	5	
	11	5	11 to 12 0·03
	11 30 min.	7	12 to 1 0·02
	12	9	1 to 2 0·03
	1	9	2 to 3 0·03
	2	10	3 to 4 0·02
	3	9	4 to 5 0·01
	4	10	5 to 6 0·02
	5	13 ! !	6 to 7 0·01
	6	8	
	7	7	
	8	8	
	9	6	9 to 10 0·01
	10	4	10 to 11 0·01
	11	3	11 to 12 0·01
	12	4	

Being a total of 0·21 of rain fallen from 10 a.m. on Tuesday to 12 p.m. on Wednesday, the wind the whole time blowing from the S.E., only twice for a minute or two getting to the N. of E. viz. at 3 a.m. and 8 a.m. of December 18.

It will be my endeavour to ascertain the other localities visited by this storm, as the former ones registered by my anemometer, and also by that used by Mr. Osler of Birmingham, would appear to have occurred at the time that might have been expected for each locality within the circle of the storm, thereby bearing out most fully Col. Reid's ingenious theory. The storm of Tuesday last was at its maximum at about a quarter to 5 a.m. of the 18th, being then at the pressure of 13 pounds on the square foot, or according to Dr. Hutton's table, travelling at the rate of about 60 miles per hour. The barometer on Tuesday at 9 a.m. stood at 29·82, it fell at 5 p.m. to 29·58; at 9 a.m. on Wednesday (to-day) to 29·27, and at 5 p.m. to 29·19, being a fall of 0·73 in the twenty-four hours.

Should the above remarks be worth your acceptance, they are most perfectly at your service.

I remain, Gentlemen, yours, &c.

Swansea, Dec. 18, 1839. J. W. G. GUTCH.

XVI. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions. By* JAMES IVORY, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from vol. xv. p. 507, and concluded.]

12. WE next proceed to inquire into the influence which the term multiplied by f', before omitted, may have on the refractions.

Investigation of the integral Q_3.

The expression of this integral is,

$$Q_3 = \int_0^m \frac{e\,dx}{\Delta}\left(8\,c^{-2x} - 8\,c^{-x} + 7\,x\,c^{-x} - 2\,x^2\,c^{-x} + \frac{x^3}{6}\,c^{-x}\right),$$

which is a negative quantity, as appears from the valuation of it in § 9: it will therefore contribute to distinctness if its sign be changed, in which case it will be thus written,

$$Q_3 = \int_0^m \frac{e\,dx}{\Delta}\left(-8\,c^{-2x} + 8\,c^{-x} - 7\,x\,c^{-x} + 2\,x^2\,c^{-x} - \frac{x^3}{6}\,c^{-x}\right);$$

and the formula for refractions will now be,

$$\delta\,\theta = \sin\theta \times \frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}}\left(Q_0 + \lambda\,Q_1 - f\,Q_2 - f'\,Q_3\right).$$

Suppressing the tedious operations of reducing, we may put the integral Q_3, taken indefinitely, in the following form, which it is not difficult to verify by differentiating:

$$\varepsilon = \frac{1-e^2}{e},$$

$$\begin{aligned} Q_3 = -4\int\frac{e\,.\,2\,d\,x\,c^{-2x}}{\Delta} &+ \frac{91}{16}\int\frac{e\,d\,x\,c^{-x}}{\Delta} \\ &+ \left(\frac{215}{16}\varepsilon^2 + \frac{175}{16}\varepsilon^4 + \frac{125}{48}\varepsilon^6\right).\int\frac{e\,d\,x\,c^{-x}}{\Delta} \\ &+ \frac{c^{-x}\Delta}{e}\left(\frac{185}{16} + \frac{125}{12}\varepsilon^2 + \frac{125}{48}\varepsilon^4\right) \\ &- \varepsilon\left(\frac{185}{16} + \frac{125}{12}\varepsilon^2 + \frac{125}{48}\varepsilon^4\right) \\ &- \frac{c^{-x}\,\Delta}{e}\left(\frac{95}{24}x - \frac{5}{12}x^2 + \frac{25}{24}x\,.\,\varepsilon^2\right). \end{aligned}$$

This being the indefinite integral, the value of Q_3 in the formula for the refractions will be obtained by putting $x = m = 10$; which gives

$$\frac{c^{-x}\,\Delta}{e} = \frac{1+e^2}{e}\,.\,c^{-m}:$$

and this value, as well as that of ε, being substituted, the quantity sought will be expressed as follows:

$$\begin{aligned} Q_3 = -4\int\frac{e\,.\,2\,d\,x\,c^{-2x}}{\Delta} &+ \frac{91}{16}\int\frac{e\,d\,x\,c^{-x}}{\Delta} \\ + \left\{\frac{215}{16}\left(\frac{1-e^2}{e}\right)^2 + \frac{175}{16}\left(\frac{1-e^2}{e}\right)^4 + \frac{125}{48}\left(\frac{1-e^2}{e}\right)^6\right\}&\int\frac{e\,d\,x\,c^{-x}}{\Delta} \\ + c^{-m}\left(\frac{125}{48}\frac{1}{e^5} - \frac{125}{16}\frac{1}{e} + \frac{905}{48}\,.\,\frac{1}{e} + \frac{905}{48}e - \frac{125}{16}e^3 + \frac{127}{48}e^5.\right)& \\ - \frac{125}{48}\,.\,\frac{1}{e^5} + \frac{125}{48}\,\frac{1}{e^3} - \frac{305}{48}\,\frac{1}{e} + \frac{305}{48}e - \frac{125}{48}e^3 + \frac{125}{48}e^5.& \end{aligned}$$

The series equivalent to the integrals must now be substituted, in order to express the quantity sought in terms containing the powers of e.

In the first place we have these three terms, each of which is zero when the exact values of A_1, A_3, &c. are substituted, viz.

$$\frac{125}{48}\left(A_1 - 1 + c^{-m}\right).\,\frac{1}{e}$$

$$+ \left\{\frac{175}{16}A_1 + \frac{125}{48}\left(A_3 - 6\,A_1\right) + \frac{125}{48} - \frac{125}{16}c^{-m}\right\}.\,\frac{1}{e^3}$$

$$+\left\{\frac{215}{16}A_1+\frac{175}{16}(A_3-4A_1)+\frac{125}{48}(A_5-6A_3+15A_1)\right.$$
$$\left.-\frac{305}{48}+\frac{905}{48}c^{-m}\right\}\times\frac{1}{c^5}.$$

The next three terms are as follows:

$$\left\{-4a_1+\frac{91}{16}A_1+\frac{215}{16}(A_3-2A_1)+\frac{175}{16}(A_5-4A_3+6A_1)\right.$$
$$\left.+\frac{125}{48}(A_7-6A_5+15A_3-20A_1)+\frac{305}{48}+\frac{905}{48}c^{-m}\right\}.e$$
$$+\left\{-4a_3+\frac{91}{16}A_3+\frac{215}{16}(A_5-2A_3+A_1)\right.$$
$$+\frac{175}{16}(A_7-4A_5+6A_3-4A_1)$$
$$+\frac{125}{48}(A_9-6A_7+15A_5-20A_3+15A_1)$$
$$\left.-\frac{125}{48}-\frac{125}{16}c^{-m}\right\}.e^3$$
$$+\left\{-4a_5+\frac{91}{16}A_5+\frac{215}{16}(A_7-2A_5+A_3)\right.$$
$$+\frac{175}{16}(A_9-4A_7+6A_5-4A_3+A_1)$$
$$+\frac{125}{48}(A_{11}-6A_9+15A_7-20A_5$$
$$+15A_3-6A_1)$$
$$\left.+\frac{125}{48}+\frac{125}{48}c^{-m}\right\}.e^5.$$

On substituting the exact values of A_1, A_3, &c., these three terms will come out as follows:

$$+\frac{158}{3}c^{-m}.e,\qquad\text{or } +\cdot 00239.e$$
$$-\frac{348}{5}c^{-m}.e^3,\qquad\text{or } -\cdot 00316.e^3$$
$$+\frac{8891}{75}c^{-m}.e^5,\qquad\text{or } +\cdot 00538.e^5.$$

These three terms are the part of the refraction that depends on the height of the atmosphere: at the horizon, or when $e = 1$, their amount is greatest and equal to

$$f'\times\frac{\alpha(1+\alpha)}{\sqrt{5i}}\times\cdot 00461=f'\times 726''\cdot 7\times\cdot 00461=f'\times 3''\cdot 3,$$

which, on account of the smallness of f', will be a minute fraction of a second.

Rejecting the six foregoing terms, we may assume

$$Q_3 = H_7 e^7 + H_9 e^9 + H_{11} e^{11} + \&c.:$$

and, having computed the differences in the following table,

	Δ^2	Δ^4	Δ^6
A_1	....	....	+·0278859
A_3	....	—·0595755	—·0175110
A_5	+·0873930	—·0177390	—·0199864
A_7	+·0748672	+·0065865	—·0079396
A_9	+·0446024	+·0109256	—·0002312
A_{11}	+·0209241	+·0073251	+·0016762
A_{13}	+·0081714	+·0034934	+·0012515
A_{15}	+·0027438	+·0013379	+·0005925
A_{17}	+·0008096	+·0004339	+·0001891
A_{19}	+·0002133	+·0001224	....
A_{21}	+·0000509	....	....

we shall have

$$H_7 = -4\,a_7 + \frac{91}{16} A_7 + \frac{215}{16} \Delta^2 A_5 + \frac{175}{16} \Delta^4 A_3 + \frac{125}{48} \Delta^6 A_1 = ·04861$$

$$H_9 = -4\,a_9 + \frac{91}{16} A_9 + \frac{215}{16} \Delta^2 A_7 + \frac{175}{16} \Delta^4 A_5 + \frac{125}{48} \Delta^6 A_3 = ·07091$$

$$H_{11} = -4\,a_{11} + \frac{91}{16} A_{11} + \frac{215}{16} \Delta^2 A_9 + \frac{175}{16} \Delta^4 A_7 + \frac{125}{48} \Delta^6 A_5 = ·04469$$

$$H_{13} = -4\,a_{13} + \frac{91}{16} A_{13} + \frac{215}{16} \Delta^2 A_{11} + \frac{175}{16} \Delta^4 A_9 + \frac{125}{48} \Delta^6 A_7 = ·00249$$

$$H_{15} = -4\,a_{15} + \frac{91}{16} A_{15} + \frac{215}{16} \Delta^2 A_{13} + \frac{175}{16} \Delta^4 A_{11} + \frac{125}{48} \Delta^6 A_9 = -·02230$$

$$H_{17} = -4\,a_{17} + \frac{91}{16} A_{17} + \frac{215}{16} \Delta^2 A_{15} + \frac{175}{16} \Delta^4 A_{13} + \frac{125}{48} \Delta^6 A_{11} = -·02558$$

$$H_{19} = -4\,a_{19} + \frac{91}{16} A_{19} + \frac{215}{16} \Delta^2 A_{17} + \frac{175}{16} \Delta^4 A_{15} + \frac{125}{48} \Delta^6 A_{13} = -·01835$$

$$H_{21} = -4\,a_{21} + \frac{91}{16} A_{21} + \frac{215}{16} \Delta^2 A_{19} + \frac{175}{16} \Delta^4 A_{17} + \frac{125}{48} \Delta^6 A_{15} = -·01023$$

$$H_{23} = -4\,a_{23} + \frac{91}{16} A_{23} + \frac{215}{16} \Delta^2 A_{21} + \frac{175}{16} \Delta^4 A_{19} + \frac{125}{48} \Delta^6 A_{17} = -·00487.$$

The coefficients of the assumed series being found, and being expressed in seconds of a degree, the part of the refractions depending on Q_3 will be as follows:

$$f' \times \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3 = f' \times \sin\theta \times \left\{ \begin{array}{rl} & \text{log.} \\ e^7 \times 35''{\cdot}324, & 1{\cdot}54807 \\ +e^9 \times 51{\cdot}529, & 1{\cdot}71205 \\ +e^{11} \times 32{\cdot}476, & 1{\cdot}51156 \\ +e^{13} \times 1{\cdot}809, & 0{\cdot}25755 \\ -e^{15} \times 16{\cdot}205, & 1{\cdot}20965 \\ -e^{17} \times 18{\cdot}588, & 1{\cdot}26925 \\ -e^{19} \times 13{\cdot}334, & 1{\cdot}12498 \\ -e^{21} \times 7{\cdot}427, & 0{\cdot}87080 \\ -e^{23} \times 3{\cdot}480, & 0{\cdot}54158 \end{array} \right\}$$

The amount of this expression at the horizon, or when $e = 1$, is $f' \times 62''{\cdot}1$, almost the same with $f' \times 62''{\cdot}4$, which, as is shown in § 9, is the limit of the integral when it is extended from $x = 0$ to $x = \infty$. It is thus proved that the error of the series is of no account. This part of the refraction cannot be computed because f' is unknown. But although the precise value of f' is uncertain, it is probably very considerably less than f, or $\frac{2}{9}$; so that the effect on the refraction cannot exceed a few seconds even at the horizon. We shall be better able to form a just notion with respect to this point, when the Theoretical Table in this paper is compared with observations.

13. It remains to investigate the corrections that must be made in the practical application for the deviations indicated by the meteorological instruments from the mean constants used in constructing the table.

For this purpose we have

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times S,$$

$$S = Q_0 + \lambda Q_1 - f Q_2,$$

$$\frac{\sqrt{5i}}{\cos\theta} = \frac{e}{1-e^2},$$

$$\lambda = \frac{\alpha}{i}.$$

The quantities e and λ depend only upon α and i: α varies both with the barometer and thermometer, and i, with the thermometer only: the quantity f does not seem liable to change in our climate. Admitting that the prefix d refers only to variations of the barometer and thermometer, we shall have

$$\begin{aligned}\delta\theta + d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \Big\{ \Big(1 + \frac{d\alpha}{\alpha} - \frac{1}{2}\cdot\frac{di}{i}\Big)\cdot S \\ + \frac{de}{e}\cdot\frac{dS}{de}\,e \\ + \frac{d\lambda}{\lambda}\cdot\lambda Q_1 \Big\}.\end{aligned}$$

Now
$$\frac{de}{e} = \frac{1}{2}\cdot\frac{di}{i}\cdot\frac{1-e^2}{1+e^2},$$
$$\frac{d\lambda}{\lambda} = \frac{d\alpha}{\alpha} - \frac{di}{i}:$$

wherefore,

$$\begin{aligned}\delta\theta + d.\delta\theta = \delta\theta\Big(1 + \frac{d\alpha}{\alpha}\Big) \\ - \sin\theta\cdot\frac{\alpha(1+\alpha)}{\sqrt{5i}}\cdot\frac{di}{i}\Big(\frac{S}{2} - \frac{1}{2}\cdot\frac{1-e^2}{1+e^2}\cdot\frac{dS}{de}\,e\Big) \\ + \sin\theta\cdot\frac{\alpha(1+\alpha)}{\sqrt{5i}}\cdot\Big(\frac{d\alpha}{\alpha} - \frac{di}{i}\Big)\cdot\lambda Q_1.\end{aligned}$$

If p denote the observed height of the barometer, reduced to the fixed temperature of 50° Fahr.; and τ the temperature of the air on the same scale; then $\beta = \frac{1}{480}$,

$$1 + \frac{d\alpha}{\alpha} = \frac{1}{1+\beta(\tau-50)}\cdot\frac{p}{30},$$
$$\frac{d\alpha}{\alpha} = -\frac{\tau-50}{480} - \frac{30-p}{30},$$
$$\frac{di}{i} = +\frac{\tau-50}{480},$$
$$\frac{d\alpha}{\alpha} - \frac{di}{i} = -2\times\frac{\tau-50}{480} - \frac{30-p}{30}.$$

These values being found, if we put

$$T = \sin\theta\times\frac{\alpha(1+\alpha)}{\sqrt{5i}}\times\frac{1}{480}\times\Big(\frac{(1+e^2)S - (1-e^2)\frac{dS}{de}\,e}{2(1+e^2)} + 2\lambda Q_1\Big)$$
$$b = \sin\theta\times\frac{\alpha(1+\alpha)}{\sqrt{5i}}\times\frac{2\lambda Q_1}{30};$$

the expression of the mean refraction with its correction will be as follows,

$$\delta\theta + d\,.\,\delta\theta = \frac{\delta\theta}{1-\beta(\tau-50)}\cdot\frac{p}{30} - \mathrm{T}\,.\,(\tau-50) - b\,(30-p).$$

The first term of this expression is the mean refraction corrected in the manner usually practised by astronomers. If we assume that the temperature of the mercury in the barometer is the same with that of the air, this term will be equal to

$$\frac{1}{1+\beta\,(\tau-50)}\cdot\frac{1}{1+\dfrac{\tau-50}{10000}}\cdot\frac{p}{30} = \frac{1}{1+c\,(\tau-50)}\cdot\frac{p}{30},$$

$$c = \cdot 002183,$$

the new factor being added to compensate the expansion of the mercury. Two subsidiary tables are given for computing this part: Table II. contains the logarithms of $\frac{1}{1+c\,(\tau-50)}$ for 30° on either side of the mean temperature 50°, negative indices being avoided by substituting the arithmetical complements; and Table III. contains the logarithms, or the arithmetical complements, for all values of p from 31 to 28.

The coefficients, T and b, of the other two terms vary with the distance from the zenith; and they can be computed in no other way than by reducing them to series of the powers of e. By substituting for $\lambda\,\mathrm{Q}_1$, the equivalent series already known, we immediately obtain

$$b = \sin\theta\,.\,\frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}}\cdot\frac{1}{30}\cdot\left\{\mathrm{B}_3\,e_3 + \mathrm{B}_5\,e^5 + \mathrm{B}_7\,c^7 + \&\mathrm{c.}\right\}.$$

Further, by expanding S and its differential, the expression of T will take this form,

$$\mathrm{T} = \sin\theta\,.\,\frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}}\cdot\frac{1}{480}\cdot\left\{\mathrm{G}_3\,e^3 + \mathrm{G}_7\,e^7 + \mathrm{G}_9\,e^9 + \&\mathrm{c.}\right\};$$

and we shall have

$\mathrm{G}_3 = \mathrm{A}_1 - \mathrm{A}_3 + 2\,\mathrm{B}_3 = 0{\cdot}2436$

$\mathrm{G}_5 = -\mathrm{A}_1 + 3\,\mathrm{A}_3 - 2\,\mathrm{A}_5 + 2\,\mathrm{B}_5 = 0{\cdot}4523$

$\mathrm{G}_7 = \mathrm{A}_1 - 3\,\mathrm{A}_3 + 5\,\mathrm{A}_5 - 3\,\mathrm{A}_7 + 2\,\mathrm{B}_7 = 0{\cdot}4705$

$\mathrm{G}_9 = -\mathrm{A}_1 + 3\,\mathrm{A}_3 - 5\,\mathrm{A}_5 + 7\,\mathrm{A}_7 - 4\,\mathrm{A}_9 + 2\,\mathrm{B}_9 = 0{\cdot}3502$

$\mathrm{G}_{11} = \mathrm{A}_1 - 3\,\mathrm{A}_3 + 5\,\mathrm{A}_5 - 7\,\mathrm{A}_7 + 9\,\mathrm{A}_9 - 5\,\mathrm{A}_{11} + 2\,\mathrm{B}_{11} = 0{\cdot}2092$

$\mathrm{G}_{13} = -\mathrm{A}_1 + 3\,\mathrm{A}_3 - 5\,\mathrm{A}_5 + 7\,\mathrm{A}_7 - 9\,\mathrm{A}_9 + {}^{11}\mathrm{A}_{11} - 6\,\mathrm{A}_{13} + 2\,\mathrm{B}_{13}$
$= 0{\cdot}1050.$

The series for T and b being now known, the coefficients of the terms must next be expressed in seconds of a degree, which being done, the following final results will be obtained.

$$T=\sin\theta\times\left\{\begin{array}{ll} & \quad\text{log.}\\ e^3\,.\,0''{\cdot}369, & -1{\cdot}5668,\\ +e^5\,.\,0{\cdot}685, & -1{\cdot}8356\\ +e^7\,.\,0{\cdot}712, & -1{\cdot}8526\\ +e^9\,.\,0{\cdot}530, & -1{\cdot}7263\\ +e^{11}\,.\,0{\cdot}317, & -1{\cdot}5006\\ +e^{13}\,.\,0{\cdot}159, & -1{\cdot}2013\end{array}\right.\qquad b=\sin\theta\times\left\{\begin{array}{lr} & \text{log.}\\ e^3\cdot 0''{\cdot}530, & -1{\cdot}7240\\ +e^5\,.\,1{\cdot}113, & 0{\cdot}0465\\ +e^7\,.\,1{\cdot}350, & 0{\cdot}1306\\ +e^9\,.\,1{\cdot}207, & 0{\cdot}0817\\ +e^{11}\,.\,0{\cdot}873, & 1{\cdot}9412\\ +e^{13}\,.\,0{\cdot}539, & -1{\cdot}7313\end{array}\right.$$

The values of T and b are added in separate columns of the annexed table for altitudes less than 10°: for greater altitudes they are omitted as of no account. The application for finding the corrected refraction from the formula

$$\delta\theta+d\,.\,\delta\theta=\frac{\delta\theta}{1+c\,(\tau-50)}\cdot\frac{p}{30}-\mathrm{T}\,(\tau-50)-b(30-p),$$

will best be explained by the examples afterwards given.

14. The Theoretical Table of refractions which has been computed by the foregoing formulas, and which is deduced solely from the phænomena of the atmosphere without arbitrary assumptions, is next to be compared with the tables most esteemed by astronomers. Two tables more eminently deserve this character; namely, Bessel's table with its supplement in the *Tabulæ Regiomontanæ*, which may be considered as the result of observations, and as being nearly exact to 88° or 88½° from the zenith; and the table published annually in the *Connaissance des Temps*. As all the tables are supposed to contain the same series of refractions, the numbers corresponding to the same altitude should have constantly the same proportion: so that taking the number α which answers to the zenith-distance θ in Bessel's table, the logarithm of the refraction at the same zenith distance in the new table should be equal to

$$\log\alpha+\log\tan\theta+{\cdot}00507,$$

the number ·00507 being the difference of the logarithms of the refractions at the altitude of 45° in the two tables: but, in the supplemental table, which contains the logarithms of the refractions, it is sufficient to add ·00507 to obtain the logarithms in the new table. With regard to the refractions in the *Conn. des Temps*, it is more convenient to use the table in the *Tables Astronomiques*, published by the French Board of Longitude: for the logarithms in this table with the addition of ·0011, should agree respectively with the logarithms of the new table. According to these directions the following comparative view has been drawn up.

Zenith dist.	Reflections.		
	New Table.	Tab. Reg.	Conn. des Temps.
°	″	″	″
10	10·30	10·30	10·30
20	21·26	21·26	21·26
30	33·72	33·72	33·72
40	48·99	48·99	48·99
45	58·36	58·36	58·36
50	69·52	69·52	69·52
55	83·25	83·24	83·25
60	100·85	100·85	100·86
65	124·65	124·62	124·65
70	159·16	159·11	159·22
75	214·70	214·58	214·83
80	320·19	319·88	320·63
81	353·79	353·38	354·33
82	394·68	394·20	395·37
83	445·42	444·86	445·87
84	509·86	509·23	511·22
85	593·96	593·38	595·80
85½	646·21	647·10	648·34
86	707·43	707·15	710·07
86½	779·92	777·36	783·07
87	866·76	864·59	870·37
87½	971·93	972·21	975·89
88	1101·35	1101·40	1105·1
88½	1262·6	1265·5	1265·0
89	1466·8	1481·8	1464·9
89½	1729·5	1764·9	1716·4

From this view it appears that the three tables agree within less than 1″ as far as 80° from the zenith; the new table is in accordance with Bessel's, with slight discrepancies, to 88° or 88½° from the zenith; from 80° to 88° of zenith distance the numbers in the French table exceed those in Bessel's, the excess being 2″ at 84°, and 4″ at 88°. But when the distance from the zenith is greater than 80°, the accuracy of the French table is questionable, both on account of the hypothetical law of the densities, and because the quantity assumed for the horizontal refraction is uncertain.

A few examples are subjoined, as well for explaining the use of the new table as for affording some indications of its accuracy at low altitudes. The two first instances are taken from the *Tables Astronomiques*, and are likewise published yearly in the *Conn. des Temps.*

EXAMPLE 1.
$\theta = 86° 14' 42''$
Therm. 8°·75 cent. = 47°·75 F.
Barom. 0m·741 = 29·17 in.

86° 10′	2·86345
4 42″	664
	2·87009
Therm.	·00214
Barom.	9·98781
Log $\delta\theta$	2·86004
$\delta\theta$	724·5
— ·25 × — 2¼	+ ·5
·4 × ·8	— ·3
Corrected refraction	12′ 4″·7
Observed refraction	12 4 ·2

EXAMPLE 2.
$\theta = 86° 15' 20''$
Therm. 8½° cent. = 46°·9 F.
Barom. 0m·766 = 30·16 in.

86° 10′	2·86345
5 20″	753
	2·87098
Therm.	·00276
Barom.	·00232
Log $\delta\theta$	2·87606
$\delta\theta$	751·7
— ·25 × —3·1	+ ·8
— ·4 × — ·16	+·6
Corrected refraction	12′ 33″·1
Observed refraction	12 32 ·5

EXAMPLE 3.
Mean of 42 sub-polar observations of α Lyræ by Dr. BRINKLEY.
Irish Transactions, 1815.
$\theta = 87° 42' 10''$
Therm. 35°
Barom. 29·5

87° 40′	3·00522
2 10″	392
	3·00914
Therm.	·01444
Barom.	9·99270
Log $\delta\theta$	3·01628
$\delta\theta$	1038″·2
— ·6 × — 15	+ 9·0
— 1·13 × ½	— 0·6
Corrected refraction	17′ 26″·6
Observed refraction	17 26 ·5

EXAMPLE 4.
Mean of 10 observations of Capella, from a memoir of M. PLANA.
Acad. de Turin, tom. 32.
$\theta = 88° 24' 9''·7$
Therm. 47°·75
Barom. 29·75

88° 20′	3·08087
4 9″·4	847
	3·08934
Therm.	·00214
Barom.	9·99607
Log $\delta\theta$	3·08755
$\delta\theta$	1223″·3
— ·95 × — 2¼	+ 2·1
— 1·6 × ·27	— 0·4
Corrected refraction	20′ 25″
Observed refraction	20 24 ·3

We may now inquire how far the refractions are likely to be affected by the term which it was found necessary to leave out, because the present state of our knowledge of the phænomena of the atmosphere made it impossible to determine the coefficient f' by which it is multiplied. For this purpose the term alluded to, viz.

$$\sin\theta \times f' \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3,$$

which may be shortly denoted by $f' \times \chi(\theta)$, has been computed by means of the equivalent series, for every half degree between 85° and 88°, the results being as follows:

θ	$f' \times \chi(\theta)$
85°	$f' \times 1''\cdot5$
$85\frac{1}{2}$	$f' \times 2\cdot0$
86	$f' \times 3\cdot3$
$86\frac{1}{2}$	$f' \times 4\cdot9$
87	$f' \times 7\cdot4$
$87\frac{1}{2}$	$f' \times 11\cdot2$
88	$f' \times 17\cdot0$

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From this view it appears that f', although considerably less than f or $\frac{2}{9}$ may still have some influence on the refractions at very low altitudes. The mean refraction in Bessel's table, and in the new table, can hardly be supposed to differ 2″ from the true quantity, which would limit f' to be less than $\frac{1}{10}$. It is a matter of some importance to obtain a near value of f': and it is probable that this can be accomplished in no other way but by searching out such values of f and f' as will best represent many good observed refractions at altitudes less than 5°. If such values were found, our knowledge of the decrease of heat in ascending in the atmosphere would be improved, and the measurement of heights by the barometer would be made more perfect.

April 25, 1838.

TABLE I.

Mean Refractions for the Temperature 50° Fahrenheit, and the barometric Pressure 30 inches.

Zenith dist.	$\partial\theta$.	Log $\partial\theta$.	Diff.	T.	C.	Zenith dist.	$\partial\theta$.	Log $\partial\theta$.	Diff.	T.	C.
1°	0′ 1″·02	0·0085	3012			7°	0′ 7″·17	0·8557	587		
2	2·04	0·3097	1763			8	8·21	0·9144	519		
3	3·06	0·4860	1252			9	9·25	0·9663	466		
4	4·08	0·6112	974			10	10·30	1·0129	424		
5	5·11	0·7086	796			11	11·35	1·0553	388		
6	6·14	0·7882	675			12	12·42	1·0941	359		
7	7·17	0·8557				13	13·49	1·1300			

TABLE I. (*continued*).

Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.	Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.
13°	0′ 13″·49	1·1300	334			57°	1′ 29″·73	1·95291	1664		
14	14·57	1·1634	313			58	33·23	1·96955	1691		
15	15·65	1·1947	294			59	36·93	1·98646	1722		
16	16·75	1·2241	278			60	40·85	2·00368	1756		
17	17·86	1·2519	265			61	45·01	2·02124	1794		
18	18·98	1·2784	252			62	49·44	2·03918	1836		
19	20·11	1·3036	241			63	54·17	2·05754	1881		
20	21·26	1·3277	230			64	59·23	2·07635	1932		
21	22·42	1·3507	222			65	2 4·65	2·09567	1988		
22	23·60	1·3729	215			66	10·48	2·11555	2048		
23	24·80	1·3944	207			67	16·78	2·13603	2116		
24	26·01	1·4151	201			68	23·61	2·15719	2191		
25	27·24	1·4352	195			69	31·04	2·17910	2275		
26	28·49	1·4547	189			70 00	39·16	2·20186	388		
27	29·75	1·4736	185			10	40·59	2·20573	390		
28	31·05	1·4921	181			20	42·04	2·20963	393		
29	32·38	1·5102	177			30	43·52	2·21356	396		
30	33·72	1·5279	173			40	45·02	2·21752	398		
31	35·09	1·5452	170			50	46·53	2·22150	402		
32	36·49	1·5622	168			71 00	48·08	2·22552	404		
33	37·93	1·5790	164			10	49·65	2·22956	407		
34	39·39	1·5954	162			20	51·25	2·23363	410		
35	40·89	1·6115	160			30	52·87	2·23773	413		
36	42·42	1·6276	159			40	54·53	2·24186	417		
37	44·00	1·6435	156			50	56·21	2·24603	419		
38	45·61	1·6591	155			72 00	57·92	2·25022	423		
39	47·27	1·6746	155			10	59·66	2·25445	425		
40	48·99	1·6901	154			20	3 1·43	2·25870	429		
41	50·75	1·7055	152			30	3·23	2·26299	433		
42	52·57	1·7207	151			40	5·06	2·26732	436		
43	54·43	1·7358	152			50	6·93	2·27168	440		
44	56·35	1·7510	151			73 00	8·83	2·27608	443		
45	58·36	1·76611	1512			10	10·77	2·28051	447		
46	1 0·43	1·78123	1514			20	12·74	2·28498	450		
47	2·57	1·79637	1518			30	14·75	2·28948	454		
48	4·80	1·81155	1523			40	16·80	2·29402	458		
49	7·11	1·82678	1530			50	18·88	2·29860	462		
50	9·52	1·84208	1539			74 00	21·01	2·30322	467		
51	12·02	1·85747	1551			10	23·18	2·30789	470		
52	14·64	1·87298	1565			20	25·39	2·31259	475		
53	17·38	1·88863	1577			30	27·66	2·31734	479		
54	20·24	1·90440	1596			40	29·95	2·32213	483		
55	23·25	1·92036	1617			50	32·30	2·32696	488		
56	26·41	1·93653	1638			75 00	34·70	2·33184	493		
57	29·73	1·95291				10	37·16	2·33677			

TABLE I. (*continued*).

Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.	Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.
75° 10	3′ 37″·16	2·33677	497			82° 40	7′ 7″·19	2·63062	899		
20	39·65	2·34174	502			50	16·13	2·63961	916	·07	·11
30	42·21	2·34676	507			83 00	25·42	2·64877	833		
40	44·82	2·35183	512			10	35·09	2·65810	948		
50	47·48	2·35695	517			20	45·14	2·66758	970		
76 00	50·21	2·36212	523			30	55·64	2·67728	985		
10	53·00	2·36735	528			40	8 6·55	2·68713	1005		
20	55·85	2·37263	533			50	17·95	2·69718	1028		
30	58·76	2·37796	538			84 00	29·86	2·70746	1047	·10	·16
40	4 1·74	2·38334	545			10	42·31	2·71793	1069		
50	4·79	2·38879	551			20	55·33	2·72862	1092		
77 00	7·91	2·39430	557			30	9 8·96	2·73954	1116		
10	11·11	2·39987	563			40	23·25	2·75070	1140		
20	14·39	2·40550	569			50	38·23	2·76210	1166		
30	17·74	2·41119	576			85 00	53·96	2·77376	1194	·15	·24
40	21·19	2·41695	583			10	10 10·52	2·78570	1219		
50	24·72	2·42278	589			20	27·90	2·79789	1248		
78 00	28·33	2·42867	596			30	46·21	2·81037	1281		
10	32·04	2·43463	603			40	11 5·55	2·82318	1312		
20	35·84	2·44066	611			50	25·90	2·83626	1342		
30	39·75	2·44677	618			86 00	47·43	2·84968	1377	·24	·39
40	43·76	2·44295	626			10	12 10·21	2·86345	1412		
50	47·88	2·45921	635			20	34·34	2·87757	1448		
79 00	52·12	2·46556	642			30	59·92	2·89205	1488	·31	·51
10	56·47	2·47198	650			40	13 27·11	2·90693	1627		
20	5 0·94	2·47848	659			50	55·99	2·92220	1570		
30	5·54	2·48507	669			87 00	14 26·76	2·93790	1612	·39	·67
40	10·28	2·49176	677			10	59·54	2·95402	1658	·43	·75
50	15·16	2·49853	688	·03	·04	20	15 34·55	2·97060	1704	·47	·83
80 00	20·19	2·50541	696			30	16 11·93	2·98764	1758	·52	·91
10	25·36	2·51237	707			40	52·10	3·00522	1808	·58	1·01
20	30·70	2·51944	716			50	17 35·12	3·02330	1862	·63	1·13
30	36·20	2·52660	727			88 00	18 21·35	3·04192	1918	·69	1·24
40	41·88	2·53387	738			10	19 11·07	3·06110	1977	·78	1·41
50	47·74	2·54125	749	·04	·05	20	20 4·68	3·08087	2040	·87	1·58
81 00	53·79	2·54874	761			30	21 2·60	3·10127	2102	·96	1·75
10	6 0·04	2·55635	772			40	22 5·22	3·12229	2169	1·07	2·00
20	6·50	2·56407	785			50	23 13·11	3·14398	2239	1·19	2·24
30	13·18	2·57192	797			89 00	24 26·8	3·16637	2316	1·32	2·48
40	20·09	2·57989	811			10	25 46·8	3·18943	2388	1·52	2·91
50	27·26	2·58800	824	·05	·08	20	27 14·2	3·21331	2461	1·72	3·34
82 00	34·68	2·59624	838			30	28 49·5	3·23792	2529	1·92	3·77
10	42·37	2·60462	851			40	30 33·2	3·26321	2573	2·20	4·34
20	50·33	2·61313	866			50	32 15·1	3·28894		2·48	5·00
30	58·59	2·62179	883			90 00	34 32				
40	7 7·19	2·63062									

TABLE II.

Thermometer.					
	Log.	Diff.		Log.	Diff.
°			°		
50	0·00000		50	0·00000	
49	0·00094		51	9·99906	
48	0·00190		52	9·99811	
47	0·00285		53	9·99717	
46	0·00380		54	9·99623	
45	0·00476	96	55	9 99529	
44	0·00572		56	9·99434	94
43	0·00668		57	9·99341	
42	0·00764		58	9·99248	
41	0·00861		59	9·99154	
40	0·00957		60	9·99061	
39	0·01053		61	9·98969	
38	0·01151	98	62	9·98875	
37	0·01248		63	9·98783	92
36	0·01346		64	9·98690	
35	0·01444		65	9·98598	
34	0·01541		66	9 98506	
33	0·01640		67	9·98414	
32	0·01738		68	9·98323	
31	0·01837		69	9·98231	
30	0·01935		70	9·98140	
29	0·02033		71	9·98049	
28	0·02133	100	72	9·97958	
27	0·92232		73	9·97867	
26	0·02331		74	9·97777	90
25	0·02432		75	9·97686	
24	0·02531		76	9·97596	
23	0·02630		77	9·97506	
22	0·02730		78	9·97416	
21	0·02832	102	79	9·97326	
20	0·02933		80	9·97237	

TABLE III.

Barometer.		
	Log.	Diff.
In.		
31	0·01424	
30·9	0·01248	
8	0·01143	
7	0·01002	142
6	0·00860	
5	0·00718	
4	0 00575	
3	0·00432	144
2	0·00289	
1	0·00145	
30·0	0·00000	
29·9	9·99855	
8	9·99709	
7	9·99563	
6	9·99417	146
5	9·99270	
4	9·99123	
3	9·98075	148
2	9·98826	
1	9·98677	
29·0	9·98528	150
28·9	9·98378	
8	9·98227	
7	9·98076	
6	9·97924	
5	9·97772	152
4	9·97620	
3	9·97466	
2	9·97313	
1	9·97158	
28·0	9·97004	154

XVII. *Letter to* Richard Taylor, Esq., *with reference to two Papers in the Philosophical Magazine for January*, 1840. *By* JAMES D. FORBES, *Esq., F.R.S.S., L. & Ed., &c.*

MY DEAR SIR,

IN order that your readers may be aware why I do not answer Mr. Potter's animadversions in the January Number of the Philosophical Magazine, I would request you to insert the following letter which I addressed to him after reading his communication.

"MY DEAR SIR, "Edinburgh, Jan. 8th, 1840.

"I have read with some pain, and at least equal surprise, some observations by you on photometry, stated to have been *occasioned by* reading a brief notice of some experiments of mine, with respect to *heat*, which I thought might, perhaps, bear some analogy to the case of *light*. It is evident, however, that as it would be impossible to predicate beforehand that heat and light are reflected according to the same law, any verification of Fresnel's law for light obtained in this way could only be an analogical one, and therefore acceptable only whilst photometric methods are so very imperfect as I still consider them to be, however dexterously employed.

"It was a matter, as I say, of equal surprise and pain to me to find that you should have so gratuitously misinterpreted my sentiments towards you, which I deliberately declare to you were solely those of entire friendship and respect. The object of this letter is simply to assure you of this, and if I could do it in stronger terms, I would.

"It seems to be strange and almost incredible that one whose experiments I have so often quoted with respect, whose results I have made known, and whose originality in the matter of metallic reflection I have so often vindicated at home and abroad, in private conversation and in public lectures, should take a pleasure in misinterpreting my expressions. I am persuaded that at some future time you will do me justice, and in the mean time I will rather run the risk of sustaining any prejudice which your letter may excite against my experiments until they appear to speak for themselves, than enter into a public disputation about statements and expressions, to the certain loss both of time and temper. I mean to write to Mr. Taylor to this effect, and shall perhaps communicate to him the substance of this letter.

"I am, my dear Sir, yours very truly,
"JAMES D. FORBES."

Now, Sir, after this statement it is not my intention to enter into any defence of the "memorandum" inserted in your Journal for December (L. & E. Phil. Mag. vol. xv. p. 479.). I shall correct neither Mr. Potter's statements nor his inferences, which so far as they relate to myself are certainly unfounded. Had I been at all aware of the extreme importance which Mr. Potter attributes to the particular experiments on photometry to which I alluded, I should certainly have done so with far greater caution. I imagined that Mr. Potter probably considered (as I think I would have done under the circumstances) his earlier contributions to physical science as subject to the revision of his own maturer skill and judgement, and, until they had received that revision, as open to some doubt; the subject being one of such difficulty, that if Mr. Potter failed, he failed only in common with perhaps every other person who had attempted it. In this supposition it appears that I was mistaken. I have since learned from Mr. Potter himself that he considers the true measure of light as more attainable than that of heat. A proposition so startling, and which is at variance with all that I have ever heard expressed, or should have been disposed to conclude upon the subject, I could not be expected to anticipate, and therefore a collision of opinion, though to be regretted, was unavoidable.

Before concluding, I have a single observation to offer upon Mr. Warington's interesting communication on Nobili's coloured rings. If Mr. Warington will refer to your Number for July last, page 27, *note*, (L. & E. Phil. Mag. vol. xv.) he will find the following remark: "The explanation of these colours, by supposing with the philosopher of Reggio (if I understand him aright) that they are produced by thin plates of *adhering oxygen gas*, is too evidently founded in error to require any notice." I may now add the consideration which seemed to me so conclusive, which is not a chemical but an optical one. The colours of thin plates are on all hands admitted to be produced by the interference of the light reflected at their first and second surfaces. In the present case the first surface would be the common boundary of air and oxygen gas, which can neither be considered as a sharp mathematical surface, nor if it could, would there be any appreciable quantity of light reflected from the boundary of substances having scarcely an appreciable difference of refractive power, much less could such intensely vivid colours be the result. This is but one of many palpable oversights in a paper, which, whatever may be its value to artists, seems unworthy of the scientific reputation usually given to Nobili, and in which notwithstanding, he speaks with very little respect of the reasonings of Newton and Berzelius.

I am, my dear Sir, yours very truly,

Edinburgh, Jan. 21, 1840. JAMES D. FORBES.

☞ We regret to find that we have incurred blame on account of some expressions in Mr. Potter's paper on Photometry in our preceding number, implying a charge of unfairness in the treatment of scientific questions against the Cambridge Philosophical Society, and which are complained of as being, "under the form of a scientific communication, an irrelevant and most unjust attack upon a public body." We freely admit the justice of the remark of a correspondent, that the editors of a scientific journal should avoid giving currency to imputations of this kind; and can only state, that had the nature of the charge, and the tone of some other expressions, caught our attention, we should have objected to its admission in the form in which we received it. We can safely appeal to the spirit in which our work has long been conducted in proof of our wish not to occupy its pages with personal imputations, or with the remonstrances to which they necessarily give rise. —EDIT.

XVIII. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals. By* George Gulliver, *F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards.* No. II.*

IN my former communication† I omitted to mention that in many of the observations an achromatic object glass was used, of one tenth of an inch focal length, made by Powell, and adapted to the same eye-piece as the excellent object-glass by Ross. They both perform admirably, and the additional power gained by the former one is not only of considerable advantage, but it affords an opportunity of instituting comparative trials, so as to diminish the chances of error. Both these glasses will therefore be employed in the succeeding observations, and I shall avail myself of opportunities of testing the measurements previously given, and of recording the results when they seem to be of any consequence. The magnifying power of Powell's glass with the micrometer eye-piece is as nearly as possible nine hundred and eighty diameters, and the object is very distinctly defined.

After repeating very carefully numerous observations on the corpuscles in their own serum, as compared with specimens dried in the manner formerly described, it appears that the latter are almost always a little larger and more accurately defined in the outline than the former. This is particularly observable at the margins of the dried preparations, where the corpuscles are very thinly spread, and where desiccation takes place instantaneously when they are applied warm from the wound to the glass. Towards the centre, as the particles are more thickly aggregated, they do not dry so quickly, but have time to contract a little, and accordingly correspond in diameter pretty accurately with those observed in their own serum. I have noted instances in which they manifestly shrunk while under examination in the serum, as if they were acted on by the glasses between which they were placed, the edges of the disks becoming more rounded, occasionally granulated, and not unfrequently puckered or swollen, so that the central concavity in many of them was very remarkable, and often more or less misshapen from the bulging of the edges towards the centre, a triangular depression with concavity of the margins being thus occasionally produced on the surface of the corpuscles.

Though saline solutions are useful in diluting the blood for comparative observations, measurements from corpuscles so preserved are not worthy of much reliance. The shrinking, or the alteration of form in the disks, may generally be observed even in the course of a minute or two after the mixture. Nor is the serum of one animal always proper to dilute the blood of another; for I could seldom get the corpuscles of the carnivora, or even of some ruminants, to mix well with the serum of the horse.

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* Communicated by the Author. † L. & E. Phil. Mag. for January 1840.

If obtained from the body a day or two after death, the disks are generally so clustered together as to be seen very indefinitely in the wet state, although some of the smallest detached from the masses are often tolerably distinct. The corpuscles are mostly very irregular in size, approaching more to the spherical shape, and even more susceptible of alteration from any of the common methods of dilution, than in blood procured from the living animal. By drying, however, a tolerably clear outline of the disks from the carcass may in most instances be procured, although every method sometimes fails, as I experienced a short time since in some blood from the Sloth Bear and from the Malay Sun Bear. Though the bodies of these animals were perfectly fresh, and the masses of fibrine in the heart and great vessels firmly coagulated, the particles of the blood were so much conglomerated, and their size so singularly variable, that it was impossible either by drying or any method of dilution to obtain even an approximation to their average diameter; and yet some corpuscles procured from the living Sloth Bear did not exhibit such irregularities.

Of the accidental circumstances by which the particles are liable to become enlarged, besides incipient putrefaction, the moisture of the atmosphere, of the breath, or of the hand are the most frequent. Dried or drying specimens are thus instantly injured or destroyed, the disks being more or less altered in shape and deprived of their colouring matter. But much depends on the degree in which these causes may have acted; for a diminution in the magnitude of the corpuscles may be the consequence. If, for example, some water be mixed with blood, the disks immediately become much enlarged and spherical, quickly losing their colouring matter; and yet if the whole of this be thus removed, after a while the outlines of the disks, very faint indeed, may frequently be recognised, diminished considerably in diameter and apparently quite flat. They may always be clearly seen by treating them with a strong solution of corrosive sublimate. The human blood corpuscles, thus enlarged at first, and then deprived of their colouring matter, and reduced in size, generally present a diameter of about 1-4800th of an inch, whether detected in the pure water or rendered more apparent by the sublimate.

They have a very characteristic appearance, being remarkably flat and pellucid, several generally touching at their edges so as to form groups, scarcely ever turning over or even moving in the fluid. It is obvious from the size, shape, and general appearance of these particles, that they are not identical with those which have been usually described as the nuclei of the blood corpuscles. The average diameter of the disks in the first instance was 1-3429th of an inch.

Besides the precaution concerning the preservation of the blood, it is equally necessary to be careful as to how it is obtained. The corpuscles are more or less modified very quickly after extravasation; and if some delay occur, and the drop of blood, effused into the subcutaneous tissue, require pressure to determine it to the surface, the corpuscles in that blood may be expected to be irregular in form and magnitude, many of them particularly presenting a granulated appearance. Hence the glass should not be pressed upon the wound, but merely lightly touched on a drop of the blood, however small, that has appeared freely immediately after the puncture. A specimen thus procured, and dried as before noticed, will be excellent, although in all cases where it is practicable a small incision directly into a superficial vein will be preferable.

The granulated particles are almost uniformly smaller than the common disks, and it is not improbable that some of the former may be produced by the irregular shrinking of the latter. In some instances I could not detect any of the granulated corpuscles in the blood immediately after it was taken from the animal, although they were to be seen abundantly after a few hours' exposure in the serum to the atmosphere, the temperature ranging between 45° and 50°. In one observation some of the extremely minute spherules, which are not uncommon in the blood, were observed to attach themselves to a few of the smaller disks, so as to produce the granulated appearance.

The blood corpuscles therefore are so singularly susceptible of variation in size and form from the operation of very slight agency, that there is probably no other microscopic object of equal delicacy, or that requires so much experience in its management. Hence it is not surprising that cursory observers should have committed remarkable errors, and that the history of the blood corpuscles, even after the labours of more careful inquirers, should have been so much obscured by discrepancies, particularly when it is considered how the inherent difficulties of the subject have been increased by the imperfection of instruments. Hewson indeed, whose observations apart from his hypotheses are generally remarkable for their accuracy, laboured under these disadvantages, but there is reason to believe that his results were not obtained without the most devoted and diligent inquiry.

The discrepancies just alluded to are frequently adduced as instances of how little credit should be attached to microscopical anatomy generally. There does not appear to me to be any reason in the objection; first, because it is seldom if ever urged by those who are sufficiently acquainted with the instrument to enable them to judge fairly of its use; and secondly, the same objection might in like manner be made to the use of the unassisted vision in minute anatomy. Whoever has attended to the history of the elementary structure of different parts in the animal œconomy, as given by anatomists who have not employed glasses, must be acquainted with differences in the observations, just as remarkable as those which have resulted from the use of the microscope. But no one ever yet ventured to suggest that the imperfection of our senses was a reason why they should be dispensed with. The intimate structure of the bones, of the cellular or adipose tissues, are, among others, singularly obscured by false observations, notable for their number as well as for their disagreement. Yet the microscope has made these things infinitely more simple. No anatomist would now require to found the distinctions between the cellular and adipose tissues chiefly on remote physiological phænomena; no one would doubt the difference who had once seen the vesicles of the latter by the aid of the microscope.. Haller could thus have immediately seen and shown to others the proof of that great discovery, which required so much labour to demonstrate. A curious collection might be given of the errors of anatomical observations made by the unaided vision as compared with those which have arisen from the use of the microscope*. But this subject is foreign to my purpose, and I have only to repeat the conviction which I have elsewhere expressed†, that the minute anatomy of the fluids, both healthy and diseased, is of the utmost importance, and that the steady and successful pursuit of this object will ultimately be the foundation of a new æra both in physiology and pathology.

76. Orang-outang, (*Pithecus Satyrus*,) a female, about a third grown. All the following sizes very common: 1-3552nd,

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* On the limits of vision with the best instruments there is an ingenious paper by Ehrenberg, of which we are indebted to Mr. Francis for an excellent English version. See Taylor's Scientific Memoirs, Part iv.

† On the Softening of Fibrine, Med. Chir. Trans., vol. xxii.

1-3429th, 1-3368th, 1-3309th, and 1-3200th. Extreme diameters 1-4000th and 1-3000th, though a very few considerably smaller might be seen in the serum. Blood from a vein of the fore arm.

77. Hoolock Gibbon, (*Hylobates scyrites,*) nearly full-grown female. Most frequent size of the corpuscles 1-3200th, but very variable from 1-4570th to 1-2782nd of an inch. Blood from the left ventricle, dried, three days after death.

78. White Whiskered Gibbon, (*Hylobates* ——— ?) a male, about two-thirds grown. Most frequent diameters of corpuscles 1-3428th and 1-3200th. Extreme sizes 1-4570th and 1-2900th. In serum 1-4000th common; in weak saline solutions still smaller, and many of the edges swelled and punctured, so as to form a very remarkable triangular or quadrangular depression in the centre of the disks. Blood from the left ventricle of the heart.

I am informed by Mr. Ogilby that this is a new species of Hylobates.

79. Magot or Barbary Ape, (*Papio sylvanus*), 1-3428th and 1-3200th most frequently. Extreme sizes 1-4570th and 1-2900th. Blood from the aorta and vena cava.

80. Rhesus Monkey, (*Macacus rhesus,*) a young male. Corpuscles very variable in size, 1-3200th very common; extreme diameters 1-4000th and 1-2666th of an inch. Blood from left ventricle of the heart.

81. Toque or Chinese Bonnet Monkey, (*Macacus radiatus*), a male, about two-thirds grown. 1-3600th and 1-3200th common size of the corpuscles. Blood from the left ventricle.

82. Black Ape, (*Macacus niger,*) full-grown male. Most common diameter of the disks 1-3554th of an inch; extreme sizes 1-4572nd and 1-2965th. Blood from right ventricle.

83. Hare-lipped Monkey, (*Macacus cynomolgus,*) full-grown male. Most common diameter of the disks 1-3429th of an inch; extreme sizes 1-4500th and 1-2666th. Blood from the renal vein.

84. Pigtailed Monkey, (*Papio nemestrinus,*) a female two-thirds grown. Most common diameter of disks 1-3329th and 1-3555th. Extreme sizes 1-4570th and 1-2900th of an inch. Blood from the portal vein, from the coronary veins, and from the right ventricle.

85. Jacchus Monkey, (*Jacchus vulgaris,*) adult male. Four measurements of three or four in a row, gave the following diameters of each disk: 1-3552nd, 1-3554th, 1-3693rd, and 1-3555th. Extreme diameters 1-4570th and 1-3000th. Blood from a prick of the tail.

In the venous blood of the monkey tribe, besides lymph globules of the common size and appearance, there are generally spherical bodies of a very white colour, and frequently of semi-fluid consistence, as may be inferred from their being seen occasionally to alter in shape, like a drop of any viscid matter subjected to currents of particles on its margins. The white round bodies vary in diameter from 1-4000th to 1-1777th of an inch. Their number is often very great, and they may be observed very remarkably in blood from the mesenteric veins. In the blood of the right ventricle they are also extremely common, though the semi-fluid appearance is more frequently seen in the former. These observations were made on monkeys, apes, and baboons, dead of various diseases, chiefly tubercular phthisis. Tubercle seems in these animals to be as common in the spleen as in the lungs; indeed, this deposit is not unfrequent in the former organ when none can be found in the latter.

86. European Brown Bear, (*Ursus Arctos*,) female, hardly a third grown. Most frequent diameters of corpuscles 1-3600th, 1-3692nd, 1-3750th, 1-3790th, and 1-4000th. Extreme sizes 1-4570th and 1-3048th. Blood from a prick of the upper lip.

87. Black Bear, (*Ursus Americanus*,) a male, nearly same size and apparently of the same age as the European Bear. All the following diameters common: 1-3600th, 1-3693rd, 1-3790th, 1-3840th. Extreme sizes 1-4570th and 1-3000th of an inch. Blood from a prick of the upper lip.

88. Cinnamon or Chocolate Bear, (*Ursus Americanus*, var.?) a male, from North America, apparently of the same age as the Black and European Bears. 1-3693rd, 1-3790th, 1-3840th, 1-4000th, all common sizes. Extreme diameters 1-4800th and 1-3000th. Blood from a prick of the upper lip.

89. Polar Bear, (*Ursus Maritimus*,) an old female. The following sizes very common: 1-3600th, 1-3693rd, 1-3764th, and 1-3840th. Extreme diameters 1-4570th and 1-3048th. There were seen, though rarely, some as small as 1-5333rd of an inch. Blood from a prick of the upper lip.

90. Sloth Bear (*Ursus labiatus*,) an aged female and a full-grown male. Most common diameter of corpuscles from 1-4000th to 1-3555th. Extreme sizes 1-4800th and 1-3000th of an inch. Blood from the different cavities of the heart of the female; from a prick of the lip of the male, which was a healthy animal. The unsatisfactory result of the examination of the corpuscles in the female has been already noticed; the specimen from the male was not a very good one.

91. Raccoon, (*Procyon lotor*,) nearly full-grown male. In the dried corpuscles, the following sizes all very common:

1-4500th, 1-4572nd, and 1-4800th. Extreme diameters, 1-6000th and 1-4000th. In the serum, a great many of the disks 1-6000th to 1-5333rd of an inch. Blood from a vein of the fore foot.

92. Wolf, (*Canis Lupus*,) adult male. 1-3554th, 1-3635th, 1-3692nd, all very frequent diameters. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the ear.

93. Jackal, (*Canis mesomelas*,) adult female. The following diameters very common: 1-3552nd, 1-3600th, 1-3693rd, 1-3790th. Extreme sizes 1-4570th, 1-3000th. Blood from a prick of the ear.

94. Two Spotted Paradoxure, (*Paradoxurus binotatus*,) a female nearly full-grown, from Western Africa. The following sizes frequent: 1-4572nd, 1-4800th, and 1-5052nd of an inch. Extreme diameters 1-6000th and 1-3555th. Blood from a prick of the tail.

95. Striped Hyæna, (*Hyæna vulgaris*,) female not quite full-grown. 1-4000th, 1-3764th and 1-3552nd, common diameters. Extreme sizes 1-4800th and 1-3000th. Blood from a vein of the ear.

96. Lion, (*Felis Leo*,) from Africa, nearly full-grown. The most common diameters 1-4500th and 1-4365th of an inch. Extreme sizes 1-5800th and 1-3554th. Blood from a prick of the ear.

a. Lioness, about two-thirds grown. Some corpuscles obtained from the cutaneous vessels of the leg gave the same measurements.

97. Puma or Silver Lion, (*Felis concolor*,) from South America, full-grown male, 1-4572nd, 1-4500th and 1-4440th, the most frequent diameters of the disks. Extreme sizes 1-5800th and 1-3554th. Blood from a prick of the ear.

98. Tiger, (*Felis Tigris*,) from India, a female, full-grown. Common sizes 1-4440th, 1-4210th, and 1-4268th. Several also of 1-4000th. Extreme diameters 1-5333rd and 1-3428th of an inch. Blood from a vein of the ear.

The size therefore of the blood corpuscles of these larger species of the genus Felis is very nearly alike*. In some of the smaller species, as the Cat (27.), Serval (28.), and Lynx (30.), the disks have much the same diameter, as I infer from frequent examinations. Mr. Siddall too, before he was acquainted with the result of my observations, came to the same conclusion, from several trials with the blood of the cat, as compared with one specimen of that of the tiger. In some blood obtained during life from the femoral vein and from the femoral artery of a cat, about a third-grown, the disks most commonly presented the following diameters, 1-4365th and 1-4752nd of an inch; and there were several 1-4000th, the thickness of the edges of the disks being 1-16,000th. The blood was examined quickly after it was obtained, and no appreciable difference was seen between the arterial and venous corpuscles. They are certainly but very slightly smaller than in the tiger.

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* See Dublin Medical Press, No. 52.

99. A Kangaroo, (*Macropus Eugenii?*) a female, 1-3554th, 1-3432nd, and 1-3200th, a common diameter of the disks, and 1-4000th and 1-3000th rather frequent sizes. The edges of the corpuscles generally from 1-12,000th to 1-10,000th of an inch thick. Blood from a prick of the tail.

100. The Coypu, (*Myopotamus Coypus,*) an adult. Most common diameters of corpuscles 1-3500th and 1-3200th of an inch. Small disks, 1-4000th; large, 1-2666th, in the dry state. Thickness of the edges of the corpuscles 1-12000th to 1-9600th. In the serum, the corpuscles were more variable in size, 1-4572nd and 1-3000th of an inch being very frequent. Blood from a prick near the buttocks.

101. Gray Squirrel, (*Sciurus cinereus,*) adult male. Common diameters 1-4266th, 1-4000th, 1-3840th, and 1-3600th. Extreme sizes 1-6000th and 1-3000th. Magnitude of corpuscles very irregular; a few not more than 1-6400th. Blood from a prick of the nose for the first examination, and from the ear a few days subsequently for another trial.

102. Capistrated Squirrel, (*Sciurus capistratus,*) adult male. Common diameters 1-4000th, 1-3790th, and 1-3693rd. Extreme sizes 1-5333rd and 1-3000th. A few of the very small corpuscles less than 1-6400th of an inch in diameter. Blood from a prick of the upper lip.

103. Black Squirrel, (*Sciurus niger,*) an adult. All the following sizes frequent, 1-3600th, 1-3692nd, 1-3790th, and 1-3840th. Extreme diameters 1-5333rd and 1-3000th. A few of the very small corpuscles were seen.

The size of the corpuscles, as far as I have observed, is very irregular in the genus Sciurus. There are circular particles, though not in large numbers, yet very remarkable for their regularity and diminutive size in regard to the common blood disks. For this reason the very small corpuscles should be further examined. I have certainly seldom seen them so remarkably in any other genus, though I think they appeared in the blood of the female Sloth Bear. In the Palm Squirrel (72.) there were several about 1-7000th of an inch.

104. Wild Boar, (*Sus Scrofa,*) from Asia, male, nearly full-grown. 1-4266th, 1-4365th, and 1-4000th, most frequent

diameters of the disks. Extreme sizes 1-1533rd and 1-3555th. Blood from a prick of the nose.

105. Collared Peccari, (*Dicotyles torquatus*,) from Mexico. Full-grown female. 1-4173rd, 1-4500th, 1-4572nd, and 1-4800th very common diameters of disks. Extreme sizes 1-6000th and 1-3555th. Much more irregular in size than in the Wild Boar, and from two examinations appearing to be smaller. Blood from a prick of the upper lip.

106. Zebra, (*Equus Burchellii*,) full-grown female. Average-sized disks 1-4500th; and 1-4800th, 1-4365th and 1-4000th, not uncommon. Extreme sizes 1-5800th and 1-3368th. Blood from a prick of the nose.

107. Dshikketai or Wild Ass, (*Equus Hemionus*), a female, as large as a common Ass. The most frequent sizes of corpuscles 1-4572nd and 1-4800th. Several as large as 1-4000th. Extreme diameters 1-5800th and 1-3555th of an inch. Blood from a prick of the nose.

With reference to the blood of the Horse (see No. 34 in the former paper) Mr. Siddall obtained the following measurements of the common-sized corpuscles from a cart stallion, aged 12, suddenly killed: 1-4360th, 1-4208th, and 1-4362nd. The blood, from a vein of the testicle, examined soon after the death of the animal.

108. Axis Deer, (*Cervus Axis*,) adult male. Most frequent sizes 1-4924th and 1-5333rd of an inch. Extreme diameters 1-6000th and 1-4365th. Altogether very irregular in size. Blood from a vein of the ear.

109. A Deer, (*Cervus macrourus?*). Most frequent diameters 1-5142nd and 1-5333rd. Extreme sizes 1-6400th and 1-4000th. An adult male and female; the blood from the renal vein of the former, and from the ear of the latter.

These animals were shipped at Honduras, and brought home with the Mexican Deer.

110. Reeves's Muntjac, (*Cervus Reevesii*, Ogilby). The corpuscles belonging to the same class as those of the Porcine and Mexican Deer. They will be all fully described together on a future occasion.

111. Sing Sing Antelope, (*Antilope Sing Sing*,) adult female. Corpuscles very variable from 1-6000th to 1-4000th of an inch; 1-4800th was very common, and 1-5333rd common. Blood from a prick of the nose.

112. Nyl-ghau, (*Antilope picta*,) a young male, hardly half-grown. 1-4924th, 1-4800th and 1-4572nd, all common diameters. Extreme sizes 1-6000th and 1-4365th of an inch. Blood from a vein of the ear.

113. Cervine Antelope, (*Antilope bubalis*,) adult male. Cor-

puscles remarkably variable in size, and differing much whether examined dry or in their serum. In the former state 1-5333rd and 1-6000th most common diameters, and extreme sizes 1-6400th and 1-4562nd. In the serum 1-6856th the most frequent size. Blood obtained for the first examination from a prick of the nose, and for the second some weeks afterwards from an incision of the ear. The animal was diseased.

114. Buffalo, from Manilla, (*Bos Bubalus*,) adult female, 1-5142nd, 1-4800th, and 1-4500th common diameters. Extreme sizes 1-5333rd and 1-3600th. Average thickness of the edges of the disks 1-14,000th of an inch. Blood from a vein of the ear.

115. Cape Buffalo, (*Bos Caffre*,) full-grown male. 1-5142nd and 1-4800th, most frequent sizes in the dried specimens. Extreme diameters 1-6000th and 1-3554th. The corpuscles in their serum were commonly 1-5333rd, or even as small as 1-6000th of an inch. Blood from a vein of the ear.

From two trials it appeared that disks were slightly smaller than in *Bos Bubalus*.

Napu Musk Deer (49.) An adult female having lately died at the Zoological Gardens, I availed myself of an opportunity of examining some blood from the different cavities of the heart, as well as from the cava, portal, and mesenteric veins. The result fully confirms the accuracy of my former observations*, especially that the blood corpuscles of the Musk Deer are smaller than any previously described in the mammalia. In relation to the parts from which the blood was obtained, there was no appreciable difference in the disks. The average diameter of those procured from the dead animal was 1-13,400th of an inch.

In an animal with blood corpuscles so remarkably minute, it was interesting to ascertain the comparative magnitude of the lymph globules. The latter were therefore carefully examined with this view; and their size, appearance, and chemical characters found to be identical with the lymph globules of many mammals with large blood particles. The lymph granules in the Napu Musk Deer varied in diameter from 1-5000th to 1-3500th of an inch. Hewson inferred from his observations that there was a relation between the particles of the lymph and blood in the same animal, and a difference in the size and shape of the former in different animals.

I have recently examined the blood of the Vicugna (37.), and compared it again with that of the Dromedary (36.). In the former the following measurements of the corpuscles were taken. Long diameters, 1-4000th very common, many 1-3555th, and a few, not commonly seen, 1-2666th. Several considerably shorter than 1-4000th of an inch, for instance, 1-5333rd; and even 1-6000th very rarely. Short diameters most frequently 1-6400th and 1-7110th. The shorter corpuscles are generally broader in proportion than the others, some nearly circular, but in this observation none perfectly so. Mr. Siddall's measurements agree as nearly as possible with mine, and the corpuscles in the Vicugna appear to be a little smaller than those of the Dromedary. Though taken from a vein of the ear, the blood was of a bright brick-red colour, as it appeared from the puncture. It would be singular if the venous blood of any of the mammals with oval particles should not have the usual dark colour.

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* Dublin Medical Press, Nov. 27, 1839. L. and E. Phil. Mag. Dec. 1839, and Jan. 1840.

The Rhinoceros (52, p. 32.) 1-2554th is a misprint for 1-3554th.

The Red American Fox (24) is the *Canis fulvus*, not a variety of C. Vulpes.

XIX. *On Galvanic Series formed of Zinc and Inactive Iron.*
By Mr. THOMAS HAWKINS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ONE of Professor Schœnbein's experiments described at p. 429 of vol. x. of the L. & E. Phil. Mag., by which he obtained an electric current of high intensity with wires "having one of their ends coated with peroxide of lead, and each end put into a separate vessel filled with nitric acid, a hundred times diluted," interested me so much, that I was led, at the time of its publication, to make some experiments, with the view of establishing the possibility of employing iron in the *peculiar condition* as the negative metal in voltaic batteries. After some failures I succeeded in forming a galvanic circle with a fine iron wire put into nitric acid of sp. gr. 1·5 contained in a tube of plaster of Paris, and associated with amalgamated zinc in diluted sulphuric acid, which electrolyzed water; and two of such circles, in series, liberated the gases rapidly, continuing in action for seven hours, when sudden effervescence of the acid occurred and the wires were dissolved. Efficient circles, but of less permanency, were constructed of iron wires alone, having one of their ends in the porous cells of nitric acid, the other in diluted sulphuric or even very diluted nitric acid. The liability of the iron to be suddenly attacked by the nitric acid was prevented, so far as my experience went, by dissolving mercury in the acid, in accordance with the Professor's observation of the effect of nitrate of mercury in preserving the inactive state; but it was also necessary to protect the wires at and above the surface of the acid from the corroding action of its fumes by a coating of wax or glass. By these means the inactive condition of the iron was maintained; but another obstacle then arose in the crystallization of nitrate of mercury, by which the cells after twenty or thirty hours' use were generally broken; and this could not be surmounted but at the sacrifice of some power, by diluting the acid, which the presence of the nitrate permitted, and the substitution for the cells of pipe-clay of others made of wood.

I cannot give comparative results obtained with iron and platina batteries, but I may mention that with an arrangement of six cylinders of sheet iron, each containing thirty-six square inches in strong nitric acid, without mercury, and associated with zinc plates of half their size, a current was evolved which for several hours ignited charcoal, or the whole of a strip of platina six inches in length by one-eighth of an inch in width. The construction of this battery was imperfect in several respects, particularly in the porous vessels being much too thick; there is, therefore, reason to suppose that an equal power might have been obtained from a less number of cells. As some progress in advance of the Professor's experiment in which the current ceased with the solution of the film of peroxide of lead, this communication may possibly be deemed worthy of being recorded: but the importance of my results in relation to the proposed object of the experiments is, I think, materially affected by the discovery of Mr. Cooper, as given in your last Number, of the application of charcoal and other forms of carbon as a substitute for platina in the voltaic arrangement of Mr. Grove.

I have the honour to be, Gentlemen, &c.

52, King's Road, Brighton, THOMAS HAWKINS.
Jan. 10th, 1840.

XX. *On Lightning Conductors, and the Effects of Lightning on Her Majesty's Ship Rodney and certain other Ships of the British Navy: being a further examination of* Mr. Sturgeon's *Memoir on Marine Lightning Conductors. By* W. SNOW HARRIS, *Esq.*, *F.R.S.*, *&c.*

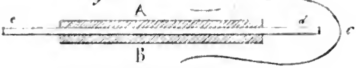
[Illustrated by Plate I.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

1. IN my former communication (L. and E. Phil. Mag. vol. xiv. p. 461.) I considered the nature of a well-known

Fig 1.



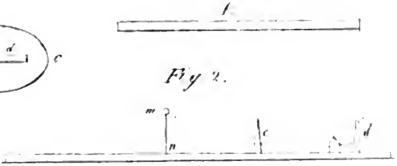


Fig 2.

Fig 7.

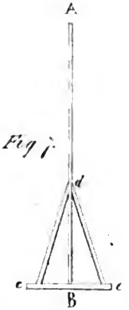


Fig. 3.

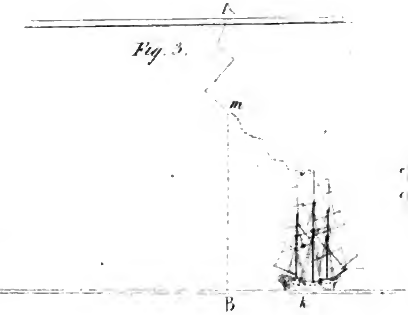


Fig 8.

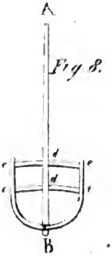


Fig. 4.

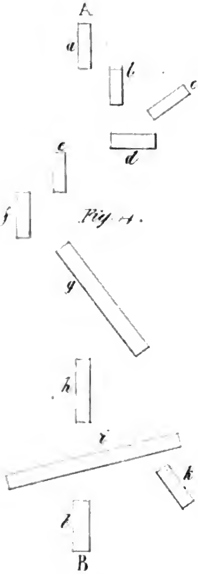


Fig. 5.

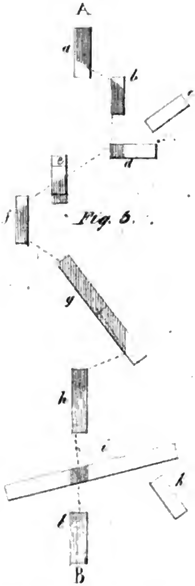
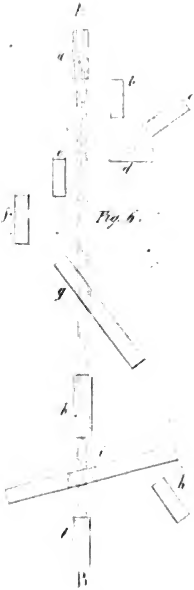


Fig. 6.



Effects of Lightning on Ships.

phænomenon in electricity, termed by Cavallo, Priestley, and others the lateral explosion, and showed that it did not apply to the state of a metallic rod in the act of transmitting a vanishing electrical accumulation between two opposed electrified surfaces, as insisted on by Mr. Sturgeon in a recent number of his Annals of Electricity. I will now proceed to examine the general character and effect of ordinary electrical discharges, whether produced on the great scale of nature, or artificially, with a view of further showing, that such lateral explosions do not occur at the instant of the passing of a shock of lightning through a metallic conductor, as also with a view of meeting certain other objections which have been advanced at different times to the use of lightning rods in ships.

2. I should not have felt myself called upon to notice further Mr. Sturgeon's memoir, did I not consider the statements it contains, although superficial and inconclusive, likely to mislead the public upon many important points connected with the effectual protection of shipping against the destructive effects of lightning, and convey false views of the nature of electrical action. Under these impressions I have little hesitation in noticing what he has advanced under the following heads:—

1st. Examination of the observed effects produced on shipping by lightning.

2nd. A comparison of the observed effects of lightning and the probable effects which lightning would produce by the application of Mr. Harris's conductors to shipping.

3. The first contains an excellent, and I have no doubt, an accurate statement, by an intelligent officer of the Rodney, of the destructive effects of lightning lately experienced in that ship, together with notices of two cases in which ships fitted with my conductors were struck by lightning without any attendant ill consequence. In the second, it is the author's object to prove, from the effects of lightning in the Rodney, that my system is inadmissible; since the discharge of lightning, he observes, which struck the Rodney, "would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder."

Considering the boldness of this assertion, and the high pretension of the memoir, we should expect, on examining the author's researches, to find him in possession of a copious induction of facts from well-authenticated cases of damage by lightning on ship-board, illustrating clearly the views he so strenuously insists on,—cases in which continuous or other metallic conductors have been from any cause placed along the masts or rigging, and in which the electric agency found its way through the hull to the sea. We should further expect from him, something like an examination of the general nature and effects of electrical discharges, since it is clear, before any accurate estimate can be arrived at, of the relative quantity of electricity likely to be discharged from a thunder-cloud, and the probable effects on metallic rods, or other conductors set up with a view of directing it in any given course, such information is quite indispensable.

4. Now it is to be particularly observed, that Mr. Sturgeon's memoir is really deficient in such information; a few clumsy experiments in illustration of a well-known fact in electricity, deceptively associated, by means of a vague hypothesis, with some of the ordinary effects of lightning, on a ship *not having* any regular conductor, and with some every-day phænomena of the electrical kite, is virtually the amount of all that the author has advanced, under the imposing title of "Theoretical and Experimental Researches."

5. In illustration of the careless way in which he meets this question, it may not be out of place to notice the following specimen of his inductive philosophy,—being the very outset of the comparison he has proposed, of the observed effects of lightning, and the probable effects on my conductors*.

In the account given of the damage recently sustained by H.M. Ship Rodney, it appears, that the shock of lightning which shivered the top-gallant-mast, damaged the top-mast, &c., &c., fell on a small brass sheave in the truck for signal halliards, and *slightly* fused it. This sheave weighed about 4 ounces; it was only about an inch and a half diameter, hollowed except at the centre and rim, where it was somewhere about half of an inch in thickness. The lightning also fell on the copper funnel for top-gallant rigging, being a hollow cylinder of 16 inches in length, 10 inches in diameter, and not quite a quarter of an inch thick. This funnel was not anywhere fused. It fell also on other metallic masses, such as the iron-bound tie-block, on the top-sail-yard, &c., &c., the iron hoops of the mast, &c., on which no calorific effect was apparent.

6. Now we have here something like evidence what was really the *actual power* of the charge. We see, for example, that it *did not* fuse a copper funnel, 16 inches long, 10 inches in diameter, and about $\frac{1}{5}$th of an inch thick. In the face of which fact Mr. Sturgeon insists, that had the charge fallen on

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* Sturgeon's Memoir, sec. 204.

my conductor, the thickest part of it would have become red-hot. His reasoning, in fact, amounts to this; an explosion of lightning having *slightly* fused a small brass sheave, weighing 4 ounces, and having failed to fuse a short copper funnel, therefore had it fallen on a rod of copper of one inch in diameter, and 200 feet long*, that rod would have been rendered *red*-hot.

This, it must be allowed, is a somewhat amusing kind of special pleading, quite unprecedented, I believe, in any paper on science.

7. The author wishes to strengthen his deduction, such as it is, by adverting in a foot-note to the case of a small brig struck by lightning, in which some part of a chain conductor is *supposed* to have been fused; how much is not known, "as the lower part fell overboard." The statement is given without any quoted authority, and is altogether deficient in the very information most required, viz. the *size of the chain, and how much of it was fused.* Let us, however, take it upon the author's own ground, and suppose the conductor to have been such as is commonly used in the merchant service,—that is to say, links of iron wire of about one-fourth of an inch in diameter, united by rings, a kind of conductor very easily disjointed and fused at the points of junction by lightning;—the reasoning then stands thus: because a shock of lightning fused and disjointed some unknown portion of a lightning chain in a merchant brig, therefore the same shock, had it fallen on a solid copper rod of one inch in diameter and 100 feet long, would have rendered that rod *red*-hot.

7. The fallacy and entire worthlessness of such reasoning, seems not altogether to have escaped Mr. Sturgeon's notice, as appears by his amplification of the above effects; thus on entering upon the comparison of the effects of lightning, he resorts to a sort of wholesale dealing, and leads the reader to conclude that the *entire* sheave in the Rodney and *all* the brigs' conductor underwent fusion. But even if it were so, no such conclusion as that above mentioned is admissible†, especially in reference to a continuous and massive conductor termina-

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* This is the equivalent of my conductor on the main-mast of such a ship as the Rodney, taking it at its least value.

† "Were there no other data than those of the *fusion* of *the* metallic sheave in the Rodney and the fusion of *the chain-conductor* in the brig Jane," &c. &c.

"The impressions which these facts convey to the mind are too definite to be easily misunderstood; they clearly imply that either of the discharges which struck the Rodney or Jane would have rendered the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder," &c. &c.—Sturgeon's Memoir, sec. 204.

ting in a point, and equalizing with inconceivable rapidity the disturbed electrical state of the sea and clouds.

8. The manifest deficiency of sound practical information in Mr. Sturgeon's memoir, imposes upon me the necessity of adverting to the general character and operation of common electrical discharges, whether produced by artificial means or on the great scale of nature. In doing this I have no desire to excuse myself, in case I should not have written clearly and explicitly on the subject, since in no department of physics is the field of observation so fertile, and the path of experiment so sure and easy. We have before us the experience of nearly a century, during which time lightning-rods have been employed; a great number of instances have occurred of shocks of lightning falling on ships under a variety of different circumstances, in some cases where lightning conductors have been present, in others where absent; in many instances where ships have been near each other and exposed to the same storm, some *having* conductors, others *not*. The general laws of the discharge are traceable in them all, and the effects on metallic bodies distinctly shown. On the other hand, we can on a minor scale, imitate successfully the great operations of nature, and examine experimentally every possible contingency attendant on the operation of a shock of lightning in a ship. It is our own fault, therefore, if we do not treat the subject scientifically, and arrive at complete practical solutions of such questions as these: Is a lightning conductor desirable in a ship? Will it cause by attraction a shock of lightning to fall on a ship when otherwise such would not take place? If so, can it cause damage by its inability to get rid of the lightning which falls on it? What is the *best* form and dimensions of a lightning conductor for a ship? What is the greatest probable force of lightning to which it may become exposed? Is it liable to cause damage by any lateral operation of the charge passing through it? I say, if such questions as these cannot now be reasonably determined they probably never can; and, therefore, any one who writes or reasons obscurely about them, and without due regard to a good induction of facts, can have no claim to be considered as a sound reasoner in experimental science; for, as beautifully observed by Lord Bacon, "Man, who is the servant of nature, can act and understand no further than he has, either in operation or in contemplation, observed of the method and order of nature." Under these impressions I proceed to examine the general character and effects of electrical discharges as exhibited artificially, and on the great scale of nature.

9. Although some theoretical differences may have arisen

concerning the precise nature of electricity, yet the following explanation runs sufficiently parallel with facts to entitle it to our confidence, and put us in possession of one of the great advantages of *every* theory, viz. a classification and connexion of observed effects; the province of human knowledge, being, as justly observed by a most intellectual and accomplished writer, "to observe facts, and trace what their relations are*."

General principles:—

10. There is an invisible agency in the material world intimately associated with common matter, termed electricity.

11. Lightning, thunder, and a variety of analogous phænomena of a minor kind, artificially produced, result from discharges of this agency between bodies differently affected by it.

12. In every case of electrical discharge there are two surfaces of action; one existing on some substance eager to throw off redundant electricity, being, according to Dr. Franklin, overcharged with it; the other existing in some other substance eager to receive electricity, being, according to the same philosopher, deficient of it, or undercharged.

13. Two opposed bodies, when placed in these opposite electrical states, have a sort of exclusive action on each other, either *directly* through any intervening substance, whether a conductor of the electrical principle or not, or otherwise *indirectly* through any lateral circuit.

Thus two metallic surfaces A B (fig. 1.) pasted on the opposite sides of a square of glass *c d*, have, when the square is said to be charged, an exclusive action on each other, either through the intervening glass, or otherwise through any conductor, A o B, connecting them.

Now we have only to suppose these planes placed further apart, as in fig. 2, to have a discharging conductor, *m n*, of greater or less extent between them, to be greatly increased in size, to be separated by air instead of glass, and to consist of free vapour or water, and we have a pretty faithful representation of the conditions, under which a discharge of lightning takes place, when passing partly through the air, and partly through a discharging conductor, *m n*, or any other body, *c d*, placed on the plane B†.

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* Abercrombie on the Intellectual Powers.

† The thickness of the intervening air, and the amount of free electricity in the clouds, has led Professor Henry to question in some measure, the perfect analogy of a discharge of lightning, with that of a Leyden jar; but I think upon *mature* consideration this circumstance will not be found in any way subversive of the general principle. Thus whether electricity

14. Any continuous metallic rod or other body, *m n* (fig. 2.), connected with the lower plane, must be considered merely as a passive way of access for the charge so far as it goes; the electrical agency being observed to seize upon substances best adapted and in a position to facilitate its progress, or otherwise to fall with destructive effect upon such as resist it.

15. It is easy to perceive here, that the presence of a conducting rod, *m n* (fig. 2), or other conducting body, has nothing whatever to do with the great natural action set up between the planes A B. It is in fact to be considered merely as a point in one of them. The original accumulation of electricity and subsequent discharge, would necessarily go on whether such a rod were present or not, as is completely shown by experience. When present, its operation is confined to the transmission, so far as it extends, of that portion of the charge which happens to fall upon it; and since it is quite impossible to avoid the presence of conducting bodies in the construction of ships, it is the more important to understand clearly in what way damage by lightning occurs to the general mass, and how it may be best avoided.

16. When discharges of lightning fall upon a ship in the way above stated, as being a heterogeneous mass fortuitously placed between the charged surfaces A B (fig. 3.), the course of the discharge is always determined through a certain line or lines, which upon the whole least resist its progress. The interposed air between the ship and the clouds first gives way in some particular point, probably the weakest,—suppose at A, fig. 3;—the electrical agency then meeting with continued resistance from the non-conducting particles of air, is often turned into a tortuous course. Suppose it arrives in this way at some point, *m*, in the vicinity of a ship at *k*, the

be accumulated on thick glass or on thin, the result is the same; it is merely the intensity as indicated by the electrometer which changes.

Now the term free electricity, applies to the greater or lesser influence of the opposed coating in respect of other bodies. In the case of the opposed surfaces of the clouds and earth, all the charge is necessarily free electricity, since there exists no other point upon which it can tend to discharge. In the same way the electricity of the jar, when the coatings are very near, is nearly all redundant, or free electricity, in respect of the action between them, although latent in respect of other bodies. Hence with a moderate accumulation, the electrometer exhibits but a small intensity, if any. The only difference at the time of the discharge, is in the position of the discharging circuit, which in the case of the clouds and sea, is directly in the interval of separation; and as we find the principle of induction always active in cases of lightning, the thickness of the stratum has evidently no influence on the conditions of the accumulation, especially when we consider the great extent of the opposed surfaces, which may possibly be 20,000 or more square acres. Dr. Faraday has shown that no distance excludes the inductive action.

question whether it would strike upon the mast at *y* would be determined by the resistance in the direction of *m y k*, as compared with that in any other direction *m*, B; whether, in fact, it would be easier to break down the remaining air in the direction M B, or otherwise the air in the direction *m y*, supposing the ship's mast to facilitate the progress in that direction.

17. Let the charge however strike in the direction *m y*, and so fall upon the mast,—then in proceeding to its ultimate destination, viz. the plane of the sea B, its course is still determined by the same general principles; that is to say, it seizes upon all those bodies which tend to assist its progress, and which at the same time happen to be placed in certain relative positions, *and upon no others*, falling with destructive effect upon intervening bad conductors, and exhibiting in non-conducting intervals all the effects of a powerful expansive force. If we examine carefully the course of discharges of lightning on ships in some hundred instances in which damage has ensued, we shall find this effect invariable. The damage has always occurred where good conductors cease to be continued, and the destructive consequences most apparent are those usually produced by expansion. The calorific effects, except as depending on this cause, are really inconsiderable; there are comparatively few instances in which metallic bodies have been fused, and no instance in which a bolt or chain of any considerable magnitude has been even much heated.

The following experimental and natural illustrations of these facts will be found conclusive and interesting.

Exp. 1. Lay some small detached pieces of leaf-gold *a*, *b*, *c*, *d*, &c. on a piece of paper, as represented in fig. 4; pass a dense shock of electricity over these, from the commencement at A to the termination at B, so as to destroy the gold; the line which the discharge has taken will be thus shown by the blackened parts; the result will be as in fig. 5, in which we perceive the course of the discharge has been in the dotted line *a*, *b*, *d*, *e*, *f*, *g*, *h*, *l*, being the least resisting line; and it is particularly worthy of remark, that not only are the pieces *c*, *k* untouched, being from their positions of no use in facilitating the progress of the charge, but even portions of other pieces, which have so operated, are left perfect, as in the transverse piece *i* and portions of *a*, *b*, *d*, *e*, and *f*; so little is there any tendency to a lateral discharge, even up to the point of dispersion of the metallic circuit in which the charge has proceeded; indeed, so completely is the effect confined to the line of least resistance, that percussion powder may be placed with impunity in the interval between the portions *c*, *d*.

Now the separate pieces of leaf-gold thus placed, may be taken to represent detached conducting masses fortuitously placed along the mast and hull of a ship.

Exp. 2. Let a thin continuous line, *m*, *n*, be passed through the separated pieces, and a dense accumulation discharged over the whole, as in the preceding case. The effect will be as represented in fig. 6.: the discharge will be confined to the line of least resistance; and we may perceive in this, as in the former case, that those pieces, or parts of pieces, out of the track of the discharge, are not affected; thus a part only of the piece *g* is destroyed, also of the piece *i*, whilst other pieces, *b*, *d*, *e*, *f*, *l*, which in the former case, where the continuous line, *a*, *b*, was not present, were blackened by the discharge, remain here perfect.

Exp. 3. If the continuous line A, B (figs. 7, 8) be assisted by other comparatively short collateral branches, as *d e*, *d c*, having one common connexion at B, then a discharge which would destroy the line A, B, will divide upon these auxiliary lines, and the part *d*, B will either escape, or the whole will suffer together.

Exp. 4. Pass a discharge over a strip of gold-leaf, as A, fig. 2; every part of it, as indicated by the last experiment, will participate in the shock; and if it be of uniform density and thickness it will be everywhere equally affected, so that one portion will not be destroyed without the whole. This result will be readily distinguished from that represented at *d* and *i*, fig. 5, where the masses lie across the track of the discharge.

The diagrams here referred to, are copied from the actual effects of the electrical discharge in the way above mentioned.

18. These experiments are instructive. They evidently prove, that an electrical explosion will not leave a good conductor, constituting an efficient line of action, to fall upon bodies out of that line. Mr. Sturgeon's assertion that a conductor on a ship's mast would operate on the magazine is therefore quite unwarranted. Besides, we have many instances of the masts having been shivered by lightning into the step, whilst acting as partial conductors, without any such consequence; as happened in the Mignonne in the West Indies, the Thetis at Rio, the London, Gibraltar, Goliath, and many others. Instead, therefore, of a conductor on the mast being dangerous, it is absolutely requisite as a source of safety to the ship, by confining the discharge to a given line and leading it to the sea.

19. It was from a careful consideration of the common effects of lightning, and from such experimental facts as those above mentioned, that I was led to suggest the propriety of

fitting continuous conductors of lightning of great capacity in the masts of ships, linking them by efficient communications, together with the principal detached metallic bodies in the hull, into one general continuous system, and finally connecting the whole with the sea. These conductors consist of two laminæ of copper-sheet, varying from one inch and a half to five inches wide, and being together nearly one-fourth of an inch thick; they are inlaid so as to be fair with the surface of the mast, and form a series of shut-joints; they are otherwise so constructed as to present an uninterrupted line of action from the highest point to the sea. The method has been partially used in the British navy for several years, and has been proved in every way efficient. In no case has any of the vessels fitted with them received the slightest damage, although frequently exposed to severe thunderstorms, and in some instances actually struck by heavy discharges similar to that which fell on the Rodney in December, 1838*.

20. If we consider attentively the effects of this shock, we shall find them in complete accordance with the principles just stated. The attendant phænomena were of the simplest kind, and such as have always occurred in cases of ships struck by lightning not having a continuous conductor: e.g. the electrical discharge, in forcing its way between the sea and clouds, over resisting intervals, and between discontinuous metallic masses, was productive of a violent expansive effect in these intervals; causing at the same time a considerable evolution of heat. There was really nothing particularly remarkable in this instance; the course of the discharge was a very simple affair, being, according to the law of electrical action just exemplified (Exp. 2), in the line or lines of least resistance from the highest point to the sea: thus the course of the discharge was, as represented in the annexed diagram, along the masts and rigging, upon the general mass of the hull and sea. The vane-spindle *a*, upon which the accumulation was first concentrated, was of course severely dealt with. From this, being probably assisted by the moisture on the surface of the wood, it glanced over the royal pole to the head of the top-gallant mast at *b*, where it found intermediate metallic assistance in the copper funnel for the top-gallant rigging: from this, the resistance in the mass of the wood appears to have been less than that on its surface, probably from the long interval of air between the funnel and conducting bodies about the cap below, the mast was therefore split open as far as the cap at *c*. Here again it was enabled to strike over the surface of the

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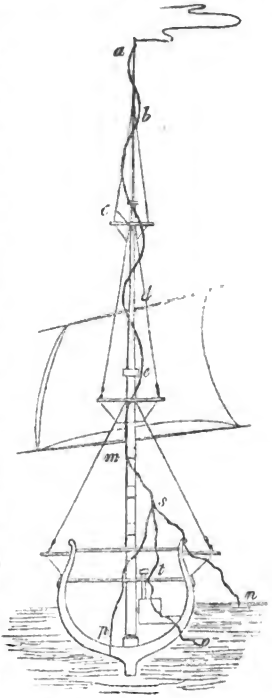
* See a letter in the Nautical Magazine for December 1839, by Lieut. Sullivan, R.N., who witnessed these effects.

mast upon the metals about the parrel of the top-sail-yard at *d*, where the accumulation became again concentrated, producing a powerful expansion and heating effect so far as the lower cap at *e*; and thus it passed along *per saltum* over the lower mast *m*, from one metallic mass to another, until within a striking distance *s* of the sea and hull, it divided upon the hull and sea in convenient directions *s n*, *s o*, *s p*. In this course, as indicated by the waving black line *a*, *b*, *c*, *d*, &c., it evidently sought assistance from all the conducting matter it could seize upon; such as the wet ropes, the copper funnel for top-gallant rigging at *b*, the iron work and other bodies about the topmast cap at *c*, as also the men in the top-gallant crosstrees at *c*. The charge evidently divided upon them in proportion to the assistance each could afford as a small auxiliary circuit, as in Exp. 3; the men nearest the mast would be necessarily in the more direct course of the discharge, the others would be more or less so according to their respective positions; that these poor fellows who were killed suffered in this way as being conductors to parts of the charge is evident from the appearance of the bodies. Mr. Sturgeon calls especial attention to the circumstance of the men being thrown in opposite directions, and thinks it remarkable: but how could it be otherwise? the intervening air being caused to expand violently from a central point, would necessarily operate as a central force; surely there is nothing very new in this. About the parrel of the topsail-yard at *d*, we should expect again powerful effects; for here again the charge became concentrated, and set the sail, &c., on fire. This is quite in accordance with the known laws of electrical



action; thus we find the points of ingress and egress of an artificial charge, when caused to fall on a slip of gold leaf or other matter, are always those in which the most powerful effect arises; and when we desire to fire inflammable matter by electricity we place it directly between detached metallic points.

21. The circumstance of the lightning striking over portions of the wet mast without damage, is precisely the same effect as observed in certain cases of artificial electrical discharges. Thus a very slight film of moisture will allow a jar intensely charged to discharge a luminous ball over a long strip of glass. Dr. Franklin found he could destroy a dry rat by an electrical shock when he failed to hurt a wet one. If we continue to follow the discharge we find similar expansive and destructive effects; such as the bursting of the hoops on the mast, &c., &c., which will sometimes occur and sometimes not.

22. There is really nothing in all this to call for especial remark, except we may observe, as shown by the experiments already described, that if a good capacious conductor had been incorporated with the mast from the truck to the metallic masses in the hull and to the sea, then these *expansive* and destructive effects could not possibly have occurred; since the interrupted circuit would have been avoided, and the intense electrical action have vanished, or nearly so, at the mast-head, for it would have no longer been driven to force its way in a dense explosive form to the hull and sea: of this we have the most complete evidence from experience, particularly in the cases of the ships struck by lightning having such conductors as those just alluded to, curiously enough quoted by Mr. Sturgeon as evidence to the contrary. It seems a strange way of disproving a fact to quote those who, having been eye witnesses, insist upon its truth. That the electric matter finally distributed itself upon the hull as *well* as on the sea, is evident from the circumstance of the casing of Hearle's pump at *t*, which led through the side under water being shivered; from the vivid electrical sparks below, and from the usual smell of sulphur in the well, and appearance of smoke in the orlop-deck.

23. The interrupted circuit therefore to be traced here, is first from the vase-spindle to the copper funnel of top-gallant rigging; 2nd, from this to the conducting bodies at the heel of the top-gallant mast; 3rd, thence to the metallic masses about the parrel of topsail-yard; 4th, between this and the metallic bodies about the head of lower mast; 5th, from this over the detached metallic bodies on lower mast; finally, from lower mast to the hull and sea. The effect of this shock of lightning appears to have been somewhat palliated by heavy rain.

Although Mr. Sturgeon has gone far out of his way to twist these phænomena into an accordance with certain theoretical views, and sets them up as being of an extraordinary kind, they are nevertheless of a very simple character, and are merely illustrative of a few well-known laws of electrical action.

[To be continued.]

XXI. *On a new Compound of Ferrocyanide of Potassium, with Cyanide of Mercury. By* Robert Kane, *M.D., M.R.I.A.*[*]

IT had frequently occurred to me to notice that, in the process for obtaining cyanide of mercury by the action of ferrocyanide of potassium on sulphate of mercury, it was necessary to observe accurately the equivalent proportions of these substances, in order to ensure success. If any ferrocyanide of potassium were present in excess, a corresponding deficiency in the quantity of the cyanide of mercury always occurred. I at last traced this circumstance to the fact, that the ferrocyanide of potassium in excess combines with the cyanide of mercury, to form a new substance so similar in appearance to the former of the two, as to be very easily confounded with it and thus rejected in the crystallizations.

This new salt is most easily prepared by dissolving together in a moderate quantity of water about one part of ferrocyanide of potassium in crystals with two of cyanide of mercury. On cooling, the new salt separates in the form of rhomboidal plates of a rich yellow colour, almost as deep as that of ferrocyanide of potassium. When heated, these crystals lose some water and become whitish and opake, then blacken and yield cyanogen and mercury; the usual products of the decomposition of ferrocyanide of potassium remaining behind.

With a protosalt of iron, a solution of this new compound yields Prussian blue, and indeed, every reagent which acts on either constituent gives its characteristic reaction with this new body.

For its analysis very simple methods were sufficient.

Forty grains dried at 300° Fahr. lost 2·31 of water or 5·78 per cent.

The remaining 37·69 grains were dissolved in water, and then decomposed by a stream of sulphuretted hydrogen. The sulphuret of mercury was collected and dried. It weighed

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[*] Communicated by the Author.

23·5 grains corresponding to 58·75 per cent. containing 50·13 mercury, equivalent to 63·05 of cyanide.

The liquor from which the mercury had been thus separated, was evaporated carefully to dryness, and the salt obtained was deprived of all water of crystallization by exposure to a temperature of 300°, until it ceased to lose weight. It then weighed 31·32. It was pure dry ferrocyanide of potassium.

A quantity of the new salt equal to 50·4 grains was ignited and icinerated; the residue then treated by muriatic acid, and the iron thrown down by ammonia added in excess. The oxide of iron weighed contained 2·23 of metallic iron, corresponding to 4·47 per cent.

The liquor after the separation of the iron was evaporated to dryness, and ignited; there remained chloride of potassium, equal to 12·3, containing 6·51 potassium or 12·91 per cent.

This new salt, therefore, contained

Mercury...............	50·13
Iron	4·47
Potassium	12·91
Water..................	5·78
Loss and cyanogen	26·71
	100·00

The relation is exactly such that the mercury employs half of the cyanogen to form cyanide of mercury, and the remaining half forms with the potassium and the iron common ferrocyanide of potassium: the result as calculated should be

3	atoms mercury	304·2	50·26
2	—— potassium	79·4	13·11
1	—— iron..................	28·0	4·62
6	—— cyanogen............	157·8	26·07
4	—— water...............	36·0	5·94
		605·4	100·00

The existence of this salt is of considerable practical importance, as it shows the necessity of avoiding any excess of ferrocyanide of potassium in preparing cyanide of mercury; an error into which, from motives of œconomy, the manufacturing operator would be peculiarly liable to fall.

23, Gloucester Street, Dublin, Dec. 23, 1839.

XXII. *On the Decomposition of the Neutral Sulphate of the Peroxide of Iron by boiling its Solution.* *By* Th. Scheerer.*

A CONCENTRATED solution of the neutral sulphate of iron may be heated to boiling without becoming opake, but if one part of this salt be dissolved in 40 parts of water, continued boiling precipitates traces of a basic salt, which increase so as to form a considerable precipitate the more the solution is diluted with water.

This salt is a combination of sulphuric acid, peroxide of iron and water, in the following proportions:

74·70 peroxide of iron.
12·57 sulphuric acid.
12·70 water.
———
99·97

Thus it consists of 6 atoms of the peroxide of iron, 2 atoms of sulphuric acid, and 9 atoms of water; the theoretical composition, therefore, would be

74·46
12·71
12·83——100·00

and accordingly the formula of Berzelius is $2\,\underline{\dddot{\mathrm{Fe}}}{}^{3}\,\dddot{\mathrm{S}} + 9\,\underline{\dot{\mathrm{H}}}$, or $2\,(\underline{\dddot{\mathrm{Fe}}}\,\dddot{\mathrm{S}} + 8\,\underline{\dddot{\mathrm{Fe}}}) + 27\,\underline{\dot{\mathrm{H}}}$.

According to the nomenclature of Berzelius, this salt may be called the eight-fold basic sulphate of the peroxide of iron. The oxygen of the water amounts to the half of that in the oxide, being quite analogous to the five-fold basic salt which is produced by the oxidation of a solution of sulphate of iron in the open air.

Dried at 212° Fahr., this salt forms a dark orange yellow powder, its colour being lighter in proportion as the solution is previously diluted, and the less it is boiled. It is not dissolved by water, but pretty readily by acids. At a temperature below a dull red heat, it loses its water and becomes of a dark brown colour. At a red heat the sulphuric acid is expelled and the peroxide of iron is left behind.

Experiments were made to discover how much of the sulphate of iron was decomposed by various degrees of dilution, which may be determined by the quantity of the salt precipitated. The results were as follows: dissolved in

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* Communicated by the Author, to whom we beg to return our kind thanks. The present extract forms the substance of two distinct articles, published in Poggendorff's *Annalen*, vol. xlii. p. 104, and vol. xliv. p. 453.—Edit.

Parts of water		Precipitated		Opacity commenced at
100 parts of water,		0·309	precipitated.	203°F.
200	— —	0·558	—	— — 158
400	— —	0·749	—	— — 137
800	— —	0·806	—	— — 122
1000	— —	0·912	—	— — 117.

If 1 part of the sulphate of iron was dissolved in 10·000 parts of water, the solution became opake even at the temperature of about 63° F. which was that of the water employed; but if it was afterwards heated to boiling, not a trace of iron could be detected in the solution filtered from the precipitate either by ammonia or by tincture of galls. The above quantities are mere approximations to accuracy, as in the first place, during the boiling of the solution, more or less water is evaporated, by which the degree of dilution is altered; 2ndly, the water which is condensed in the upper part of the tube again falling down, causes a momentary increased dilution, and consequently an increased quantity of the precipitate; and 3rdly, the boiling point is heightened the greater the quantity of salt of iron dissolved in the water. Nevertheless the result of the experiments is sufficiently accurate to allow of our establishing the following law with respect to the relative quantities of the peroxide of iron remaining in solution after boiling: *With a 200-fold and greater dilution, the quantities of the peroxide of iron remaining in solution are in inverse ratio to the dilution.* Indeed if these quantities are calculated acording to the above-mentioned proportions, we find approximatively that,

Dilution		Fraction	
With a 200-fold dilution,		$\frac{1}{2}$	of the peroxide of iron remains dissolved.
— 400	—	$\frac{1}{4}$	—
— 800	—	$\frac{1}{8}$	—
— 1000	—	$\frac{1}{10}$	—

This progression, however, is not generally exact, for instance by a 100-fold dilution $\frac{2}{3}$ds of the iron should remain dissolved. The law therefore is approximative only for the central members.

If a solution of the neutral sulphate of the peroxide of iron is mixed with a solution of the neutral sulphate of potash, the same basic salt is precipitated without any part of the potash entering into the combination. The properties of the solution of sulphate of iron now described may be employed to separate the peroxide of iron from some salts. The neutral sulphates of manganese, nickel and cobalt have no acid reaction upon litmus paper like the neutral sulphate of the peroxide of iron. If therefore one of these sulphates is mixed with the latter in solution, by far the greater portion of the iron may be precipitated by saturating, as nearly as possible, the solution with potash, and the remaining portion of iron may be thrown down by dilution and boiling. It remains only to be observed that no other acid than sulphuric acid must be present, and that the solution nearly saturated must be diluted with at least twice or three times its quantity of water.

This method may not only be employed with advantage in preparing pure oxide of cobalt, but also in analysis. When no error has occurred, the iron is perfectly free from cobalt, although the cobalt may sometimes contain a slight trace of iron.

During the preparation of pure oxide of cobalt from the roasted ores, arsenious or arsenic acid is constantly present. This need not first be separated by sulphuretted hydrogen, for it is precipitated, on treating it in the manner above described, as arseniate or arsenite of iron. It is, however, better in this case to add to the solution previously to saturation a quantity of the sulphate of the peroxide of iron, as otherwise there might not be a sufficient quantity of iron present to take up the whole of the arsenious acid, and then arseniate or arsenite of cobalt would also be thrown down.

XXIII. *A Method of determining by mere Inspection the derivatives from two Equations of any degree.* By J. J. SYLVESTER, *F.R.S. and R.A.S., Professor of Natural Philosophy in University College, London.*[*]

LET there be two equations, one of the nth, the other of the mth degree in x; let the coefficients of the first equation be $a_n \ a_{n-1} \ a_{n-2} \ \dots\dots\dots \ a_0$, *each* power of x having a coefficient attached to it, a_n belonging to x^n and a_0 to the constant term.

In like manner let

$b_m \ b_{m-1} \ \dots\dots\dots \ b_0$ be the coefficients of the second equation.

I begin with

A Rule for absolutely eliminating (x).

Form out of the (a) progression of coefficients (m) lines, and in like manner out of the (b) progression of coefficients form (n) lines in the following manner:

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[*] Communicated by the Author. See the December and January Numbers of this Magazine.

1. (a). Attach $(m-1)$ zeros all to the *right* of the terms in the (a) progression; next attach $(m-2)$ zeros to the right and carry over to the left; next attach $(m-3)$ zeros to the right and carry over 2 to the left. Proceed in like manner until all the $(m-1)$ zeros are carried over to the left and none remain on the right.

The (m) lines thus formed are to be written under one another.

1. (b) Proceed in like manner to form n lines out of the (b) progression by scattering $(n-1)$ zeros between the right and left.

2. If we write these (n) lines under the (m) lines last obtained, we shall have a solid square $(m+n)$ terms *deep* and $(m+n)$ terms *broad*.

3. Denote the lines of this square by arbitrary characters, which write down in vertical order and permute in every possible way, but separate the permutations that can be derived from one another by an even number of interchanges (effected between *contiguous* terms) from the rest; there will thus be half of one kind and half of another.

4. Now arrange the $(m+n)$ lines accordingly, so as to obtain $\frac{1}{2}\,(\overline{m+n}\,.\,\overline{m+n-1}\ \ldots\ldots\ 2.1)$ squares of one kind which shall be called positive squares, and an equal number of the opposite kind which shall be called negative.

Draw diagonals in the same direction in all the squares; multiply the coefficients that stand in any diagonal line together: take the sum of the diagonal products of the *positive* squares, and the sum of the diagonal products of the *negative* squares; the difference between these two sums is the prime derivative of the zero degree, i. e. is the result of elimination between the two given equations reduced to its ultimate state of simplicity, there will be no irrelevant factors to reject, and no terms which mutually destroy.

Example. To *eliminate* between

$$\begin{aligned} ax^2 + bx + c &= 0 \\ lx^2 + mx + n &= 0 \end{aligned}$$

I write down

$$\left.\begin{array}{cccc} a & b & c & o \\ o & a & b & c \\ l & m & n & o \\ o & l & m & n \end{array}\right\}\begin{array}{l} \ldots\ (1) \\ \ldots\ (2) \\ \ldots\ (3) \\ \ldots\ (4) \end{array}$$

I permute the four characters (1) (2) (3) (4) distinguishing them into positive and negative; thus I write together

Positive Permutations.

1	2	3	1	2	3	2	1	3	4	4	4
2	3	1	4	4	4	1	3	2	2	1	3
3	1	2	2	3	1	4	4	4	1	3	2
4	4	4	3	1	2	3	2	1	3	2	1

and again *Negative Permutations.*

1	2	3	4	4	4	2	1	3	2	1	3
2	3	1	1	2	3	4	4	4	1	3	2
4	4	4	2	3	1	1	3	2	3	2	1
3	1	2	3	1	2	3	2	1	4	4	4

I reject from the permutations of each species all those where 1 or 3 or both appear in the 4th place, and also those where 2 or 4 or both appear in the 1st place, for these will be presently seen to give rise to diagonal products which are zero.

The permutations remaining are

Positive effectual permutations.

1	3	3	1
2	1	4	3
3	2	1	4
4	4	2	2

Negative effectual permutations.

3	1	1	3
1	4	3	2
4	3	2	1
2	2	4	4

I now accordingly form four positive squares, which are

$$\begin{array}{cccc} a & b & c & o \\ o & a & b & c \\ l & m & n & o \\ o & l & m & n \end{array} \qquad \begin{array}{cccc} l & m & n & o \\ a & b & c & o \\ o & a & b & c \\ o & l & m & n \end{array} \qquad \begin{array}{cccc} l & m & n & o \\ o & l & m & n \\ a & b & c & a \\ o & a & b & c \end{array} \qquad \begin{array}{cccc} a & b & c & o \\ l & m & n & o \\ o & l & m & n \\ o & a & b & c \end{array}$$

Drawing diagonal lines from left to right, and taking the sum of the diagonal products, I obtain $a^2n^2 + lb^2n + l^2c^2 + am^2c$. Again, the four negative squares

$$\begin{array}{cccc} l & m & n & o \\ a & b & c & o \\ o & l & m & n \\ o & a & b & c \end{array} \qquad \begin{array}{cccc} a & b & c & o \\ o & l & m & n \\ l & m & n & o \\ o & a & b & c \end{array} \qquad \begin{array}{cccc} a & b & c & o \\ l & m & n & o \\ o & a & b & c \\ o & l & m & n \end{array} \qquad \begin{array}{cccc} l & m & n & o \\ o & a & b & c \\ a & b & c & o \\ o & l & m & n \end{array}$$

give as the sum of the diagonal products

$$l\,b\,m\,c + a\,l\,n\,c + a\,m\,b\,n + l\,a\,c\,n$$

be $\qquad i\,.\,e\;\; l\,b\,m\,c + a\,m\,b\,n + 2\,a\,c\,l\,n.$

Thus the result of eliminating between

$$\begin{aligned} a\,x^2 + b\,x + c &= 0 \\ l\,x^2 + m\,x + a &= 0 \end{aligned}$$

ought to, and is

$$a^2\,n^2 + l^2\,c^2 - 2\,a\,c\,ln + l\,b^2\,n + a\,m^2\,c - l\,b\,m\,c - a\,m\,b\,n = 0.$$

Rule for finding the prime derivative of the 1*st degree, which is of the form* $\mathrm{A}\,x - \mathrm{B}$.

1. Begin as before, only attach one zero less to each progression; we shall thus obtain *not* a square, but an oblong broader than it is deep, containing $(m+n-2)$ rows, and $(m+n-1)$ terms in each row: in a word, $(m+n-2)$ rows, and $(m+n-1)$ *columns.*

To find (A) reject the column at the extreme right, we thus recover a square arrangement $(m+n-2)$ terms, broad and deep.

Proceed with this new square as with the former one; the difference between the sums of the positive and negative diagonal products will give A.

To find B, do just the same thing, with the exception of striking off not the last column, but the last but *one.*

Rule for finding the prime derivative of any degree, say the rth, viz. $\mathrm{A}_r\,.\,x^r - \mathrm{A}_{r-1}\,x^{r-1} + \ldots\ldots\ldots \pm \mathrm{A}_0\,.$

Begin with adding zeros as before, but the number to be added to the (a) progression is $(m-r)$ and to the (b) progression $(n-r)$.

There will thus be formed an oblong containing $(m+n-2r)$ rows, and $(m+n-r)$ terms in each row, and therefore the same number of *columns.*

To find any coefficient as A_s, strike off all the last $(r+1)$ columns except that which is (s) places distant from the extreme right, and proceed with the resulting squares as before.

Through the well-known ingenuity and kindly proferred help of a distinguished friend, I trust to be able to get a machine made for working Sturm's theorem, and indeed all problems of derivation, after the method here expounded; on which subject I have a great deal more yet to say, than can be inferred from this or my preceding papers.

University College, London, Jan. 16, 1840.

[To be continued.]

XXVI. *New Researches on the true nature of the Boetian Contractions, especially with reference to the Explanation given by* M. Chasles. *By* J. O. Halliwell, *Esq., F.R.S., F.S.A., F.R.A.S., &c.*[*]

I HAVE the pleasure of placing before the readers of the Philosophical Magazine a complete explanation of the first tract in No. 343 of the Arundel MSS. in the British Museum; and that I have been able to accomplish this desideratum affords me the greater gratification, because in so doing I am fulfilling the wish of the patriarch of English Literature[†].

The manuscript referred to, sometimes called the *Mentz Manuscript*, is a small quarto of the twelfth century, on vellum; and the first tract, entitled *de Arte Numerandi*, consists of four leaves only, unfortunately being imperfect at the end. A fragment from the recto of the first folio is lithographed in the appendix to the *Rara Mathematica*, which serves to show the style of the manuscript and the forms and names of the contractions.

The treatise itself commences with an explanation of the increasing value of igin, andras, &c., in the different abacal compartments; in point of fact, a definition of abacal numeration dependent upon the principle of local decimal value. It is important to notice that, after this explanation, the compiler gives the usual definitions of *digiti* and *articuli*, clearly showing by that his comprehension of their future value. It is remarkable that everything stated is subservient to multiplication and division, no notice whatever being taken of addition, subtraction, duplation, or mediation;—a plain proof, if any were needed, that when the boundaries were abolished, and when an attempt at a generalization of the local system was made, artificial methods were adopted to come to the same conclusions. Now I would ask M. Libri, or any one who agrees with him, how he can possibly account for such a clumsy, primitive, yet most ingenious, method of avoiding *abacal* difficulties, if we suppose that the writers of the thirteenth and following centuries derived their arithmetical knowledge direct from the Arabs?

And now for the *modus operandi*: and in order to render it intelligible to every reader, let us take the first example in multiplication, on account of its great simplicity:—

"Sint ergo iiij. pedes equi, unusquisque habens vj. clavos."

Arbas is to be placed in the lower part of the singular arc

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[*] Communicated by the Author.

[†] Hallam's Introduction to the Literature of Europe during the Middle Ages, vol. i. p. 151.

(arcus singularis), and in the upper part of the same arc is placed chalcus "quasi fundamentum multiplicationis." But in the actual multiplication recourse is had to the common Roman notation, and the result of the multiplication of arbas and chalcus in the singular arc is xxiiij. Then the system of *articuli* comes into operation, and the *articulus* of this number (24) is andras, which, *by the principle of local position and of no other*, is placed in the *decenal* arc. Now I would ask M. Libri, in reply to every one of his arguments, how can we possibly suppose a rule of this nature with its full explanation to exist, without allowing its author to have possessed the knowledge of the value of local position? The decenal arc is made use of in a simple but masterly manner, and the articulate system is invented to avoid the principal difficulty. The digit arbas, it is almost unnecessary to observe, is placed in the singular arc, and thus we have the complete number represented.

In higher numbers the centenal, millenal, and other arcs come into use. The following rule is a fair specimen of the methods employed:—

"Cum autem per decenum multiplicabis singularem, dices hanc regulam deceni;—Decenus quemcunque arcum multiplicat, in secundo ab eo pone digitum in ulteriori articulum," fol. 2, r°, the reason of which is obvious. Thus, in the MS., the operation for finding the square of twelve is as follows:—

	2	
1		4
	2	

		6
1	4	4
	2	4

proceeding in a most complicated manner, but merely using the simple formula

$$m.(np) = mnp, \text{ or, } 12 \times 12 = 12 \times 2 \times 6 = 24 \times 6.$$

in which latter case the above rule is applicable. This rule is afterwards generalized.

"His patefactis, oculus mentis aperiatur ad subtilitatem divisionis;" but as the same system is carried out, precisely similar to the methods of Johannes de Sacro-Bosco, I do not consider it at all necessary to repeat them.

Gerbert uses the Boetian fractional notation*, and I consider this fact a grand argument for his acquaintance with the Boetian contractions, if, indeed, the passage in the geometry of Boetius was not introduced by him. It does not appear to me that much authority ought to be given to the well-known passage of William of Malmsbury,* as far as it is supposed to prove that Gerbert brought the knowledge of the abacus from Spain: and, as Professor Peacock† so well observes, "the passage of this historian contains no certain intimation of the knowledge of the notation by nine figures and zero, as the rules which would be thence derived, would tend rather to relieve than increase the labours of the sweating calculators,"—*quæ a sudantibus abacistis vix intelliguntur.* Now had the question of the Boetian contractions been broached when Professor Peacock composed his history of arithmetic, he would immediately have seen how evidently this passage refers to them, and this supposition would have explained his doubts in the remaining part of his argument.

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* Pezii Thesaurus, tom. i. pars ii. col. 13. "Quod abacistæ facillimum est." *Ibid.* col. 30.

In the treatise of Berhelinus in the Bodleian library‡, the Boetian contractions occur explained by *Greek* numerals,—a most singular and important fact, and one which affords a very strong argument for what M. Chasles has stated at p. 474 of his *Aperçu Historique. En passant,* this is also an argument for the antiquity of this artificial abacal system.

Again, what difference is there between the system of the Greeks, the system in the Mentz Manuscript, the system in the passage in Boetius as satisfactorily explained by M. Chasles, and the Arabic method? I mean with regard to first principles§. All, in fact, are contained in the following formula, which is the general expression for any finite number:—

$$N = a_m 10^m + a_{m-1}10^{m-1} + a_{m-2}10^{m-2} + \dots + a_1\, 10 + a_0,$$

where a_0, a_1, a_2, ... a_m are digits, or integers less than the radix 10.

XXV. *On the Permeability of various bodies to the Chemical Rays. By* ROBERT HUNT.||

HAVING many years since repeated, with much interest, the experiments of Wedgwood, Davy, and Wollaston on the chemical influence of light, it was with much pleasure

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* Wright's Essay on Anglo-Saxon Literature (p. 66).

† History of Arithmetic, p. 415.

‡ I possess a transcript of this manuscript, but, having mislaid it, am compelled to defer any commentary on it till M. Chasles has published his edition.

§ M. Chasles, *Aperçu Historique*, p. 474.

|| Communicated by the Author.

that I read Mr. Talbot's paper on photographic drawing, which opened to me new views, and pointed out paths rich in the promise of important results.

The vast sum of delight which the pursuit of this subject during the past year has afforded me, makes me a large debtor to that erudite gentleman, which I thus humbly, but sincerely, acknowledge.

My first endeavours in the photographic art were directed to restoring the natural order of light and shadow; and I fortunately succeeded in effecting this very early in the summer of 1839. My next were to improve the camera for photographic purposes, in which object I was most materially assisted by Mr. John Towson, of this town, who, having directed much of his attention to optics, furnished me with information and instruments by which my progress was greatly accelerated*.

Having, in conjunction with this gentleman, while trying a variety of lenses, been often perplexed by the dissimilar results obtained on the same paper from different kinds of glass, I was induced to commence a series of experiments on the interference of transparent bodies to the permeation of chemical light.

The same subject has, I am informed, engaged the attention of two scientific inquirers on the continent; but beyond a brief notice of M. Edmond Becquerel's experiments in the Athenæum, No. 621, I am perfectly unacquainted with the methods or results of their observations.

Being anxious to obtain a measure of the interference of the various bodies I was about to examine, I constructed a very delicate galvanometer—the coil being of ribbon copper and the needles of French watch-spring. To this instrument I connected, by platina wires, a U tube, as suggested by M. Becquerel in his *Traité d'Electricité*, which held in one arm a solution of nitrate of silver, and in the other a solution of iodide of potassium. Every part of the tube was screened from light, except the lowest point, at which the fluids met. On this point, by means of a powerful lens, a concentrated pencil of light was thrown, which was made to pass through the bodies to be examined. The force of electro-chemical action being dependent on the quantity of chemical light impinging on the exposed portion of the fluids, led me naturally to conclude that the deflections of the needle would furnish very accurate comparative results. I have also tried the plan M. E. Becquerel adopts, of floating one photometric fluid upon another. Experience has, however, convinced me that the galvanometer, although capable of being made in the hands of a skilful manipulator a very accurate measurer of the diurnal variations of the quantity of chemical rays in the solar beam*, cannot be depended on where a series of nice comparisons are required. I have never yet been enabled to arrive at precisely the same results by this instrument in any two sets of observations; every thin cloud or the lightest smoke materially altering the deflections. I have, however, found it of use in giving me near approximations to a correct arrangement. I proceed thus: having by the galvanometer tabulated a number of bodies, I select those whose interference seems to approach near each other, and place them in regular order, under the same circumstances, upon a sheet of highly sensitive photographic paper in a dark room; then opening the window-shutters, expose it for three minutes to the direct influence of the solar rays, or for twice that time to diffused daylight; again darkening the apartment I examine the tints at which the paper has arrived under each body, and mark their correspondence or otherwise with the observations by the galvanometer. By carefully repeating many times each set of experiments, I am enabled to correct small errors of observation.

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* See L. and E. Phil. Mag. for November last, vol. xv. p. 381.—Edit.

I use yet another method to test the correctness of the foregoing processes, which consists in filling a camera with the fluid or gas to be examined, or interposing the solid body and receiving the sun's image on a disc of silvered copper, prepared according to the principles of the Daguerreotype.

As many simple contrivances will suggest themselves to those who are desirous of repeating the experiments, it may be sufficient for me to state, that my apparatus is simply one cylinder sliding within another for the purpose of adjusting the focus to the different densities of the bodies, and that the photographic disc is protected from the fluid or gas by a piece of tested plate glass well greased around the edges, as are also the cylinders, throughout their length.

This plan may appear open to some of the objections I have urged against the galvanometer; but, as from the sensitiveness of the preparation an exposure of thirty seconds is sufficient, you are enabled to select your moments of observation, and using the galvanometer at the same time to mark the intensity of light, try every substance under precisely the same circumstances. Having completed the exposure of a series, I place all the discs in the mercurial vapour-box together, and the instant the impression appears the strongest, remove them and carefully compare effects.

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* Long prior to the publication of the speech of M. Arago on the report of the Commission on the Daguerreotype, both Mr. Towson and myself had remarked that the light of morning acted more powerfully on photographic preparations than the evening light. The paper which at nine in the morning became in ten minutes a rich purple bronze, took almost twice that time to reach the same hue at three in the afternoon.

The following are the results I have arrived at by these means. At the head of each series I have placed the mean permanent deflection of the galvanometer needle, from ten careful experiments with each of the bodies included within it. By this means a comparative view is given of one series with another.

Series 1.—*Deflection* 22° 30″.
Nitrogen
Atmospheric air
Oxygen
Hydrogen
Carbonic Acid
Carbonic Oxide
Steam (invisible)
Nitrous Oxide
Water
Alcohol (absolute)
Æther (sulphuric)

Series 2.—*Deflection* 20°.
British Plate Glass
Iceland Spar
Carbonate of Soda
Nitrate of Potassa
Ditto, *fused and opake*
Camphor
Sulphuric Acid
Hydrocyanic Acid (Scheele's)
Nitric Acid

Series 3.—*Deflection* 18° 80″.
Crown Glass
Flint Glass
Mica
French Plate Glass
Alum
Gum Arabic
B. Plate and Crown Glass
German Plate (with a pink shade)
Two pieces of Crown Glass
Purple Fluor Spar
Nitrous Acid Gas
Iodine Vapour

Series 4.—*Deflection* 17° 15″.
B. Plate and Flint Glass
Flint and Crown Glass
Three pieces of Crown Glass
Three laminæ of Mica
Creosote
Oil of Aniseed (German)
—— Peppermint (English)
—— Rosemary
—— Savine
Four pieces of Crown Glass
Amber
Green Fluor Spar
Oil of Lavender
—— Caraways
—— Cloves
Canada Balsam

Series 5.—*Deflection* 4° 35″.
Green Bottle Glass[*]
Chlorine
Protoxide of Chlorine
Bromine (vapour)
——— (liquid)
Lightly-smoked Glass

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[*] I have been much surprised at some of the manufacturing chemists in London sending out their hydrocyanic acid and other easily decomposable preparations in bottles of *purple* glass, which offers no interruption to the chemical rays. Dark green glass should be substituted.

It is necessary I should state that the results are likely to be differently recorded by different observers, unless the same photographic preparation is used in all cases. I have been in the habit of using a paper washed with a solution of the muriate of baryta and nitrate of silver, while it is yet damp. The sensitiveness of this preparation may be shown by the fact of its being acted on very decidedly in five minutes by a gas flame from a ten-holed Argand burner. On this paper the tints are blue under greenish glasses, while under those inclining to a yellow they are reddish. If a paper prepared with a solution of the chloruret of soda be used, the tints are reddish under the green, and bluish under the pure white or yellowish glasses.

The above list does not contain all the bodies I have examined, but they are all I am satisfied to place in a determinate position.

Not having deduced any fixed principles from my observations, I may appear to act prematurely; but as it is probable the same subject may be engaging those whose minds and means are superior to my own, and as it is possible even my humble experiments may be of service to such persons, I feel myself excused from the charge of obtrusiveness.

12, Cornwall Street, Devonport, January 2, 1840.

XXVI. *On an anomalous Electric Condition of Iron.* By MARTYN J. ROBERTS, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT is now some months since, that while prosecuting a series of novel galvanic experiments, I discovered a singular anomaly in the electric condition of iron, which is, that although iron if associated with copper as a galvanic pair is highly positive to the copper, yet when associated with zinc, it is more highly negative to the zinc than copper would be under similar circumstances; or in other words, that although copper and iron form a galvanic combination, in which the iron is in the same relation to the copper that a zinc plate would be, yet that iron and zinc form a galvanic pair that has a greater power of generating electric action than a similar sized pair of copper and zinc. This singular phænomenon will, I believe, lead the way to some important discoveries; but not to occupy too much space in your valuable Journal, I will without further comment give extracts from my notebook of some experiments made by me on this subject.

Jan. 1st, 1839. A galvanic combination of iron and zinc was put in communication with two poles of a differential galvanometer; a like-sized combination (or gal. pair) of copper and zinc was connected with the other two poles of the diff. galvanometer: Deviation of needle in favour of the iron and zinc pair = 25 degrees.

Feb. 27, 1839. Experiments made on the comparative power of two galvanic batteries fitted up on Wollaston's plan (that is with the negative plate opposed to both surfaces of the zinc or positive plate.) The size of the plates was the same in both batteries, viz. zinc plate 2½ inch. by 2½ inch. The number of pairs in each battery was ten. The only difference between the two batteries was, that the negative plates in one were of copper and in the other of iron.

The exciting solution was dilute sulphuric acid, which was not renewed during the experiment, but the experiment was continued until the acid was exhausted. The power of each battery was applied to the decomposition of water, and the gas collected was the measure of the power.

RESULTS.

Battery of Copper and Zinc.

The first cubic inch of gas obtained	in	33 minutes.
Then one half cubic inch ...	in	92 minutes.
Acid was now exhausted. 1½ cubic inch	in	125 minutes.

Battery of Iron and Zinc.

First cubic	inch	obtained		in	7 minutes.
Second	—	—	—	in	9 minutes.
Third	—	—	—	in	26 minutes.
Fourth	—	—	—	in	62 minutes.
Acid now exhausted. 4 cubic inch.				in	104 minutes.

Iron battery with a measured quantity of acid gave 4 cubic inches in 104 minutes.

Copper battery with a like quantity of acid gave only 1½ cubic inch in 125 minutes.

I have the honour to remain,
Gentlemen, yours, &c.

Dec. 16, 1839. MARTYN J. ROBERTS.

XXVII. *Notices respecting New Books.*

Curtis's British Entomology.

WE are happy to announce the publication of the Preface, General Indexes, &c. to this beautiful work, which is now *completed*, after sixteen years of almost unremitting application on the part of the author. The sixteen volumes which, if arranged systematically as proposed by Mr. Curtis, will form eight, contain illustrations of all the Genera of Linnæus, Fabricius, and Latreille that are recorded as native groups, as well as most of the interesting discoveries that have been made for many years past, comprising 770 copper-plates, giving faithful figures of our wild flowers, as well as the insects, beautifully coloured and finished with the greatest care. The letter-press, amounting to nearly 1700 pages, although scientific, contains concise accounts of the history and œconomy of every group that is interesting and familiar to us, such as the Hive-Bee, Wasps, Cockroaches, Molecrickets &c.; and there are two thick volumes of the Butterflies and Moths.

XXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xv. p. 544.]

Nov. 6. 1839. A NOTICE of showers of ashes which fell on board the Roxburgh, at sea, off the Cape de Verd islands, February, 1839, by the Rev. W. B. Clarke, F.G.S., was first read.

The object of this communication is to register an interesting occurrence, though the author possesses no direct evidence of its probable cause.

On February 2, when the Roxburgh was in latitude 21° 14 N., long. 25° 6′ W., the wind, which had blown from the north-east during the passage from Plymouth, changed to the east and southeast, and was accompanied with a thick haze of a peculiar kind. The same description of weather prevailed on the 3rd, when the ship was off St. Anthony, one of the Cape de Verd islands.

On Feb. 4, the latitude at noon was 14° 31′ N., long. 25° 16′ W. The sky was overcast, and the weather was thicker than before and insufferably oppressive, though the thermometer was only 72°. At 3 P.M. the wind suddenly lulled into a calm, then rose from the south-west, and was accompanied with rain, and the air appeared to be filled with dust, which affected the eyes of the passengers and crew. At 10½ P.M. the wind returned to the east and blew strongly. During the continuance of the haze, which was as thick as a November fog, and extended all around the horizon, dust was gradually deposited on every part of the ship that offered a lodgement. At noon, on the 5th of February, the Roxburgh was in lat. 12° 36′ N., long. 24° 13′ W., thermometer 72°, barometer 30°, the height at which it had stood during the voyage from England. The volcanic island Fogo, one of the Cape de Verds, was about 45 miles distant. The weather was clear and fine, but the sails were found to be covered with an impalpable reddish-brown powder, or a kind of triturated pumice, which Mr. Clarke says resembled many of the ashes ejected from Vesuvius, and was evidently not sand blown from the African desert. On the 6th the wind returned to the south-east, and the weather afterwards resumed its ordinary characters.

The circumstances connected with these atmospherical changes induced the author to infer that they were due to an eruption in the Cape de Verd group.

In June, 1822, the ship Kingston, of Bristol, bound to Jamaica, while passing near Fogo, had her sails covered with a similar brownish triturated pumice, which it is stated smelt strongly of sulphur.

Mr. Clarke also mentions the following instances of similar phenomena on the authority of the officers of the Roxburgh :—

In the lat. of the Canaries, and long. 35° W., showers of ashes have been noticed two or three times. At Bombay the decks of the vessels were on one occasion covered to the depth of an inch with dust, which was supposed to have been blown from Arabia.

In January, 1838, dust was noticed by the crew of a vessel navigating the China sea, a considerable distance from the Bashee islands, one of which had been previously seen in eruption[*].

In 1812, ashes fell on the deck of a packet bound to the Brazils, and when 1000 miles from land.

Mr. Clarke also mentions the ashes which fell at sea during the eruption of Vesuvius in 1822, and 400 miles from that volcano; likewise the reddish dust which fell in the south of Italy and in Sicily on the 16th May, 1830, as well as in 1807 and 1813, during eruptions of Etna, and at first attributed to those outbursts, but afterwards found to be sand similar to that of the desert of Africa. During the eruption in May, 1830, a caravan perished beneath a whirlwind of sand, and similar storms occurred during the eruptions of 1807 and 1813[†].

A letter was then read from Mr. Caldcleugh, dated St. Jago de Chili, 18th February, 1839, containing the following translation of the declaration of the master and crew of the Chilian schooner Thily.

"I, the undersigned, Joseph Napoleon Escofier, master of the said vessel, and with the corroborating evidence of my crew, declare as follows :—

"This, the 12th day of February, 1839, at ten minutes past 9 o'clock in the morning, being in lat. 33° 32′ S., and 74° 32′ W. long. of the meridian of Cadiz (80° 51′ W. of Greenwich), we felt an earthquake, which lasted more than a minute. The noise which accompanied it was similar to that caused by the running out of a heavy chain cable. At fifteen minutes past 7 o'clock of the same evening we saw an island rising out of the sea, of the height of Curauma

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[*] Mr. Clarke believes that this is the first announcement of a volcano in that group.

[†] Bulletin, Soc. Geol. France.

Point, bearing south 79° W. by compass, distant from six to nine miles. A considerable time afterwards the island divided itself into the form of two pyramids, the most northern one crumbled away diagonally towards the north, and the southern one disappeared in pieces, the base however always remaining above the surface of the sea. At half-past 7 o'clock the same island appeared again, or its size became considerably increased, but shortly afterwards its summit became flattened. At thirty-five minutes past 7 o'clock two other islands appeared to the southward of the first. Of these, the most southern bore south 56° west. The three islands appeared to run in the direction of north and south. The sea broke with violence upon their shores and seemed violently agitated. In the distances between these islands nothing was visible but chains of rocks, among which a great explosion was discernible.

"At eight minutes before 8 o'clock, the most northern island was the only one visible: it appeared much higher than before, and of the shape of a sugar loaf. The darkness of the night prevented us seeing the other two islands.

"The following day, the 13th of the month, at a quarter past 1 o'clock in the morning, the larboard watch and myself saw at intervals a light in the same direction as the islands, south 72° west, which appeared to be caused by a volcano.

Position of the most northern island:
Long. W. of meridian of Cadiz, 70° 33′ (76° 52′ W. of Greenwich.)
Lat. S.............................. 33 34.

Position of the most southern island:
Long. W. of meridian of Cadiz, 70° 34′ (76° 53′ W. of Greenwich.)
Lat. .... 33 40.

"I consider my longitude to be correct from having sighted Juan Fernandez on the 11th, at 8 o'clock in the morning, and compared its bearings with my latitude by observation.

"Signed, &c. &c. &c."

Mr. Caldcleugh adds, the master of another vessel reported that the islands bore 30 leagues due east of Juan Fernandez; and that a ship had been despatched from Valparaiso to discover whether they remained above water or had crumbled away.

The larger Curauma Point, referred to in the declaration, is a bluff point, about 400 feet in height, and situated to the southward of Valparaiso.

A letter was next read, addressed to Charles Lyell, Esq., V.P.G.S., by John Buddle, Esq., F.G.S., on depressions produced in the surface of the ground by excavating beds of coal.

Subsidence of the surface invariably follows the working of the subjacent beds of coal where sufficient supports are not left, but the extent of the subsidence is governed by the following circumstances:

1st. The depth of the seam of coal below the surface.

2nd. The thickness of the seam or seams removed.

3rd. The nature of the strata between the surface and the seams of coal.

4th. Whether the pillars of coal are wholly or partially worked.

If the depth from the surface does not exceed 30 fathoms, and sandstones form the predominant strata, the subsidence is about equal to the thickness of the seam of coal removed; but if metal-stone constitute the greater portion of the intervening mass, the amount of depression in the surface is less. This rule is considered to hold good at all depths.

The degree of subsidence does not depend so much on the thickness of the bed of coal, as on the entire removal of it; but Mr. Buddle states, that he has had no opportunities of making correct observations on the relative effect produced in the surface. If a considerable portion of the coal be left, although quite inadequate to the support of the superincumbent strata, the subsidence is retarded. This is more particularly the case in the Newcastle system of working, where rectangular pillars are left in the first instance and afterwards removed. In working these pillars, stooks or blocks of coal of considerable strength are left as props to protect the colliers from the exfoliation of the roof; and though a subsidence of the superincumbent strata invariably takes place, yet the extent in the first instance is governed by the degree of resistance of the stooks; and it frequently happens, that a large tract of a coal mine remains for several years only filled in part, and without any perceptible change occurring. In course of time, however, from the exfoliation of the stooks and the operation of the atmosphere, a further subsidence, called a second creep, takes place, and generally continues until the excavation is completely closed.

In the Yorkshire system, by which all the coal is taken out in the first instance, except small pillars, the roof being principally supported by wooden props and stone pillars, the subsidence of the strata takes place immediately after the coal is removed, and there is no second settlement.

It is only where water accumulates on the surface or a railway traverses a coal-field, that the amount of subsidence can be accurately ascertained.

In one instance, mentioned by Mr. Buddle, the excavation of a bed of coal 6 feet thick, one-fourth having been left in "stooks," the depth of the bed from the surface being 100 fathoms, and the overlying strata principally sandstone, the amount of subsidence was shown by the accumulation of a pond of water, to have been rather more than 3 feet deep.

In another instance, it was found necessary to restore the level of a railway three times, in consequence of three distinct sinkings of the surface having followed the successive excavating of three seams of coal. The tract in question is of a quadrangular form and about 23 acres in area, and contains the following five seams of coal:

Coal.	Depth below the surface. fath.	Thickness. ft.	 in.
1. The three-quarter seam	54½	1	8
2. The five-quarter	62	3	6
3. The high main	73	6	3
4. The Maudlin	83½	5	0
5. The Hutton	107	3	8

The high-main seam was first worked, then the Maudlin, and afterwards the Hutton, and the removal of each was attended with a depression in the line of the railway. The extent of each settlement was not measured, but the whole amount was 5 feet 6 inches, the aggregate thickness of the seams being 14 feet 11 inches. This small effect Mr. Buddle explains by showing, that the railway passes near one end of the excavated tract, and that metal-stone predominates over sandstone in the superincumbent strata. The working of the five-quarter seam is now in progress, and the effects occasioned by the removal of the three lower seams are well exposed. Innumerable vertical cracks pass through the coal, its roof and pavement, but they are perfectly close except around the margin of the settlement. Along this line the strata are bent down, the cracks in the pavement are frequently open, forming considerable fissures, the coal is splintered, and the roof-stone is shattered. In the interior of the settlement the pavement is as level and smooth as if it had never been disturbed, and the cracks are quite close, passing through the seam without splintering it or producing any effect except that of rendering it tougher, or, in the language of the colliers, "woody." This effect, Mr. Buddle conceives, may be attributed to the escape of the gas, and he states that it is sometimes produced by other operations, when the coal is said to be "winded." The smoothness of the pavement, he is of opinion, is due to the direct downward pressure of the superincumbent mass; and he states, that he has never noticed any tendency to a sliding or sideway movement in any subsidence of strata occasioned by the working of the coal, except the slight obliquity occasioned by the offbreak at the sides of the settlement.

[To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

December 14, 1839. The following communications were read:—

On the parallax of Sirius. By Thomas Henderson, Esq. Astronomer Royal for Scotland.

The parallax of Sirius, the brightest star in the heavens, has been several times the subject of investigation among astronomers. From the variations of the zenith distances observed at Paris, the second Cassini inferred a parallax in declination amounting to six seconds of space; and, from similar variations in the observations of La Caille made at the Cape of Good Hope, some astronomers have deduced a parallax in declination of four seconds. Piazzi has also obtained from his observations a parallax of the same amount. On the other hand, La Caille's observations of zenith distances made at Paris, more numerous and certain than those made at the Cape, do not exhibit any sensible parallax; and the observations which have since been made in the observatories of Europe, would appear to lead to the same result, as no parallax has ever been deduced from them. In the *Fundamenta Astronomiæ*, M. Bessel has investigated, from Bradley's Observations of Differences of Right

Ascension of Sirius and *α Lyræ*, the sum of the parallaxes of the two stars, and has found it to be an insensible quantity.

The extensive series of observations of Sirius, made with the mural circle of the Observatory at the Cape of Good Hope, is well adapted for the investigation of the parallax, as the observations possess some advantages over those made in Europe. The star is near the zenith of the Cape, and the temperature is nearly the same when it passes the meridian at noon in June, and at midnight in December, the periods of the greatest parallaxes in declination; so that the irregularities and uncertainties of refraction, which affect observations in Europe, may be supposed to disappear.

From May 1832 to May 1833, ninety-seven observations of Sirius were made by Mr. Henderson with the mural circle at the Cape Observatory, of which sixty-three were made by direct vision, and thirty-four by reflection; and in Mr. Maclear's printed observations of zenith distances, made with the same instrument, there are sixty-seven observations of the double altitude of the star, made between August 1836 and December 1837. Each of these series of observations was made in one position of the telescope upon the circle, so that in each series the similar observations were referred to the same divisions.

The observations made by Mr. Henderson have been reduced in the same manner as were those of *α Centauri*, given in his memoir on the parallax of the latter star. (*Monthly Notice*, Vol. IV. No. 19.) The declinations of Sirius have been determined by comparisons with such of the principal or standard stars as were observed on the same day; and it is consequently assumed that, in the observations of the stars of comparison, any errors which may arise from supposing their parallaxes to be insensible, and the coefficient of aberration to be correctly assumed, neutralize each other. The mean declinations of the standard stars of comparison have been taken from the catalogue annexed to the author's "Memoir on the Declinations of the Principal Stars:" the absolute places of the stars are not required, but only their relative positions with regard to each other.

Mr. Maclear observed the double altitude of Sirius, or the angular distances between the star seen by direct vision and by reflexion at the same transit over the meridian. These are independent of the observations and assumed positions of other stars, and are affected by twice the amount of the parallax in declination. Observations of this description appear to be the best adapted of any which can be made with the mural circle for the investigation of small variations in the declination of a star.

In the reductions of Mr. Henderson's series the constant of aberration has been assumed $= 20''\cdot50$; of Mr. Maclear's, $= 20''\cdot36$; in the reductions of both the coefficient of lunar nutation has been assumed $= 9''\cdot25$, and the annual precession and proper motion have been taken from the *Tabulæ Regiomontanæ*.

Mr. Maclear's observations are suitable for determining the constant of aberration; the correction to be applied to it has been therefore introduced as an unknown quantity into the equations of condition; and, from the value which is obtained, we may judge of the degree of accuracy with which the parallax is determined.

On resolving, by the method of minimum squares, the two sets of equations, and combining the results according to their relative weights, the greatest effect of parallax in declination is found, from the whole of the 231 observations, = + 0″·15; and the greatest effect of aberration in declination, = 13″·07. These quantities are to the total effect of parallax and aberration in the proportion of 13″·13 to 20″·50, whence the final results are—

Parallax of Sirius (or the angle subtended by the radius of the earth's orbit, at a distance equal to that of the star). = 0″·23
Constant of aberration .... = 20·41

The error of this determination of the parallax may be estimated not to exceed a quarter of a second, as it is almost certain that the constant of aberration is not in error to a greater amount. On the whole, it may be concluded that the parallax of Sirius is not greater than half a second of space, and that it is probably much less.

A Catalogue of Twenty-seven Stars of the Pleïades. By M. Bessel, Director of the Observatory Königsberg.

The catalogue was computed by M. Bessel from meridian observations made by himself and his assistant, Dr. Busche. It contains the positions, annual precession, and its secular variations in Æ and declination, together with the proper motions, and a comparison with Piazzi's catalogue.

In a letter addressed to Mr. Baily, containing the above catalogue, M. Bessel announces, that the observations respecting the parallax of 61 *Cygni** have been continued through a second year; and that the result of this new series will agree very nearly with that of the first. The publication of the observations will be delayed for a few months, in order to obtain a more certain determination of the proper motions which the two small stars compared seem to possess; and he adds, that although the *weight* of the former result was sufficiently great to leave no doubt about the real existence of the parallax, it is gratifying to see its quantity so very nearly confirmed by a second series of observations.

A Letter from M. Valz, Director of the Observatory at Marseilles, to the President, Sir J. F. W. Herschel, Bart., relative to the Variation of the Apparent Diameter of Encke's Comet.

After adverting to some objections suggested by Sir John Herschel (*Memoirs of the Royal Astronomical Society*, vol. vi. p. 102†) to the theory by which M. Valz explains the changes observed in the apparent diameters of some comets, when near their perihelia, namely, the condensation of volume produced by the pressure of an ethereal medium growing more dense in the vicinity of the sun, the author proceeds to give his own observations on Encke's comet, at the time of its last perihelion passage in 1838, when it appeared under circumstances favourable for observing the nebulosity. He states that he was able to follow the comet till the evening before the perihelion passage; that he observed it to diminish rapidly, and, after being prodigiously reduced, to melt away, as it were, under his eyes, disappearing only in consequence of its extreme smallness, inasmuch as its brilliancy should, from its position, have continued to increase. The observations are as follows:—

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* Abstracts of M. Bessel's observations on the parallax of the star 61 *Cygni* will be found in Lond. and Edinb. Phil. Mag. vol. xiv. p. 68. 226.—Ed.

† An abstract of Sir J. Herschel's paper appeared in Lond. and Edinb. Phil. Mag. vol. ii. p. 222.—Edit.

On the 9th and 10th of October, the nebulosity subtended an angle of 20′, but it diminished continually after that time. On the 15th of October, he first remarked it to be elongated in the direction of the sun; and the elongation continued to increase until the 25th of October, when the greater diameter appeared to be double the smaller, after which it began to diminish. The most luminous part was not at the centre, but at the point opposite the sun. On the 25th of October, the nebulosity was reduced to 15′, and the real volume was then eighteen times smaller than on the 10th. On the 6th of November, the nebulosity was 13′, and the volume reduced to 1–40th. On the 13th of November, the nebulosity was 11′; on the 16th, between 8′ and 9′; on the 20th between 6′ and 7′; on the 23rd, 4′; on the 24th, 3′, and the real volume, 826 times less than on the 10th of October. On the 29th of November, the comet could no longer be seen in the evening twilight, but it reappeared on the morning of the 7th of December *. On the 12th of December it appeared as a star of the fifth magnitude; and its diameter was less than 20″, being entirely covered by a wire of that thickness. The volume deduced from this apparent magnitude would be 80,242 times less than on the 10th of October. On the 14th of December it appeared feebler, and equal to a star of the sixth magnitude, with which it was compared; its diameter was then estimated at 15″. On the 16th the comet appeared as a star of the seventh magnitude, and its apparent diameter was from 10″ to 12″. On the 17th it was reduced to the eighth magnitude at most, and its apparent diameter was from 7″ to 8″. On the 18th of December it was entirely invisible, although stars of the seventh and eighth magnitudes were seen in its neighbourhood. From these comparisons it appears that the real diameter must have undergone a diminution from the 10th of December*, when it was first observed in the morning, until the 18th, when it finally disappeared.

A Letter from Professor Schumacher, to Francis Baily, Esq., announcing the Discovery of a Comet by M. Galle, assistant in the Berlin Observatory.

The comet was discovered on the 2nd of the present month, 17^h 45^m mean time (Berlin), in the constellation *Virgo*. Comparing it by the great refractor, with a star of the tenth magnitude (which star was immediately compared with γ *Virginis*), M. Galle obtained the following positions:—

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* There must be some error here in the dates.—Edit.

Sidereal Time, Berlin.			AR. of Comet.			Declination of Comet.		
h	m	s	h	m	s	°	′	″
11	1	14	12	38	25·18			
11	9	42	12	38	28·26	−2	10	22·8
11	21	45	12	38	32·38	−2	10	13·9
11	40	39	12	38	39·63	−2	9	57·3

These observations give its daily motion in AR, + 2° 12′, in decl. + 0° 19′. It has a well-defined point, as a nucleus, within the uniform nebula, which, opposite to the sun, expands in the form of a tail.

Tables for the Calculation of Precession, for the year 1825, of Stars observed by M. Bessel in the several Zones, from − 15° to + 15° Declination. By Dr. Max. Weisse, Director of the Observatory at Cracow.

Observations of Moon and Moon-culminating Stars, Eclipses of Jupiter's Satellites, and Occultations of Fixed Stars by the Moon, made at the Observatory of Paramatta, in New South Wales, in the year 1838, by Mr. Dunlop. Communicated by Sir Thomas Macdougal Brisbane, Bart.

XXIX. *Intelligence and Miscellaneous Articles.*

THEORY OF SUBSTITUTIONS.—ACETIC AND CHLOROACETIC ACIDS.

M. DUMAS has lately read the following note on these subjects to the Royal Academy.

"In a previous memoir I have shown that chlorine decomposes acetic acid under solar influence, and that it gives rise to a new acid, which I call chloroacetic acid. On this occasion I expressed my opinion that acetic acid and chloroacetic acid belong to the same chemical type, one being represented by $C^8H^8O^4$; and the other by $C^8H^2O^4Cl^6$. I endeavoured to generalize this view, and to explain how these types might serve to group organic bodies into well-characterized classes.

"M. Berzelius, not admitting the theory of substitutions, has given, as soon as he became acquainted with my memoir, a refutation of the views announced in it. He considers acetic acid and chloroacetic acid as very different from each other, because they have not the same density, nor the same boiling point, nor the same odour, &c.*

"M. Berzelius has certainly not understood what I call the *fundamental properties* of a body, for I have long known that by replacing the hydrogen of a compound by chlorine it is rendered more dense and less volatile, and at the same time the density of its vapour is increased. It is therefore perfectly clear to me that the objections made by M. Berzelius are not at all directed to the views which I would really express. In order therefore to avoid any fresh misunderstanding, I shall endeavour to illustrate my opinion by an example. In causing chloroacetic acid to act upon an alkali, I observed a very remarkable reaction. The acid was converted into two new bodies, namely, carbonic acid, which combined with the alkali, and chloroform, which was set free. We have therefore

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* See our last Number, p. 1.

$$C^8\ H^2\ O^4\ Cl^6 = C^4\ O^4 + C^4\ H^2\ Cl^6.$$

"I was convinced, and I had to a certain extent announced it in my memoir, that acetic acid would produce an analogous reaction; that is to say, under the influence of an excess of base it would change into carbonic acid and a carburet of hydrogen, the formula of which would be $C^4\ H^8$. After some trials I perfectly succeeded in producing this remarkable reaction. It is only necessary to mix 10 grammes of crystallized acetate of soda with 30 or 40 grammes of caustic barytes, and to heat the mixture very slightly in a retort, to effect the conversion of the acetic acid into carbonic acid and a gas, the formula of which is $C^4\ H^8$.

"This decomposition is quite perfect: the residue is perfectly white: not the slightest trace of oil or of pyroacetic spirit is disengaged, nor any vapour, except the water which accompanies the gas. The analysis of this gas by the eudiometer proved that it was formed, as is commonly stated, of one volume of vapour of carbon and two volumes of hydrogen. This is precisely the composition of a gas which chemists have never been able to produce, I mean light carburetted hydrogen (gaz des marais). It is impossible not to observe the connexion which exists between light carburetted hydrogen, produced by the spontaneous decomposition of vegetable substances, and that resulting from the final decomposition of acetic acid, which has been itself produced by the destructive distillation of wood.

"I intend to perform a complete examination of this gas, and to follow out an examination of reactions analogous to that which caused its discovery. At present I confine myself to announcing in a distinct manner that the gas $C^4\ H^8$, corresponding to chloroform $C^4\ H^8\ Cl^6$, according to the theory of substitutions, has been produced by chloroacetic acid; that is to say, that acetic acid and chloroacetic acid possess the same fundamental properties as I had determined, and belong to the same organic type."—*L'Institut, No.* 313.

MYRONIN, MYRONIC ACID.—ESSENTIAL OIL OF MUSTARD.

It results from the experiments of M. Bussy that there exist in the farina of mustard seed two principles, the reaction of which, under the influence of water, gives rise to an essential oil. One of these is a peculiar acid, which M. Bussy calls *myronic acid* (*μύρον* essence), and the other is a substance which has great analogy with albumen, and which he calls *myronin*.

The properties of these substances are as follows:

Myronic acid is inodorous, it exists in mustard combined with potash. Myronate of potash is a salt which is soluble in water, perfectly crystallizable, inodorous, colourless, of a bitter taste, and decomposable by heat. The myronic acid, which may be isolated, combines also with soda, barytes, ammonia, and yields salts, which like the myronate of potash develop essential oil under the influence of myronin.

Myronin is a substance soluble in water, coagulable like albumen by heat, alcohol, and acids; it has great analogy with emulsin, but neither albumen, emulsin, nor the synoptase of M. Robiquet can replace it for the production of essential oil of mustard. When put into contact with a solution of myronate of potash, it develops the odour of mustard, and the liquor submitted to distillation yields essential oil. It exists in black mustard, together with myronate of potash; but yellow mustard on the contrary contains myronin, but no myronate of potash.

It appears that M. M. Boutron and Fremy had simultaneously with M. Bussy discovered the above-described facts.—*L'Institut*, No. 313.

POLYCHROMATIC ACID.

M. Boutin has presented to the French Academy of Sciences a memoir on a new substance resulting from the action of nitric acid upon socotrine aloes. This product, which he calls *polychromatic acid*, is, in the opinion of the author, of considerable importance in dyeing and calico printing. By varying the mordants, it yields an infinite number of tints, all of them finer and more permanent than can be obtained by the usual processes. It has the appearance of a reddish brown powder, is very slightly soluble in water, but still sufficiently so to colour a large quantity of it at common temperatures; it is more soluble in alcohol, and in dyeing possesses the double advantage of yielding, in small quantities, much more colour than the substances usually employed.

This acid is susceptible of combining with metallic oxides, and of forming salts of different degrees of solubility, and all of different colours. Those which the author presented to the Academy were the salts of potash and silver.—*L'Institut*, No. 313.

CYANIL.

M. Boutin also gave an account to the Academy of a new substance which is formed by the action of nitric acid upon aloes, or rather upon polychromatic acid. This product is liquid and colourless when it has been purified by distillation from chloride of calcium, and has so great an analogy, on account of its poisonous properties, with hydrocyanic acid, that it is natural to conclude that they are isomeric bodies. It is so deleterious, that one or two drops in an eight-ounce bottle, half-filled, are sufficient to impart to the air which occupies the remainder of the bottle the power of immediately killing a bird which is made to breathe it; a capillary tube, slightly impregnated with this liquid, and put into the eye of a bird, produces also the same sudden effect.—*L'Institut*, No. 313.

ACTION OF ALBUMEN ON METALLIC SALTS.

M. Lassaigne has presented to the Academy of Sciences researches on the chemical action exerted by metallic salts on albumen and on certain animal tissues.

According to the author the experiments detailed in this memoir prove:

1st. That albumen has the property of combining with a great number of metallic salts without decomposing them, and forming with them compounds, which are insoluble in water, when they are united in certain proportions, but susceptible of dissolving in them severally when there is either an excess of albumen or of the metallic solution combined with it.

2nd. That these compounds, which may be designated by the name of *albuminates*, appear to result from the union of several atoms of albumen with one atom of metallic salt, as shown by the analyses which have been performed.

3rd. That these compounds possess the singular property of dissolving, without undergoing any immediate alteration, in the solutions of alkaline salts, which decompose the metallic salts when taken by themselves, and they remain dissolved for a shorter or longer period, according to the temperature.

4th. That it is extremely probable that when metallic salts are externally exhibited, there takes place in the œconomy, effected by absorption, an analogous combination between these salts, the tissues and the albumen contained in the various animal fluids, and that it is probable they are conveyed in the humours, and their medicinal effect is thus most commonly produced.

5th. That it would be interesting if physicians would ascertain the therapeutic effects of these compounds of albumen and metallic salts.

6th. That in the action of a metallic salt on any tissue, a combination is effected between these two bodies, which must modify its vital properties and effect a change in its functions.

7th. That the property of certain metallic salts of combining either with albumen or with the base of various tissues of our organs, generalizes what has been already stated with respect to bichloride of mercury and the same substances.—*L'Institut*, No. 313.

HAYDENITE.

M. Lévy has read the following notice respecting this mineral to the Academy of Sciences:—

"Cleaveland, in the second edition of his Treatise on Mineralogy and Geology, published at Boston [U. S.] in 1822, has given the name of haydenite to a mineral which had then been recently discovered by Dr. Hayden of Baltimore [U. S.]. He gives the following description of this mineral substance.—It is found in small crystals of a reddish colour, the form of which is cubic or slightly rhombic, and the surface of the faces vary from $\frac{1}{8}$th to $\frac{1}{4}$th of an inch square. It appears susceptible of ready decomposition, and becomes porous and spongy, but always retains its form. Before the blowpipe, it fuses with some difficulty into a yellow enamel; it is soluble in hot sulphuric acid, and the solution deposits small white needles. It has also been found accompanied with zeolite and carbonate of iron, in the fissures of gneiss at a mile and a half from Baltimore.

"The authors who have since mentioned this substance have merely repeated what has been stated by Cleaveland. Mr. Brooke, in his article on Mineralogy in the Encyclopædia Metropolitana, classes, without assigning any reason, haydenite with heulandite. I will add what appears to me singular, which is, that in a work recently published in the United States, entitled a System of Mineralogy, by James Dana, and printed at Newhaven in 1837, no mention is made of this species, though in other respects the work appears to be pretty complete.

"The cause of our ignorance respecting the nature of haydenite, may be explained by the small number of specimens which have been brought to Europe. M. Lévy then goes on to state that he had seen only three specimens of haydenite, and the account which he gives of it is as follows:—Haydenite is regularly crystallized; the crystals have the form of a small oblique prism with rhombic bases, in which the incidence of the lateral faces is 98° 22′, and the incidence of the base, on each of the lateral faces, is 96° 5′. The crystals are frequently macled. The axis of revolution, around which one of the two crystals forming the macle is supposed to have turned 180°, is perpendicular to the base of the primitive form, and the face by which the two crystals are united is parallel to the same base. The crystals are thickly grouped together, and a small portion only of each is isolated. I have observed no modification either upon the edges or angles, so that the relation between the sides of the base and the lateral edges remains undetermined. The crystals cleave with the same facility on every face of the primary form. The cleavage faces sometimes present an uneven surface on account of small dark spots, as if the substance had suffered incipient decomposition. The crystals are usually covered by a thin layer of hydrate of iron, which is readily detached by the knife, and the faces of the crystals thus exposed are sufficiently brilliant to be measured by the reflective goniometer. The colour of haydenite is brownish yellow or greenish yellow; the crystals are translucent and sometimes transparent, they are easily scratched by the knife, readily friable; the hardness is nearly the same as that of fluor spar. The quantity detached was too small to admit of its specific gravity being taken."—*L'Institut.*

BEAUMONTITE, A NEW MINERAL.

This is a new mineral discovered by M. Lévy. It accompanies haydenite, and is named in honour of M. Elie de Beaumont. It occurs in small brilliant crystals of a pearly lustre. Their form is that of a prism with square bases, terminated by obtuse pyramids. The summits of all the crystals are closely grouped. The incidences of the faces of the terminal pyramids, measured with Wollaston's goniometer, are 132° 20′ of the two faces, the intersection of which is parallel to one of the edges of the base of the primary form, and 147° 18′ of the two faces above, whose intersection is inclined to this base. One of these angles is a necessary consequence of the other. By calculating from the first, the second is 147° 28′, instead of 147° 18′, as determined by experiment. The primary form of the beaumontite may therefore be taken as a right prism with square bases, in which the relation between the sides of the base and the height is nearly as the numbers 23 and 10, and then the faces of the pyramid have b^1 as a crystallographical sign. The crystals cleave readily parallel to the lateral faces of the primary form, but more easily parallel to one of the faces than the other, and this greater facility corresponds with the pearly lustre peculiar to them. There are also some indications of cleavage, parallel to the diagonal planes of the primary form, the crystallographical sign of which is g^1. The colour of the crystals is whitish yellow; they are translucent; their hardness is greater than that of haydenite, and is almost equal to that of phosphate of lime.

The crystals of beaumontite and haydenite form a crystalline layer, the brilliant portions of which belong to the first-mentioned substance, and the parts covered with brownish hydrate of iron to the second. This layer covers a granular rock composed principally of grains of quartz and haydenite. The other face of the specimen is covered with small flat elongated prisms of green amphibole.—*Ibid.*

AN ACCOUNT OF THE EXPERIMENT PROPOSED BY M. ARAGO AS A TEST OF THE ACCURACY OF THE UNDULATORY HYPOTHESIS OF LIGHT.*

M. Arago proposes to avail himself of Prof. Wheatstone's revolving mirror, used by that gentleman in his researches on the velocity of electricity whilst traversing good conductors, in determining experimentally the accuracy of one or other of the present hypotheses of light. The principle of the proposed experiment is readily understood. A ray of light incident on the surface of a plane mirror is reflected in the ordinary manner, the reflected and incident beams forming equal angles with a line perpendicular to the point of incidence of the ray. If the mirror be supposed to revolve around this point to an extent expressed by the quantity a, and to augment by this quantity the original angle of incidence, the former angle of reflexion will become lessened to an extent corresponding to $2a$, which must be added to this new angle to render it equal to the first. Consequently, if the incident ray remain the same, an angular movement of the mirror of a will produce an angular motion of the reflected ray equal to $2a$.

If then two perfectly parallel rays be incident in the same vertical line on a mirror revolving round the point of incidence, their parallelism will be preserved after reflexion, providing they both impinge upon the mirror at the same instant of time, and two luminous points situated exactly vertically over each other will be seen; but if the rays impinge upon the mirror at different instants, so that one will be somewhat later than the other, the reflected images will no longer preserve their original position in the same vertical line—one appearing to the right or left of the other.

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* The Editors are obliged to Dr. Golding Bird for this account.

According to the Newtonian hypothesis, or corpuscular theory of light, a ray moves in a fluid of higher refractive power, as water, quicker than in air, in a ratio expressed by the *direct relation* of the sine of the angle of incidence to the sine of refraction; whilst on the undulatory theory, light traverses a liquid much slower than air, and in the *inverse ratio* of these sines. To test the truth of either of these hypotheses, all that is therefore necessary, is to cause two rays of light to be incident on a rapidly-revolving mirror, in the same vertical line, the lowest beam traversing air only, whilst the upper one passes through a tube filled with water, or other refracting liquid. Under these circumstances, if the corpuscular theory of light be correct, the upper ray will reach the mirror before the lower one, and consequently the reflected images will no longer be in the same vertical line: if the undulatory hypothesis be the true one, then the lower ray will reach the mirror before the upper one, and the vertical position of the reflected images will become deranged. Let us suppose that the mirror revolves in a direction from right to left; then, if the upper reflected image appear to the left of the lower, *light consists of moving corpuscules!* but if the upper image appear to the right of the lower—*light is produced by ethereal undulations!* To submit this proposed experiment to the test of experimental investigation is obviously a difficult matter, for, as tubes of water of any moderate length can but very slightly affect the velocity of a ray of light, it is obvious that the rotation of the revolving mirror must be excessively rapid to produce a deviation of the reflected images sufficient to admit of accurate observation.—M. Arago is inclined to consider that a deviation of a minute of a degree produced by two positions of the reflecting plane inclined half a minute of a degree upon each other will be sufficient for this purpose.

From computations deduced from the known velocity of light, it appears that in $\frac{1}{432,000,000}$th part of a second (the time during which a revolving mirror moving by mechanism prepared by M. Gambey moves through half a minute of a degree) a ray of light traverses a portion of space corresponding to 7·07 metres, or, in round numbers, 23 English feet. Hence if the mirror perform 2000 rotations in a second of time, the tube of water through which one of the rays passes must be 23 feet in length to produce, on the theory of emission, an angular separation of the reflected images corresponding to one minute. As a velocity of the revolving mirror of the enormous quantity of 2000 turns in a second is extremely difficult to obtain, the angular deviation produced by reflection from one revolving mirror performing 1000 rotations in a second, may be increased by viewing the images afterwards in a second revolving mirror. In this manner, by repeated reflexions from several revolving mirrors, the angular deviation of the images which eventually reach the eye of the observer may be increased to a sensible quantity.

If, as M. Arago has thought probable, an angular separation of the reflected images equal to half a minute is perceptible to the eye, a tube only 11½ feet long, full of water, will be sufficient to produce such an alteration in the velocity of the transmitted ray, as to render the angular separation of the images very obvious, after reflection from four revolving mirrors, each performing 1000 rotations in a second. By employing a medium of greater refractive power than water, as the essential oils or sulphuret of carbon, a still greater alteration in the velocity of the transmitted ray will be observed, being accelerated on the corpuscular, and retarded on the undulatory hypothesis.

Another mode of testing the truth of the two theories, proposed by M. Arago, is founded upon the different velocities of the coloured rays of the spectrum, whilst traversing refracting media. If, therefore, a beam of white light be made to traverse a long tube filled with a highly dispersive fluid, as sulphuret of carbon, the different coloured rays will not reach the revolving mirror simultaneously, and a spectrum will be produced. So far, the same result occurs on both theories; but on watching the reflected images, a different arrangement of the colours will be observed, and the violet or red band will be observed, in the reflected image, to be situated to the right or to the left, according to the direction of the rotation of the mirror. The order in which the colours appear, the direction of the rotation remaining the same, will be precisely the reverse on the undulatory, to that on the corpuscular theory.

It is of course obvious that great practical difficulties lie in the way of submitting M. Arago's ingenious views to the test of experiment, and hitherto no attempt appears to have been made to overcome the difficulties connected with the construction of the apparatus required for these interesting experiments.

METEOROLOGICAL OBSERVATIONS FOR DEC., 1839.

Chiswick.—Dec. 1—3. Dense fog. 4. Frosty: fine. 5. Slight haze: fine. 6. Foggy. 7. Dense fog: fine: frosty at night. 8—10. Hazy. 11. Fine: hazy: rain. 12. Cloudy and windy. 13. Overcast: heavy rain at night. 14. Fine. 15. Frosty: hazy: rain. 16. Hazy: fine. 17. Fine. 18. Hazy. 19. Cloudy. rain. 20. Rain. 21. Cloudy: rain. 22. Rain: fine. 23. Fine: very mild for the period of the season: rain at night. 24. Boisterous with rain. 25. Very fine. 26. Heavy rain. 27. Rain: hazy. 28. Frosty: fine. 29. Clear and frosty. 30. Frosty and foggy. 31. Overcast: fine.

Boston.—Dec. 1. Fine. 2. Foggy: rain P.M. 3. Cloudy: rain A.M. 4. Foggy. 5. Cloudy: rain P.M. 6, 7. Foggy. 8, 9, 10. Cloudy. 11. Cloudy: rain early A.M.: rain P.M. 12. Fine: rain P.M. 13. Fine. 14. Cloudy. 15. Fine. 16. Foggy. 17. Fine. 18. Stormy: rain P.M. 19, 20. Cloudy: rain early A.M.: rain P.M. 21. Fine: rain P.M. 22. Cloudy: rain P.M. 23. Fine: rain P.M. 24. Cloudy. 25. Fine. 26. Fine: rain P.M. 27. Foggy. 28, 29. Fine. 30. Fine: hail and rain P.M. 31. Cloudy.

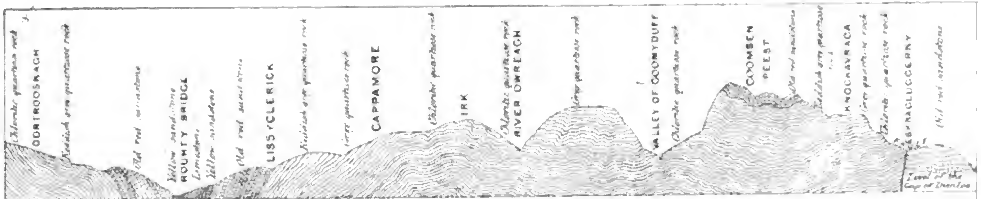
Applegarth Manse, Dumfries-shire.—Dec. 1. Clear and sunny. 2. Calm and clear: hard frost. 3. Dull: raw day. 4. Dull, but dry. 5. Frost A.M.: thaw P.M. 6. Dull moist day: hoar frost early A.M. 7. Dull and cloudy, but dry: hoar frost A.M. 8. Fine day: slight hoar frost. 9. Quiet and cloudy. 10. Quiet and cloudy: freezing P.M. 11. Fine day: wind rose P.M. 12. Heavy rain in the afternoon. 13. Fine morning: rain afternoon. 14. Moderate day: slight frost preceding night. 15. Foggy: slight frost preceding night. 16. Fine: slight frost early A.M. 17. Raw and cold. 25. Stormy day of wind and rain. 26. Clear day: frosty morning. 27. Snow in the evening, and frost all day. 28. A little more snow: hard frost P.M. 29. Clear and calm and frosty. 30. Looking dull A.M.: sleet and rain P.M. 31. Heavy rain all morning: cleared up P.M.

Sun 20 days. Rain 9 days. Snow 3 days. Frost and hoar frost 13 days.

Wind easterly 13 days. North-east 4 days. Southerly 4 days. Westerly 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc.	Chiswick.		Boston.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston.	Dumfries-shire.		London: Roy. Soc.	Chiswick	Bost.	Dumfries-shire.	London: Roy. Soc.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc.
Dec.	9 a.m.	Max.	Min.	8½ a.m.	9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.	8½ a.m.	Max.	Min.	9 a.m.	1 p.m.			9 a.m.				9 a.m.
1.	29·708	29·810	29·724	29·30	29·51	29·63	37·7	38·4	37·6	41	29	34	40	34½	E.	SE.	calm	ENE.	...	...	...	...	38
2.	29·882	29·992	29·890	29·54	29·78	29·90	38·8	39·4	37·2	42	33	37	36½	28½	E.	NW.	calm	E by N.	...	...	...	...	36
3.	29·960	29·985	29·962	29·61	29·92	29·89	38·8	39·4	35·8	39	27	39	44	31	S.	SW.	calm	NE.	...	...	·11	...	38
4.	29·830	29·963	29·835	29·48	29·78	29·82	34·3	34·6	32·0	38	30	35	36½	34½	NNW.	N.	calm	E.	·038	...	·08	...	34
5.	30·096	30·487	30·124	29·67	29·92	30·06	36·7	37·2	33·3	37	28	34	36	27½	SW.	NW.	calm	NE.	...	...	...	...	33
● 6.	30·364	30·508	30·397	29·94	30·19	30·15	36·7	37·3	36·0	37	32	32	38½	32	W.	S.	calm	NE.	...	·01	·10	0·00	35
7.	30·332	30·366	30·239	29·95	30·10	30·05	34·9	35·3	34·2	42	28	35	35½	30½	E.	SE.	calm	E.	...	...	...	...	33
8.	30·096	30·124	30·014	29·83	30·00	29·97	35·4	35·9	33·4	35	31	36·5	36½	31½	E.	E.	calm	E.	...	...	...	...	32
9.	29·830	29·860	29·712	29·60	29·81	29·65	34·9	36·0	34·7	37	32	35	36	32	NW.	NE.	calm	ENE.	...	...	...	...	31
10.	29·606	29·651	29·637	29·35	29·50	29·45	34·2	36·7	34·2	42	34	34	37	34	E.	E.	SW.	E.	...	...	...	...	31
11.	29·664	29·598	29·326	29·24	29·45	29·33	42·4	43·0	34·3	46	39	41	42	32	E.	SE.	calm	ENE.	...	·09	·14	...	37
12.	29·248	29·286	29·190	29·	29·18	29·10	44·8	45·3	41·9	48	35	42	45	39	E.	S.	E.	E.	·083	·10	·03	..	39
☽ 13.	29·294	29·311	29·277	28·87	29·10	29·09	43·3	46·4	41·3	48	42	38	46	38½	S.	S.	calm	E.	·061	·16	·09	...	40
14.	29·230	29·400	29·247	28·79	29·07	29·30	44·2	44·4	42·3	49	30	43	40½	40	S.	SW.	calm	S by W.	·250	...	...	0·48	39
15.	29·590	29·596	29·427	29·15	29·40	29·42	39·3	40·0	38·8	43	41	36	42	32	SW.	SE.	calm	S.	...	·09	...	...	36
16.	29·696	30·083	29·713	29·20	29·63	29·89	40·3	40·5	38·8	44	30	36	40	32	W.	SE.	calm	SSW.	·122	...	...	...	38
17.	30·074	30·094	29·868	29·64	29·89	29·63	38·3	38·8	37·4	44	31	34	38	32½	NE.	SE.	calm	E by S.	...	·28	...	...	37
18.	29·516	29·530	29·376	29·17	—	—	35·2	35·5	34·7	42	41	38·5	—	—	E.	SE.	E.	—	...	...	...	...	32
19.	29·418	29·426	29·356	28·98	—	—	47·7	48·4	34·7	55	50	46	—	—	S.	S.	calm	—	·333	·27	·29	...	40
○ 20.	29·282	29·480	29·262	28·82	—	—	51·3	51·7	47·3	55	46	51·5	—	—	S.	S.	calm	—	·322	·06	·12	...	46
21.	29·552	29·566	29·561	28·96	—	—	47·3	47·7	46·6	53	46	46	—	—	S.	SW.	calm	—	·180	·20	·06	1·56	45
22.	29·550	29·547	29·376	28·95	29·99	—	49·9	50·6	46·8	55	47	47	—	—	SW.	SW.	calm	—	·061	·06	·01	...	49
23.	29·586	29·586	29·333	28·95	—	—	48·6	49·3	48·4	57	48	47·5	—	—	S.	S.	W.	—	·125	·47	·11	...	48
24.	29·244	29·416	29·302	28·64	28·73	—	53·4	54·6	48·5	54	44	55	—	—	S.	SW.	W.	—	·288	·01	·35	...	52
25.	29·528	29·568	29·530	28·39	29·02	29·43	47·7	49·4	45·9	48	28	42	—	—	S.	W.	W.	W.	...	...	...	...	48
26.	29·732	29·728	29·322	29·28	29·49	29·45	40·7	41·2	38·3	54	38	34	37	30	S.	E.	calm	E.	...	·39	...	...	42
☾ 27.	29·342	29·577	29·386	28·90	29·30	29·42	47·2	47·8	39·4	46	29	42	34	29½	W.	NW.	calm	NE.	·305	·13	·18	...	45
28.	29·752	30·062	29·759	29·30	29·52	29·79	35·7	36·0	35·8	39	24	33	34½	21½	NW.	W.	calm	WNW.	·158	...	...	1·63	40
29.	30·280	30·440	30·320	29·81	30·08	30·26	33·3	33·5	32·2	40	21	32	35	26	W.	W.	calm	NW.	...	...	...	...	33
30.	30·398	30·436	30·163	30·08	30·14	29·80	32·3	35·2	32·0	44	34	29	35	21½	SW.	SE.	calm	ESE.	...	...	...	...	33
31.	29·924	29·975	29·727	29·45	29·41	29·33	44·8	45·3	32·0	52	45	42	44½	34	S.	S.	S.	SSW.	...	...	·10	...	38
Mean.	29·729	29·821	29·656	29·28	29·58	29·65	41·0	41·8	38·4	45·35	31·96	38·9	38·7	31·4					Sum. 2·326	2·32	1·77	3·67	Mean. 38·6

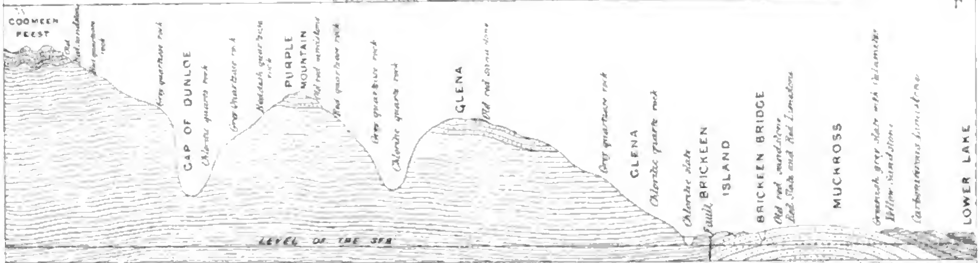


Section extending from Roughty Bridge in the Valley of Kenmare in a northern direction across the Coomeen Peest range of Magillicuddy's Reeks to the Gap of Dunloe

Fig. 1.

Longitudinal Scale ½ inch to a Statute mile

Vertical Scale —— 5000 feet to an inch



Section extending from the Coomeen Peest range of Magillicuddy's Reeks in an eastern direction to Brickeen Island in the Lake of Killarney

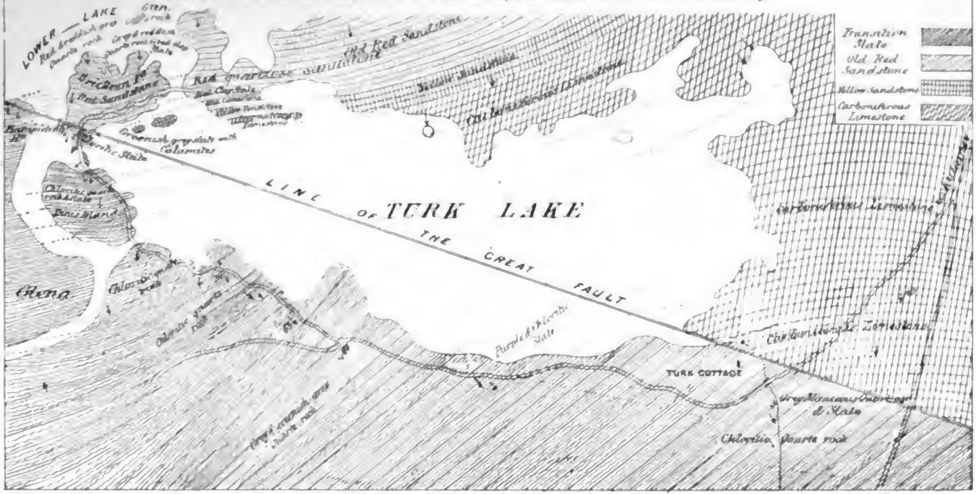
Fig. 2.

Longitudinal scale one inch to a statute mile

Vertical Scale —— 2000 feet to an inch

J. Basire lith.

Plan shewing the relative positions of the strata on the north and south sides of the Great Fault where it crosses Glena, Brickeen Island, Turk Lake, &c. at Killarney.



J. Basire lith.

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[THIRD SERIES.]

MARCH 1840.

XXX. *On the true Order of Succession of the Older Stratified Rocks in the Neighbourhood of Killarney and to the North of Dublin.* *By* Richard Griffith, *Esq., F.G.S. L., and President of the Geological Society of Dublin.*

[See Section Plate II. and Plan Pl. III.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE Geological map of Ireland, which presents an epitome of my geological labours in the field, continued at intervals for upwards of thirty years, is now before the public, and has in fact become their property. Though I do not venture to assert that all the lines representing rock boundaries which it contains are perfectly correct, still I will say that none have been laid down without consideration. Much detail no doubt still requires to be worked out within the great divisions, particularly in the carboniferous limestone series, and many of the smaller greenstone protrusions have yet to be pointed out; but I am fully of opinion that the great lines nearly represent the true boundaries of the several rock formations, according to the order of superposition indicated by the table of geological colours. Having this impression on my mind, I confess I was somewhat startled by observing in the Number of the Philosophical Magazine for December last, (vol. xv. p. 442.) a paper communicated by my friend Mr. Charles William Hamilton of Dublin, which contains statements, some of which are illustrated by sections, relative to the geological positions of the strata of several parts of Ireland, quite at variance with those assigned by me to the same rocks in the geological map. This paper also asserts that I have indicated the occurrence of rocks in certain places in which no such rocks are to be found. These are charges which I think it incumbent on me shortly to reply to; and I hope to be able to show that my map is correct, and that Mr. Hamilton is incorrect in every case in which he has thrown a doubt on its accuracy.

The main points of difference between Mr. Hamilton and myself are; 1st, he is opinion, that in the county of Kerry, south of Castlemaine Bay and the lower Lake of Killarney, the old red sandstone overlies *unconformably* those schistose rocks which in my map are comprehended under the general name of transition, and which include the Silurian system and the older or Cambrian slate*.

2nd, That the old red sandstone strata of the Gap of Dunloe extend uninterruptedly in a southern direction from the gap to the summit of MacGillacuddy's Reeks, from which point they dip to the south, and are *succeeded* conformably by a new series of rocks which Mr. Hamilton considers to belong to the Devonian system.

3rd, That the band of yellow sandstone shown on the geological map as underlying the carboniferous limestone in the valley of the River Roughty at Kenmare, does not exist there.

In illustration of these views Mr. Hamilton has given two sections, the first of which extends from the gap of Dunloe in a south-eastern direction across Toomies and Glena Mountain to the middle or Turk Lake of Killarney, and thence over Turk and Mangerton Mountains to the valley of Kenmare; the second is a representation of Mr. Hamilton's view of the strata as they appear on the west side of the gap of Dunloe.

In both of these sections Mr. Hamilton has represented the old red sandstone as resting *unconformably* on the older schistose rocks, which he calls "Cambrian;" but I state without fear of contradiction that his section and statements are incorrect in this respect, and that in the locality in question the old red sandstone has been deposited *conformably* on the older slate, and in a descending order graduates imperceptibly into that rock. This is also the opinion of Mr. Weaver, who considers the whole to belong to the transition series; and Capt. Portlock, in his presidential address to the Geological Society of Dublin, appears to entertain the same opinion.

In the first volume of the Journal of the Geological Society of Dublin, page 285, Mr. Hamilton describes the old red sandstone as forming "an anticlinal axis on the summit of

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* See note appended to the large Geological Map of Ireland.

Currawntoohill mountain*, from whence dipping to the southward it is covered by siliceous sandstones of a greenish or brownish colour;" and this view is illustrated by a section given in Plate I. fig. 3, which, together with those contained in your Magazine, exhibit the whole of his views on the subject.

For the sake of clearness, I have also prepared two sections passing nearly through the same line of country as those given by Mr. Hamilton, from which it appears that the old red sandstone on the summit of MacGillacuddy's Reeks, rests *conformably* on the schistose rocks of the gap of Dunloe, which Mr. Hamilton terms Cambrian; and that the apparent unconformability which is visible nearly in the centre of the gap between the chloritic quartz rock and the old red sandstone, has been occasioned by a great north-west and south-east fault, which crosses the gap of Dunloe nearly at right angles, and extends from thence in a south-eastern direction along the northern declivities of the Purple, Toomies, and Glena Mountains, from whence in continuation it reaches the lower Lake of Killarney, near the Glena Cottage Banqueting House, thence it crosses Brickeen Island, and passing through Turk Lake reaches the north base of Turk mountain.

This fault may be said to form the key to the geology of the Killarney district, as it explains the apparent anomaly deducible from the persistent dip to the south of the strata on both sides of Glena Cottage and Brickeen Island, which might lead, and has led incautious observers to infer that the strata belonging to the transition slate series which occur to the south of the fault rest conformably on the top of the old red sandstone of Brickeen Bridge, of the northern part of Brickeen Island, and likewise on the carboniferous limestone east of Turk Cottage.

In expressing my view of the geology of the district, I shall commence with the west side of the Gap of Dunloe; but here my section, Plate II. fig. 1, is so different from Mr. Hamilton's, that it is with difficulty we can recognise them as being intended to represent the same locality; yet such is the case. Of the accuracy of my own section I entertain no doubt, having made it with great care.

In taking a sectional view of the strata as exhibited on the west side of the Gap of Dunloe, the first beds visible at the surface consist of a reddish gray quartzose rock belonging to the old red sandstone formation. These are succeeded by a series of thick beds, of coarse-grained conglomerate composed of rounded pebbles of white quartz, varying in size from two inches to a quarter of an inch in diameter, imbedded in a reddish gray arenaceous base. The conglomerate is succeeded by coarse-grained brownish-red slate, which is occasionally quarried and used for inferior roofing slate; these strata are followed by a series of beds, consisting of red quartzose sandstone alternating with coarse slate, the sandstone beds presenting occasionally a conglomeritic character, but the pebbles rarely exceed half an inch in diameter. The strata dip to the west, though irregularly, at an average angle of about 10° from the horizon; and consequently in ascending the glen in a southern direction, the cliffs present the outgoing or strike of the beds, which are not horizontal, but exhibit a tortuous arrangement presenting frequent undulations from north to south. At Esknagluggerny, a short distance beyond the southern extremity of Coosane lake, the old red sandstone strata are cut off by the great north-west and south-east fault already mentioned, immediately to the south of which thick beds of green chloritic quartz rock appear at the surface, dipping to the south at an angle of 30° from the horizon. These strata probably form the *lowest* portion of the transition rocks situated to the south of Castlemaine Bay, as we perceive the whole series to be complete in an ascending order from them to the summit of the Reeks.

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* The highest of the Reeks.

Commencing then with this chloritic quartz rock base, and proceeding in a southern direction, we find that the same rocks continue to dip to the south, and present an accumulation of strata for upwards of 500 feet in thickness, varying little in their composition or character, with the exception of an occasional interstratification of thin beds of green and purplish gray clay slate. These slates are quite distinct in character, as well as in colour and composition, from the coarse red slate of the old red sandstone; they are in fact identical with the Valentia slates, and bear a strong resemblance in colour, composition, and lithological character to some of those of North Wales. Still ascending in the series, we find that the colour of the rocks gradually changes from green to gray, and at length the mineral chlorite is altogether wanting. These gray quartzose beds are not so thick as those which contain chlorite; they likewise alternate with thin beds of clay slate, which present a purplish gray colour, without any admixture of green. The gray strata may amount altogether to about 800 feet in thickness. Still continuing to ascend, the same character and alternations are preserved as the last described, but on a fresh fracture the quartzose beds present a slight bloom or tint of red, and the disintegrated surface of the rock exhibits a decidedly reddish hue, which is not visible lower down: the reddish gray strata alternate as before with purplish gray slate. As we ascend and approach the summits of the Coumeen Peest, or eastern ridge of the Reeks, the strata assume a more decidedly red character, till at length they pass into brick or cherry-red quartz rock, and contain some beds of conglomerate, identical in colour, composition, and structure with the old red sandstone already described, situated to the north of the fault in the Gap of Dunloe, but not quite so coarse-grained*. These red quartzose or old red sandstone beds differ materially from the schistose beds of the lower part of the series. The structure of the rock is decidedly granular, the strata are thinner, and they are divided by joints into rectangular masses, while the schistose beds beneath usually present rhomboidal forms.

The conglomerate on the top of the Reeks is perfectly conformable with the underlying strata, and in fact a regular gradation may be traced from the lower or chloritic portion of the series through the gray and reddish gray into the brick-red quartz rock and conglomerate.

From the summit of the Reeks still proceeding in a southern direction towards the valley of Kenmare, we do not find these old red sandstone strata dipping to the southward, as shown on Mr. Hamilton's section already mentioned, and published in the Journal of the Geological Society of Dublin, but on the contrary they crop out to the southward, forming a regular cap resting conformably on the inferior strata, whose ends appear in the precipitous escarpment visible on the northern side of the valley of Coomyduff, in the bottom of which the green chloritic beds already described as occurring near the fault in the Gap of Dunloe again make their appearance, and the whole succession of the strata and passage from the green chloritic beds to the red conglomerate as already described on the northern acclivity of the Reeks, may likewise be traced on the southern.

It is unnecessary to enter into any further particulars respecting the detail of the succession of the strata between the valley of Coomyduff, and the reappearance of the old red sandstone at Lisinisky, to the north of the valley of Kenmare, as it is clearly shown in the section, and the same gradation of colour and character, from the green to the red rocks, is observable. See Plate II. fig. 1.

The old red sandstone of the valley of Kenmare consists of red quartz rock and red clay slate, the quartz rock predominating; but no beds of true conglomerate have been observed, though such may be discovered on a more careful examination.

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* The conglomerate visible near the summit of Lisbug mountain on the western ranges of the Reeks, is fully as coarse-grained as that of the Gap of Dunloe.

Ascending in the series, and approaching the limestone of the valley of Kenmare, the red strata become more schistose and chiefly consist of coarse red clay slate, which, approaching the limestone, is observed to alternate with yellowish green clay slate and red limestone in thin beds. These are succeeded by strata of gray quartzose sandstone containing calamites, the characteristic fossil of the yellow sandstone series, the upper beds of which alternate with greenish gray and dark gray clay slate, with occasional beds of gray limestone. Still ascending, the limestone gradually predominates, till at length the slate disappears, and the whole stratification is composed of carboniferous limestone.

To the south of the river Roughty, in a descending order, a similar series to that above described appears at the surface dipping to the north, so that we have again the dark gray slate and limestone, the yellow sandstone with calamites, the red limestone, the old red sandstone consisting of red slate and red quartz rock, and in continuation the whole suite of the schistose strata of the transition series already described.

Having described this section in detail, I again assert, that the old red sandstone to the south of Castlemaine Bay has been deposited conformably, on the underlying strata, and that the apparent unconformability noticed by Mr. Hamilton as occurring in the Gap of Dunloe, arises from a dislocation of the strata occasioned by a fault, and not from *original deposition.*

Owing to the want of fossils throughout the entire succession of rocks above described, with the exception of the yellow sandstone and carboniferous limestone of the valley of Kenmare, it is difficult to determine the position in geological precedence which should be allotted to the green chloritic quartz rock which forms the basis of the district under consideration, or to determine at what precise point the old red sandstone should be said to commence. It appears to me that the only key which is likely to unravel this mystery, will be found in the peninsula of Dingle, to the north of Castlemaine Bay, where fossils have been discovered in the strata, which have been recognised by Mr. James Sowerby as belonging to the upper Silurian rocks of Murchison.

The fossils consist of

From *Ferriter's Cove.*

Atrypa tenuistriata, Sil. Syst. pl. 12. f. 3.
Avicula reticulata, *Ib.* pl. 6. f. 3.
——— retroflexa, *Ib.* pl. 5. f. 9.
Cornulites serpularius, *Ib.* pl. 26. f. 5-9.
Euomphalus funatus, *Ib.* pl. 12. f. 20.
Euomphalus perturbatus, Sil. Syst. pl. 22. f. 15.
Leptæna lata, *Ib.* pl. 5. f. 3.
Terebratula bidentata, *Ib.* pl. 12. f. 13.
———— Stricklandii, *Ib.* pl. 13. f. 19.

If we make a section across the Dingle peninsula from Feilaturrive on the south to Brandon Bay on the north, we find that the strata consist of a base of dark blackish gray clay slate, the upper beds of which alternate with reddish purple slate, some of which contain Silurian fossils; these strata are succeeded by red slaty conglomerates, alternating with red and green slate and brown quartz rock, above which are chloritic quartz rocks with alternating purplish and reddish gray clay slate, similar in composition and character to those of the Gap of Dunloe, and of that district generally. Again, if we make a section along the west coast of the Dingle peninsula from Fawn to Sybil Head, we find numerous fossiliferous beds, some of which have been noticed by Mr. Hamilton*, the true position of which has not yet been clearly made out; but from all the data I possess, I am inclined to place them between the dark gray clay slate and the chloritic quartzose rock. Hence I am of opinion that the chloritic rocks of the Gap of Dunloe should be classed with the Silurian system.

With regard to the dark blackish gray clay slate which forms the lowest member of the series in the Dingle peninsula, it is identical in lithological character with the dark gray slate of the Gatties mountains, which are situated directly in the line of the strike in an eastern direction, as may be clearly seen by reference to my Geological map; and following the same strike similar strata occur in the Shivnamanna mountains of the county of Kilkenny, and also in the slate district to the south of Waterford, where on the sea coast at Knockmahon, and also at Tramore, fossils occur in green chloritic metamorphic slate and quartz rock, belonging to the lower Silurian strata or Caradoc sandstone†. These fossiliferous beds are incumbent on the dark gray slate, in which no fossils have as yet been observed. This dark gray slate bears a striking resemblance in lithological character to the dark gray clay slate of the Festiniog district of Merionethshire; and I am led to conclude, that the whole of the transition slate of the south of Ireland is high in the series generally, and that the principal part belongs to the Silurian system.

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* See Journal of the Geological Society of Dublin, vol. i. p. 280.

† The fossils discovered are *Orthis radiatus*, and several others of that genus; so *Bellerophon bilobatus*, *Atrypa orbicularis?*, &c.

Our next point for consideration with regard to the district of Kerry, south of Castlemaine Bay, will be to determine where, in the graduating series between the chloritic quartz rocks and the decided red conglomerate, we should draw the line separating the Silurian system from the old red sandstone. If we again refer to Dingle peninsula, we find that the summit of Cahirconree mountain, together with many other mountains of the district, is composed of rocks belonging to the old red sandstone, which rest *unconformably* on the dark gray clay slate above-mentioned, and on the other schistose rocks which succeed it. From this unconformability, it would appear that the series of the schistose strata is not complete in the peninsula of Dingle, and that it wants the upper members which do occur to the south of Castlemaine Bay. If we examine the old red sandstone series as exhibited in the Dingle district, we find that the under beds consist of rather fine-grained, red quartzose sandstone, for a thickness varying from 100 to 150 feet in different localities; the sandstone is succeeded by thick beds of coarse conglomerate alternating with coarse red slate, and these in continuation by alternations of red and brown quartzose sandstone and coarse red slate.

Now if we compare the red strata which rest conformably on the schistose rocks on the summits of the Reeks, the Purple mountains, &c. we find red granular quartz rock in thin beds underlying the conglomerates; and considering these beds, as in the Dingle peninsula, to represent the lower portion of the old red series, I have drawn the line where the red quartz rock terminates, and where a change is indicated by an alteration in the colour of the rock, which becomes light reddish gray, and the strata present a schistose, instead of a perfectly granular structure.

As a further proof of my opinion of the inaccuracy of Mr. Hamilton's views respecting the order of succession of the strata of the Killarney district, I shall now very shortly describe the succession of rocks visible at the surface on the north and south sides of the great fault where it traverses the strata at Glena and Brickeen Island on the lower lake of Killarney, which fault, as already mentioned, extends in a south-eastern direction from the Gap of Dunloe, by the localities in question, to the northern base of Turk mountain, &c. If we trace the line of the fault on the shore of the lake immediately to the north of Glena Banqueting House, and also where it crosses Brickeen Island, it will be seen that the strike of the chloritic quartz and slate beds on the south side of the fault is unconformable with the strike of the old red sandstone and carboniferous limestone on the north of it, and that the ends of the strata of both formations abut obliquely against the opposite sides of the fault. This fact will be at once understood by reference to the plan given in Plate III.

The lower portion of the strata on the north side of the fault consists of old red sandstone: commencing near the shore of the lower lake at Cullinagh to the east of O'Sullivan's Cascade, we have a succession of beds of coarse-grained red conglomerate, similar to those already described as occurring at the northern entrance of the Gap of Dunloe; these strata, which dip to the east, are succeeded by alternating red and light gray quartzose beds, which in an ascending order continue to Glena Bay, the dip gradually changing from the east towards the south. At Glena Bay we fall into the line of the strike of the strata of the Gun rock, a small island situated immediately to the west of Brickeen Island, near Brickeen Bridge. At the Gun rock the strata consist of light gray quartzose rock alternating with thin beds of reddish gray limestone, which dip to the south at an angle of 40°. On the north point of Brickeen Island, these strata are succeeded by alternations of red and reddish gray quartzose rock, red slate, and red limestone, the general dip being 25 E. of S. at an angle of 35°. These strata abut obliquely against the fault to the north of the Banqueting House of Glena, and also where it crosses the western portion of Brickeen Island.

Still ascending in the series, the above-mentioned strata are succeeded by alternations of red clay slate, yellowish green clay slate, and red limestone, which may be considered to form the upper portion of the old red sandstone series: above them we find a succession of beds, consisting of coarse greenish gray slate containing calamites, greenish gray calcareous slate, and impure gray limestone, succeeded by beds of gray quartzose sandstone with partings of black clay slate, and alternations of gray limestone. These strata form the lowest portion of the carboniferous limestone series, to which I have given the name of the yellow sandstone*. Above the yellow sandstone we have the black carboniferous slate, which here alternates with gray limestone, the slate as at Kenmare predominating near the commencement; but as the beds accumulate, the calcareous strata increase in thickness, and at length near the old copper mine of Muckross, the slate beds entirely disappear, and the entire stratification consists of limestone, some beds of which are fossiliferous, and contain *Producta depressa*, variety of the mountain limestone *Spirifera bisulcata*, *Spirifera resupinata*, and many of the other fossils which usually occur in the lower limestone.

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* In most localities the colour of the sandstone is yellowish gray, and the siliceous rock forms by much the most important feature in the series.

From Muckross mine the calcareous strata continue without interruption, dipping to the south at an angle of 40°, to the base of Turk mountain, a quarter of a mile to the east of Turk cottage, and are still visible at the surface close to the line of the eastern continuation of the fault which is there concealed from view by diluvial matter.

I have been thus particular in describing the strata visible on the north side of the fault at Brickeen Island and Muckross, as they present one of the most perfect sections which Ireland affords of the entire suite of the old red sandstone, the yellow sandstone, the carboniferous slate, and the lower carboniferous limestone, and which by their variety form a strong contrast with the uniform character of the chloritic quartzose strata visible on the south side of the fault, which strata evidently belong to the same series as those which occur in a similar position at the Gap of Dunloe, and are in fact a continuation of the same strike. No doubt can therefore be entertained that these inferior strata form the lower portion of the schistose series of the district to the south of Castlemaine Bay, that they are identical with the chloritic rocks of the Gap of Dunloe, and do not belong to the Devonian as supposed by Mr. Hamilton *.

Proceeding to the southward from Turk mountain, the succession of rocks is similar to that already described as occurring above the chloritic beds of Dunloe: the strata undulate very much, and present several synclinal and anticlinal axes, so that the upper beds or reddish gray quartzose strata never appear on Turk or Mangerton mountains, and consequently in this line we do not meet with the old red sandstone strata till we approach the northern boundary of the valley of Kenmare, where it occurs in a line parallel to that already noticed in describing the section between the Gap of Dunloe and the valley of Kenmare. It is true, Mr. Hamilton mentions the occurrence of old red sandstone in the centre of Mangerton mountain, but in that locality he may have mistaken the purple slate which there alternates with the chloritic quartz for that rock.

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* A section similar to that from the Gap of Dunloe to the summit of the Reeks may likewise be traced from the chloritic rocks, south of the fault at Brickeen Island, up to the old red sandstone on the summit of Glena mountain, as represented in Plate III. fig. 2.

It appears to me to be extraordinary, that when arriving at the conclusion that the chloritic rocks of Turk and Mangerton mountains were newer than the old red sandstone, owing to the observed dip to the south both of the old red sandstone and the chloritic rocks, Mr. Hamilton did not (with Mr. Weaver) consider these chloritic rocks to be superior to the carboniferous limestone of Muckross and Turk. The limestone beds all dip to the south towards the fault, and apparently underlie the chloritic rocks; and as the limestone strata rest upon the yellow and red sandstone, the natural conclusion should have been, that the whole of the strata to the south of the lakes of Killarney belonged to the *millstone grit*, which we find resting on the same limestone to the north of the lakes. Mr. Weaver carefully observed all these dips; and not having noticed the fault or unconformity of the strike of the old red and limestone series on its north side with the chloritic rocks on the south, and conceiving that the limestone together with the old red sandstone belonged to the transition series, he naturally concluded that the chloritic rock to the south belonged to the same; but Mr. Hamilton has overlooked the southern dip of the limestone at the northern base of Turk mountain east of Turk cottage, and placed the chloritic rocks in a position in which they could not occur according to the dips of the strata, namely, between the old red sandstone and the carboniferous limestone series.

I shall not pursue this subject, as I should hope that sufficient data have been brought forward to prove that the strata to the south of the Lake of Killarney, which Mr. Hamilton considers to be Devonian, do really belong to the Silurian system.

I shall next allude to a paragraph in Mr. Hamilton's paper, page 445, in which he states, "that among other localities in which the yellow sandstone is laid down on the Geological map *in positions in which it does not exist*, he may mention the boundary of the carboniferous limestone, on the banks of the River Roughty in the Valley of Kenmare."

I wish Mr. Hamilton had mentioned the other instances that came within his knowledge as well as this, as I have no doubt I should have been equally well able to show that the boundaries marked were founded on actual observation.

In the present case I shall merely observe, that at Kilgawan, on the north side of the valley of Kenmare, above Roughty Bridge, yellow sandstone occurs in considerable thickness, overlying red quartz rock, green and red clay slate, and red limestone, and underlying black carboniferous slate, interstratified with thin beds of limestone, which is ultimately succeeded by limestone without admixture; the whole of the strata dipping to the south. Again, on the south side of the river Roughty, immediately to the east of Roughty Bridge, a similar succession of strata is observed dipping to the north. In this locality, the yellow sandstone contains that variety of calamite which is characteristic of the rock, and which is also abundant at Brickeen Island, near Killarney, and in the same geological position, underlying the limestone of the several troughs of the counties of Waterford and Cork. In the carboniferous slate of Roughty Bridge, *Retepora membranacea* was observed. I shall mention one other locality in which the strata in connexion with the yellow sandstone have been observed in the valley of the river Roughty, namely, at the pier at Kenmare. At low water in this place, gray quartz rock and black carboniferous slate may be observed dipping to the north under the lower beds of the carboniferous limestone, which are exposed to view in an adjoining quarry. Immediately to the south of this quartz rock and slate, no rocks are visible, the strata being concealed by sand; but in a very short distance, beds of yellowish green slate, alternating with red slate and red limestone, occur which are identical with the strata visible in the localities already mentioned underlying the yellow sandstone: no doubt can therefore be entertained that this rock is continuous on the south side of the valley from Kenmare pier to Roughty Bridge. It is true, in this locality, as well as in most others, that the yellow sandstone has not been seen at the surface throughout the entire length of the carboniferous limestone trough, as, owing to a thick covering of diluvial matter, or of bog, the precise boundary between the base of the limestone series and the old red sandstone rocks is rarely visible; but as the yellow sandstone and dark gray carboniferous slate do occur in every place where the outer boundary of the limestone series is exposed to view, I feel little doubt that these rocks equally occur in those positions where they are concealed from our view. If geologists were only to mark the limits of their rock districts in the precise localities in which the contacts are visible, no geological map could be formed. In maps on a large scale, the observed contacts might be shown by continuous, and the supposed by dotted lines: but on a general map, though desirable, it would be impossible to enter into such detail, or if attempted it would be impracticable, on a map on which the features of the country are shown, to distinguish between the continuous and the dotted lines.

I shall next very shortly allude to another point brought forward by Mr. Hamilton in his paper. In speaking of the district coloured old red sandstone on my large geological map which occupies extensive tracts in the counties of Waterford and Cork, he observes, "As to classification, it appears to me that Mr. Griffith has thrown together two rocks which are very distinct, namely, 1st, The old red sandstone and its conglomerates; 2ndly, Compact arenaceous rocks, agreeing in geological position with the upper part of the Devonian series, as described by Professor Sedgwick and Mr. Murchison."

It appears to me that Mr. Hamilton is mistaken in separating the old red sandstone from the Devonian system, as Professor Sedgwick and Mr. Murchison include the whole series under the general term Devonian: consequently, whether we apply the term Devonian, or old red sandstone, to the system, no advantage could be derived from its arbitrary subdivision.

I freely admit that doubts may be entertained as to whether the yellow sandstone and carboniferous slate, considered by me to belong to the lowest portion of the carboniferous system, should not be placed at the top of the old red sandstone or Devonian system. This however is not a question of position, but of fossils; and as far as my present information extends, I still feel inclined to adhere to my original view, founded on the fact, that in ascending in the old red sandstone series the first alteration observed is a change from *red and yellowish green clay slate,* to yellow quartzose sandstone passing into light gray, which contains in abundance the peculiar variety of calamite already mentioned. Below this sandstone no fossil remains have been discovered; above it they occur in abundance, and the upper beds which alternate with gray clay slate and limestone contain fossils which undoubtedly belong to the carboniferous limestone, though doubts may be entertained respecting a few which occur towards the bottom of the series.

Should it appear on a more careful and extended examination that some of the fossils of these lower beds are similar to those which occur in the upper part of the Devonian system, difficulties will still arise as to where in a graduating series the line should be drawn separating the Devonian from the carboniferous system; and as far as Ireland is concerned, I think I have adopted that which appears to be least liable to objection; but even on this point I am open to conviction.

In the mean time the fact to dwell upon is, that the mineral succession which I have pointed out beneath the carboniferous limestone of Ireland, is similar to that described in North Devon by Professor Sedgwick and Mr. Murchison.

In the conclusion of his paper Mr. Hamilton observes, "that all those tracts which occur between Dublin and Dundalk, along the course of the Boyne, and in the hills separating the counties of Cavan and Meath, which have been described as the older graywacke or transition series by Mr. Griffith, Mr. Weaver, and others, are in reality all conformable, and immediately inferior to the mountain limestone and superior to the old red sandstone, and consequently belong to the Devonian series."

I confess I am surprised at the view here taken by Mr. Hamilton, as one of the facts on which his argument is grounded, namely, that the rocks coloured by me as transition are superior to the old red sandstone, have been correctly stated by himself* to be inferior to that rock where it occurs near Balriggan mill, N.W. of Dundalk, in the county of Louth, in which locality the old red sandstone rests unconformably on the transition slate.

In regard to the second point, namely, that the schistose rocks are succeeded by the limestone in a conformable position, I have to observe that such is not the fact; for in the only localities in which I have been hitherto enabled to observe the contact of the two rocks, the limestone rests unconformably on the transition slate. These localities are in the river north-east of the Naul, in the county of Dublin; at the southern extremity of the village of Duleek, in the county of Meath; at Old Bridge on the banks of the Boyne, two miles west of Drogheda, in the county of Louth; and at Headfort near Kello, in the county of Meath.

Fortunately, in addition to these facts, we have also another, which is quite conclusive, namely, the discovery of fossils belonging to the lower Silurian rocks or Caradoc sandstone, which occur in considerable abundance at Grangegeeth, four miles north of Slane, in the county of Meath: the fossils have recently been examined by Mr. Murchison and Mr. Lonsdale, of the Geological Society of London, to whom I had sent them, and both are of opinion that they belong to the lower Silurian rocks †. Consequently we must come to the conclusion that Mr. Hamilton's opinion is erroneous in respect to the geological position of the slate district, north of Dublin, and of that between Drogheda and Dundalk.

I have now replied to all the important observations contained in Mr. Hamilton's paper, which tend to cast a doubt on the accuracy of my Geological map, and I think I have been able to show, in each case, that my original views have been supported by a careful *re-examination* of the facts.

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* See Journal of the Geological Society of Dublin, vol. ii. part i.

† The fossils found, and which have been compared with the original Silurian forms collected by Mr. Murchison, are *Orthis semicircularis* and *Orthis virgata*; in addition to which there are several other forms of the genus Orthis which have not as yet been clearly identified.

Dublin, January 17, 1840.

XXXI. *On Haydenite and Couzeranite.* *By* H. J. Brooke, *Esq., F.R.S.*[*]

IN the last Number of the Phil. Mag. p. 156, I observed a notice of a new mineral (Beaumontite) which M. Levy has found accompanying Haydenite, a mineral that is said to occur near Baltimore in the United States; and M. Levy remarks that I have classed Haydenite with Heulandite without assigning any reason for so doing.

I did so from perceiving that the small and brilliant yellowish crystals on the specimen I examined resembled Heulandite, in having a nacreous plane in only one direction, corresponding with the P of W. Phillips (Mineralogy, p. 39. Ed. 1823); in the form, as far as I could distinguish it in the minute and closely aggregated condition of the crystals; and in the near agreement, about 112°, of the angle between the nacreous plane and a plane appearing to correspond with the *a* of W. Phillips.

M. Levy is doubtless aware of a variety of Heulandite found at Arendal, of a brown or yellowish brown colour, in small bright crystals, and accompanied by stilbite in globularly-radiated concretions of a dull yellowish colour. The specimen sent to me from America as Haydenite has a similar accompaniment of stilbite of the same description, and this circumstance tended to confirm my impression that Haydenite was Heulandite. It is probable, therefore, that the specimen sent to me from America as Haydenite, is really not that mineral, and I do not find anything like M. Levy's Beaumontite upon it, except that the crystals are "small and brilliant, and of a pearly lustre." Mr. L. does not say on what faces this lustre appears in Beaumontite.

I find I have been formerly led into an error relative to Couzeranite by specimens received from Paris, by Mr. Heuland, from Mr. Pentland. On examining the crystals I found them to be felspar, and I accordingly stated, on the faith of the specimens so transmitted to Mr. Heuland being genuine, that Couzeranite was only felspar. I have since seen other specimens named Couzeranite in apparently square prisms, and if these are the true mineral it has no resemblance whatever to felspar.

H. J. B.

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[*] Communicated by the Author.

XXXII. *Observations on the relative Temperature of the Sea and Air, and on other Phænomena, made during a Voyage from England to India. By the Rev.* J. H. Pratt, *M.A., Member of the Asiatic Society of Bengal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I SEND you the accompanying observations on the relative temperature of the sea and the superincumbent air made at various latitudes and longitudes on a voyage from England to India, in case you should deem them of sufficient importance to give them a place in your valuable Magazine.

I regret that they do not extend through the whole voyage. It was not till after passing the Cape that I recorded any observations. I was anxious to see what effect the Mozambique Channel had upon the currents; and after that I continued my observations up to the Bay of Bengal.

I have also given the result of a few observations on the velocity of the waves of a swell in unfathomable water.

Once or twice I attempted to ascertain the temperature of the sea water at a considerable depth, such as 40 and 100 fathoms. The method I adopted was this: I sunk a quart bottle, full of sea-water and well-corked, by means of a line, and allowed it to remain a considerable time (as an hour or more), that the water within the bottle might attain the temperature of the surrounding water by conduction. I then drew it up with great rapidity (perhaps in 1½ minute), instantly uncorked the bottle, and tried the temperature. When I poured the water into a glass it would change its temperature very little in 5 or 10 minutes; so I felt assured that no *great* change could have taken place in its passage from its lowest depth. I should have made more of these observations, but the utter impracticability of sinking a bottle well, except in a dead calm, prevented this. One day I had a bottle 200 fathoms deep for one or two hours; a gentle breeze sprang up, my bottle towed astern, and in pulling it in the line broke!

A notice of my observations will be seen in the accompanying tables of temperature.

I am, Gentlemen, yours, &c.

Bishop's Palace, Calcutta,
March 22, 1839.

John Henry Pratt.

Results of a Series of Experiments on the Temperature of the Sea, made by J. H. Pratt, *M.A., on board the ship Duke of Buccleugh, bound for Calcutta.*

The experiments were begun on the east of the Cape.

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		General Remarks.
				Sea.	Air.	
1838. Nov. 17.	37° 37′ S.	20° 40′ E.	11 p.m.	69°·25	66°	
18.	37 40	24 14	10½	64·25	64·25	Clear sky.
19.	37 54	28 30	9 a.m.	65	68	Clear.
			10½ p.m.	69·25	64·50	Cloudy.
20.	37 46	33 42	8 a.m.	63·75	59	Clear.
			3 p.m.	64·75	61	
			11	69·75	62·50	Clear (starlight).
21.	38 11	37 51	7 a.m.	68·25	64	Clear.
			9	68·25	64·25	Clear.
			noon.	66·25	64	
			2 p.m.	64·50	64·50	Clear.
			5	62·50	62·50	Violent squall.
			7	62·50	57	Immediately after heavy rain.
			8½	62	58	☽ bet. squalls.
			10	62	58	Cloudy.
			midnight.	62	56	Starlight.
22.	37 55	42 20	5½ a.m.	67	58	Cloudy.
			7½	67·25	58	
			9	67·25	58	
			10½	68	59	
			noon.	67	59	Clear.
			2 p.m.	65½	59	
			5	59·75	61·50	— Sea became remarkably *green*: passing through comp. shallow water.
			7	58	59	—
			9	55·25	55·25	—
			11	62	58	Clear.
23.	37 46	47 4	4 a.m.	65·25	59	Clear.
			7½	64·50	60	
			9	63·50	59·25	Cloudy.
			11	63	62	Clear.
			noon.	63·50	61·75	
			2½ p.m.	63·50	64·50	
			4½	63·50	64·50	
			7	63·50	64	
			8½	63	63·50	Cloudy.
			10½	63	63·50	
			11½	63	62·50	
24.	38 14	51 50	7½ a.m.	64·75	64	Cloudy.
			9	63·25	64·50	
			10½	63·25	65	
			12½ p.m.	63	65·50	Clear.

Table *continued.*

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838. Nov. 24.	38° 14′ S.	51° 50′ E.	2 p.m.	63°·50	66	Clear.
			5	64	67·50	
			8½	62	64	Cloudy.
			10½	61	63	
			12	62	64	
25.	37 55	57 20	12 p.m.	61·50	63·50	Clear.
26.	38 48	62 2	8 a.m.	60·25	60	Cloudy.
			10	60	61	Clear.
			2 p.m.	60·75	62·75	
			5	60·50	60·50	Cloudy.
			7	60·50	59·50	Clear.
			10	60·50	59	Cloudy.
			12	60·25	60	
27.	38 48	64 0	8 a.m.	60	55·75	Rain.
			noon.	60	56·50	
			10 p.m.	60·50	59	Cloudy.
			12 p.m.	60	56	
28.	38 21	67 50	7½ a.m.	59	55·50	
			2 p.m.	58·75	56	Clear.
			10½	59	56·50	Cloudy.
29.	37 2	70 35	8 a.m.	59·25	57	
			10½	59·25	57·75	Clear.
			1 p.m.	60	57	
			10	59	57	Cloudy.
30.	37 30	72 5	8 a.m.	58·50	59	
			2 p.m.	58	61·50	
			6	57·50	59	Rain.
			10½	57·75	59·25	Cloudy.
Dec. 1.	37 16	77 1	8 a.m.	57	60·75	
			2 p.m.	58·50	61	Clear.
			8	60	61	Cloudy.
			10½	59·25	61	
2.	36 12	81 8	10 p.m.	59	62·25	
3.	34 25	82 50	7½ a.m.	60	62	Fog.
			noon.	62·75	66	Cloudy.
			6 p.m.	63	65	
			10	63·50	64½	
4.	31 27	85 3	7½ a.m.	66·25	67	
			9	66·25	67.50	
			2½ p.m.	69	71.50	Clear.
			10	67·50	68	
			12	67·25	67·50	
5.	28 25	86 37	7 a.m.	68·25	68·50	Clear.
			9	68·50	69·50	
			11	69	71·25	
			1 p.m.	69·25	72	
			3	70·	73	
			5	71·	72	
			7	71·50	71·50	Cloudy.

TABLE *continued.*

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838. Dec. 5.	28° 25′ S.	86° 37′ E.	9 a.m.	71·75	71·50	Clear.
			11	71·50	72	Cloudy.
6.	26 8	88 53	7 a.m.	72·25	71·25	
			9	72	71·50	Clear.
			11	73	73·75	Cloudy.
			2 p.m.	73·50	73·50	
			5	73·50	73·50	
			7	73·50	72·75	
			10	73	72	
7.	24 24	89 10	9 a.m.	73·75	73·25	
			1½ p.m.	74	74·50	
			5	74	75	Clear.
			7	73·50	73·50	
			10	74·75	73	
8.	21 2	89 12	7 a.m.	76·75	74	Cloudy.
			9	76·50	76	
			11	77·25	76	Clear.
			1½ p.m.	78	76·50	
			5	77·25	76·50	
			6½	76	76·50	
			10½	76	75·50	
			12	76	75	
9.	17 48	88 45	10 p.m.	79	78½	Cloudy.
10.	13 55	89 2	7 a.m.	80	80	
			9	80·25	79	—— very squally
			11½	80·50	80·50	Clear.
			5 p.m.	80·50	79·50	Cloudy.
			10	80	79	——
						Violent squalls at night.
11.	10 55	88 30	6 a.m.	79·50	78	Clear. Gale.
			8	80·25	80	——
			noon.	80·25	80	——
			10 p.m.	80·25	79	——
12.	11 35	88 4	7 a.m.	80	79	Rain and heavy squalls.
			10 p.m.	80	80	
13.	12 0	87 8	10 a.m.	79·75	81	Rain, heavy squalls.
			10 p.m.	79·25	78	
14.	11 30	86 28	8 a.m.	80	70	
			2 p.m.	80	82	Clear.
			12	79·50	79·50	
15.	11 11	86 42	6 a.m.	80	79·50	
			9	80	79·50	
			12 p.m.	80	80	
16.	10 52	87 8	10 p.m.	80	80	
17.	10 8	85 47	7 a.m.	80·75	81	
			2½ p.m.	81·75	82·50	
			10	81	81	
18.	9 43	85 13	7½ a.m.	81	81·50	
			noon.	81	82	
			12 p.m.	81	80·50	

TABLE *continued.*

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838. Dec. 19.	° ′ 8 21 S.	° ′ 85 0 E.	7 a.m.	81·25	81·25	Clear.
			noon.	82·50	84	— A bottle was sunk for an hour or so; 40 fathoms (about noon), when brought up 81°·50 temp. of water.
20.	7 54	85 20	9 a.m.	82·50	82	—
			noon.	84	82·50	—
			2 p.m.	84	84	—
			10½	81·75	84	—
21.	7 29	85 18	9 a.m.	82·50	82	— Bottle sunk 100 fathoms. Temp. 78°.
			noon.	83	82·50	—
			2 p.m.	85	83·50	—
25.	1 56	87 0	10 p.m.	83·50	81·25	Clear.
26.	1 6	87 0	7 a.m.	84	81	
			2 p.m.	85	84	
			10	84	79	Rain and squalls.
27.	29′ S.	87 20	7½ a.m.	84·25	82·25	Clear.
			2½ p.m.	85	85·25	
			10	84	83	
28.	24′ N.	88 25	7½ a.m.	84	82½	
			2½ p.m.	84	83	
			10	83½	82	
29.	1 30	89 16	7½ a.m.	83·50	82	
			10½ p.m.	83·25	82	
30.	4 14	91 8	noon.	83	84·50	
			12 p.m.	82·50	82	Cloudy; bottle sunk 100 f. 70°. exp. *well* made.
1839. Jan. 1.	4 10	91 28	7 a.m.	82·50	82	
			10	83	83	
7.	11 50	88 50	2 p.m.	80·50	80	Clear.
			10	80	80	
8.	13 2	88 57	8 a.m.	80	79	
			1 p.m.	80·50	81	
			10	80	79	
9.	13 54	88 9	8 a.m.	80	79	
			noon.	80	79	
			6 p.m.	80·50	78·50	
			10	80	78	
10.	14 22	88 35	8 a.m.	81	78·50	
			noon.	81·25	80	
			10 p.m.	80·50	78	
11.	14 59	88 23	noon.	80·50	81	
			10 p.m.	80·50	78	
12.	15 57	88 5	8 a.m.	80	79	
			noon.	80	81	
			10 p.m.	78	78	
14.	17 20	88 10	9 a.m.	77·75	77·75	
			2½ p.m.	78·50	81·50	
			10	77·75	77·75	
15.	18 15	88 55	8 a.m.	77·50	78	
16.	20 49	88 47	noon.	77·50	76	
			10 p.m.	76·50	73	
17.	Mouth of	Hoogly.	8 a.m.	75	71·50	

I made the following experiment on the velocity of waves out at sea. Lat. 27° 2′ S. Long. 27° 25′ W.

There was a *swell* on the sea moving from fore aft; wind only sufficient to carry the vessel (all sails set) *steadily* two or three miles an hour. Two large floats were connected by a line forty fathoms in length, the line itself being supported on the surface of the water by smaller floats. This apparatus was towed astern by a long line connected with one of the large floats by one end, the other end being wound round a reel.

The chief officer watched the chronometer; the second officer held the reel fixed; and I observed the large floats.

A few seconds before the first float was raised to its greatest height by a given wave, I gave a signal to the second officer to let the reel run, and immediately the floats became stationary in the water.

At the instant the first float, and also at the instant the second float, was raised to its greatest height by the wave already mentioned, I gave audible signals to the chief officer, who marked the interval of time between the signals.

A very good average of many trials gave a trifle less than six seconds of time for the motion of the wave from float to float, i. e. over forty fathoms. This gives nearly 27⅓ statute miles an hour.

The chief officer and I changed places, and came to the same result. Two days afterwards this was confirmed, though in rather a rough manner, by observing the motion of the vessel by a swell moving abaft.

Calcutta. J. H. P.

XXXIII. *Researches in the Undulatory Theory of Light continued: On the Absorption of Light.* By John Tovey, *Esq.*

(Continued from p. 455 of last Volume.)

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOU will have observed that the formulæ of my last communication are deduced from the fundamental principles of the undulatory theory without the aid of any assumption respecting the arrangement of the molecules, or the nature of the constant quantities, k, &c., which appear in the integration. Those formulæ indicate, as was shown, that the transmission of the light may be accompanied by an absorption, or diminution of its intensity, depending on the nature and thickness of the medium through which it is transmitted, and on the length of the wave. This, then, is a *general result* from the theory, and it agrees, as we know, with experience.

We shall now proceed with the investigation, in order to compare the theory with experiment a little further.

By (14.) and (23.) we have

$$\kappa = \epsilon + k\sqrt{-1}, \tag{36.}$$

and, by transformation,

$$e^{k\Delta x\sqrt{-1}} = \cos k\Delta x + \sqrt{-1}.\ \sin k\Delta x; \tag{37.}$$

hence, if we put

$$\begin{aligned} e^{\epsilon\Delta x}\cos k\Delta x - 1 &= u, \\ e^{\epsilon\Delta x}\sin k\Delta x &= u', \end{aligned} \tag{38.}$$

we have, by (8),

$$\begin{aligned} s &= \Sigma p u + \sqrt{-1}.\ \Sigma p u', \\ s' &= \Sigma p' u + \sqrt{-1}.\ \Sigma p' u', \\ s_{,} &= \Sigma q u + \sqrt{-1}.\ \Sigma q u. \end{aligned} \tag{39.}$$

If we compare these equations with (20.) we shall find

$$\begin{aligned} \sigma &= \Sigma p u, & \sigma' &= \Sigma p' u, \\ \sigma_{,} &= \Sigma p u', & \sigma'_{,} &= \Sigma p' u', \\ \sigma_2 &= \Sigma q u, & \sigma_3 &= \Sigma q u'. \end{aligned} \tag{40.}$$

By (13.) and (20.) we find

$$(n^2 + \sigma + \sqrt{-1}.\ \sigma_{,})\ (n^2 + \sigma' + \sqrt{-1}.\ \sigma'_{,}) = (\sigma_2 + \sqrt{-1}.\ \sigma_3)^2,$$

which, since n is real, gives

$$\begin{aligned} (n^2+\sigma)\ (n^2+\sigma') - \sigma_{,}\sigma'_{,} &= \sigma^2_2 - \sigma^2_3, \\ (n^2+\sigma)\ \sigma'_{,} + (n^2+\sigma')\ \sigma_{,} &= 2\sigma_2\sigma_3. \end{aligned} \tag{41.}$$

Hence, by eliminating n^2, and reducing, we find

$$\begin{aligned} (2\sigma_2\sigma_3 - (\sigma_{,}-\sigma)\ \sigma_{,})\ (2\sigma_2\sigma_3 + (\sigma'-\sigma)\ \sigma'_{,}) \\ = (\sigma_{,}\sigma'_{,} + \sigma_2 - \sigma_3)\ (\sigma'_{,} + \sigma_1)^2, \end{aligned} \tag{42.}$$

which, as appears by (38.) and (40.), expresses, implicitly, the relation between ϵ and k.

To obtain a precise idea of the movement represented by the expressions (35.), suppose the arbitrary coefficients denoted by a to be all zero except one; then each of the sums will be reduced to a single term, so that we shall have

$$\begin{aligned} \eta &= a e^{\epsilon x}\sin(nt + kx + b), \\ \zeta &= \beta a e^{\epsilon x}\sin(nt + kx + b + \gamma). \end{aligned} \tag{43.}$$

Put $a e^{\epsilon x} = \alpha$, $n t + k x + b = \omega$, then $\eta = \alpha \sin \omega$, $\zeta = \beta \alpha \sin (\omega + \gamma) = \beta \alpha (\cos \gamma \sin \omega + \sin \gamma \cos \omega)$; which last expression for ζ gives

$$(\zeta - \beta \alpha \cos \gamma \sin \omega)^2 = (\beta \alpha \sin \gamma \cos \omega)^2$$
$$= (\beta \alpha \sin \gamma)^2 (1 - \sin^2 \omega).$$

But, since $\alpha \sin \omega = \eta$, this equation gives

$$(\zeta - \beta \cos \gamma . \eta)^2 = (\beta \alpha \sin \gamma)^2 - (\beta \sin \gamma . \eta)^2:$$

hence we find

$$\frac{\eta^2}{\alpha^2} + \frac{\zeta^2}{\beta^2 \alpha^2 \sin^2 \gamma} - \frac{2 \cos \gamma . \eta \zeta}{\beta \alpha^2 \sin^2 \gamma} = 1, \qquad (44.)$$

an equation to an ellipse of which η and ζ are the coordinates.

Consequently, when the system is in the state of motion expressed by the equations (43.) every molecule describes an ellipse round its place of rest; and the equations (35.) show that the general motion of the system is equivalent to a number of coexisting motions of the same kind.

The period of the revolutions of the molecules, in the movement represented by (43.), is equal to $\frac{2 \pi}{n}$; where 2π is the circumference of a circle whose radius is unity. And this movement is transmitted through the medium in a series of continuous waves; the length, or rather thickness, of each wave being $\frac{2 \pi}{k}$. The direction in which the waves travel depends on the sign of k, supposing that of n to continue the same. But, by the equations (22.), it appears that the sign of n is arbitrary: therefore n as well as the arbitrary quantities a and b, may be written either positively or negatively. Now if we change the signs of a, n, b, in (43.), it is virtually the same thing as changing the signs of k and γ, while those of a, n, b, remain the same. Consequently, when we take for the positive direction of x, that in which the waves travel, we may write the equations (43.) thus:

$$\begin{aligned} \eta &= a e^{\epsilon x} \sin (n t - k x + b), \\ \zeta &= \beta a e^{\epsilon x} \sin (n t - k x + b - \gamma), \end{aligned} \qquad (45.)$$

and suppose n and k to be positive.

The intensity of the light is considered to be measured by the *vis viva* of the molecules, which, when other things are equal, is proportional to the square of the amplitude of vibration. Thus, when the movement is represented by (45.), the intensity is proportional to $a^2 e^{2\epsilon x} + \beta^2 a^2 e^{2\epsilon x}$: hence, if we put $c = a^2 + \beta^2 a^2$, it will be proportional to $c e^{2\epsilon x}$. Suppose the origin of x to be at the surface of any medium on which the light falls; then $c e^{2\epsilon x}$ will be the intensity of the light after it has traversed a thickness of the medium equal to x. And if $c_1, c_2, c_3, \ldots \epsilon_1, \epsilon_2, \epsilon_2, \ldots$ be the values of $(a^2 + \beta^2 a^2)$ and ϵ respectively in the general expressions (35.), the intensity of the light in the transmitted ray will be

$$c_1 e^{2\epsilon_1 x} + c_2 e^{2\epsilon_2 x} + c_3 e^{2\epsilon_3 x} + \&c. \qquad (46.)$$

If we were to put $y, y', y'', \ldots$ for $e^{2\epsilon_1}, e^{2\epsilon_2}, e^{2\epsilon_3}, \ldots$ in this formula, it would become the very same as that which was devised by Sir John Herschel to represent the law of absorption as indicated by experiments.

The formula (46.) shows clearly enough the manner in which the absorption depends on the thickness of the medium, and it indicates, by the different values of ϵ, which belong respectively to different values of k, that the absorption is different for waves of different lengths. But the relation of ϵ to k, which, as we have seen, is implicitly expressed by the equation (42.), is so extremely complicated that the readiest way of testing our theory with reference to it, seems to be by inquiring whether experiments show that it is of so complex a character. Now the nature of this relation, as inferred from experiments, is stated by Sir J. Herschel in his excellent paper on the absorption of light, published in vol. iii. of the current series of your Journal, where at page 402, he says:—"If we represent the total intensity of the light, in any point of a partially absorbed spectrum, by the ordinate of a curve, whose abscissa indicates the place of the ray in the order of refrangibility, it will be evident from the enormous number of maxima and minima it admits, and from the sudden starts and frequent annihilations of its value through a considerable amplitude of its abscissa, that its equation, if reducible at all to analytical expression, must be of a singular and complex nature, and must at all events involve a great number of arbitrary constants, dependent on the relation of the medium to light, as well as transcendents of a high and intricate order." This character is very suitable to our equation (42.), and may, therefore, be taken as an evidence of its truth.

That a spectrum absorbed in the apparently capricious manner described in the above extract, would result from the relation between ϵ and k implied in (42.) may be thus shown. Suppose, in the first place, ϵ to be zero, and $k_1, k_2, k_3, \ldots$

to be the roots of the equation in that case: then the waves of which the lengths are $\frac{2\pi}{k_1}$, $\frac{2\pi}{k_2}$, $\frac{2\pi}{k_3}$, ... will be transmitted without absorption, and consequently will form a number of bright lines in the spectrum, yet probably too few to afford, by themselves, any sensible light. Now suppose ϵ to decrease gradually, then all the roots k_1, k_2, k_3.... will vary, but not with equal rapidity. Some of them may be changed in magnitude considerably by a very small change in ϵ, and, consequently, in the parts of the spectrum to which these roots respectively correspond, there will be bright bands. Other roots may be only slightly affected by a considerable change in ϵ; hence there will be, in the parts of the spectrum which correspond to these roots, rapid variations in the intensity of the light, producing dark bands or dark lines.

Perhaps the equation (42.) which we are considering, may, in certain cases, be much simplified; but I cannot proceed with the subject any further in the present paper.

I am, Gentlemen, yours, &c.,
Littlemoor, Clitheroe, Feb. 6, 1840. JOHN TOVEY.

P.S. In my last paper, vol. xv. p. 451, last line but three, *for* increasing indefinitely *read* increasing or diminishing indefinitely, —p. 452, line 14, *for* $\cos m i = \sqrt{-1} . \sin m i$ *read* $\cos m i + \sqrt{-1} . \sin m i$;—p. 453, line 28, *for* $\rho, \alpha,$ *read* $\rho_1 \alpha_1$; and line 29, *for* α, *read* α_1; —p. 454, line 21, *for* (23.) *read* (33.); lines 22 and 23, *for* $e^{e x}$ *read* $e^{\epsilon x}$

XXXIV.—*On the Direction and Mode of Propagation of the Electric Force traversing Interposed Media.* By GEORGE J. KNOX, *Esq., A.M., M.R.I.A.*[*]

WHATEVER theory be adopted to explain the passage of the electric force traversing an intervening fluid or solid substance not undergoing electrolyzation,—whether we suppose it to originate in an inductive influence affecting the circumambient æther of each particle of the substance in the line of direction of the force, in whose alternate states of induction and equilibrium consists the passage of the electric current, (the rapidity of such changes constituting its intensity,) while the vibratory motion produced in the particles of the æther on each successive return to a state of equilibrium causes the phænomena of the light and heat developed; or whether we adopt the gross conception of the passage of a fluid; still it is important to determine if the electric force passes along the surface of the interposed substance, or through the interior of its mass.

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[*] From the Transactions of the Royal Irish Academy, vol. xix.

Dr. Faraday* has shown that water will convey a feeble current of electricity, without undergoing electrolyzation. To determine whether, under such circumstances, it will convey an electrical current along its surface or through its substance, a glass tube, ten feet long, and half an inch internal diameter, bent in the centre twice at right angles, was filled with distilled water. Two copper wires, twenty feet long, having platina wires soldered to their extremities, were inserted in barometer tubes of six feet in length, the platina wires being sealed in the tubes within half an inch of their extremities. The other ends of the copper wires were connected with a delicate galvanometer, and a constant battery of successively one, two, four, &c., pair of elements.

On immersing the platina wires in the liquid, their relative distances from each other should decrease if the current passes through the water, but should increase if it passes along the surface, the deflexion of the galvanometer indicating the path. With one pair of elements there was no deflexion of the galvanometer; with two pair of elements there was a slight deflexion visible through a lens, which increased slightly on immersing the platina wires in the liquid. With four pair of elements, a deflexion of two degrees took place when the platina wires were on the surface of the water; a deflexion of four degrees when they were immersed to the bottom of the tubes. As the number of alternations in the battery increased, so did proportionably the comparative deflexions of the galvanometer; the experiments proving that water, whether undergoing electrolization or not, conveys an electric current *through its substance*, and not *along its surface*, and that the decomposition of the water is an effect produced by the passage of the electricity when of sufficient intensity, and not the necessary consequence of its passage.

A similar experiment having been tried with phosphorus melted under spirits of wine, (being a non-conductor,) it was found to obey the same law with water; that is, to convey the current through its substance†.

To determine whether the metals followed the same law, I suspended from the top of the new patent shot tower at Waterloo-bridge a leaden pipe, 170 feet long, and three-fourths of an inch internal diameter, through which was drawn an insulated copper wire, 180 feet long, one extremity of which being soldered to the inside of the end of the pipe, this end was sealed with fused metal, and to its external surface was soldered a copper wire of the same length as the former; round the tube, at its orifice, was twisted a copper wire ten feet long. The insulated wire being connected with a constant battery of one pair of elements in contact with one pole of an exceedingly delicate galvanometer, (constructed by Mr. E. M. Clarke of the Lowther Arcade,) the other pole of the galvanometer was brought successively in contact with the extremities of the uninsulated wires. The deflexion was greater when the current passed along the wire connected with the orifice of the tube, (although here the contact was not so good,) than when it passed along that soldered to the sealed extremity.

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* Series VIII. (970.)

† It was unnecessary to try similar experiments with the analogous bodies, sulphur, selenium, and iodine.

Again, the uninsulated wires being connected with separate galvanometers, so as to allow the current of electricity to pass along either of the uninsulated wires alone, or to be distributed between both, it was found (as well as could be determined by transposing the galvanometers,) to have divided itself into two equal currents flowing along both wires.

From the first experiment we may infer that a current of electricity passes with greater facility along the surface of a metal than through the interior of its mass, although we cannot hereby infer that it could not pass through the interior of the metal, when this is the only road open for its transit*.

To the experiments with phosphorus it might be objected that its capability for conducting an electric current is due to the presence of water, of which some have supposed that it could not be entirely deprived, although the experiments of Sir H. Davy, wherein he obtained hydrogen and oxygen from sulphur and phosphorus by heating them in contact with potassium and sodium, and by submitting them to the electrolytic action of a powerful galvanic battery, did not prove that they were united with the basis of these substances in such proportions as to form water, nor indeed does he appear to have entertained such an opinion himself. His opinion of the nature of sulphur was, that it was "a compound of small quantities of oxygen and hydrogen, with a large quantity of a basis, that produces the acids of sulphur in combustion, and which, on account of its strong attraction for other bodies, will probably be difficult to obtain in its pure form*." To put the question beyond any further doubt, I will mention some experiments which I tried in the laboratory of the Royal Dublin Society in the year 1837, having had, through the kindness of Professor Davy, a galvanic battery of sixty pair of plates, five inches square, put at my disposal.

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* The high conducting power of mercury for electricity renders it almost impossible to determine, by this method, whether metals in the *fluid* state obey the same laws of conduction as when in the solid state. If they do not, it is highly probable there is a general law, that *all solids conduct along their surface, and all fluids through their substance.* The investigation of such general law I propose to continue in another paper.

When fused phosphorus, sulphur, selenium and iodine, were submitted separately to the action of this battery charged with a strong acid solution, they conveyed the electrical current freely during the whole time, giving a spark whenever contact was broken; yet at the end of two hours they showed not the slightest trace of decomposition, no gas being evolved at either pole, which would have been the case had there been any water present.

Having by these experiments shown the *direction* of propagation of the electric force, I will now consider the source from which it originates in the voltaic pile, the mode of its transfer, and its sustaining principle.

Sir H. Davy's† opinion that the contact of the metals was the *primum mobile* of voltaic excitement, having been proved by Dr. Faraday‡ to be erroneous, chemists are now pretty generally agreed that the electrical force developed in the voltaic pile is due altogether to chemical action, concerning which there are different opinions; of these, I will mention two, which are the most applicable to the present argument—Dr. Faraday's§ and Mr. Becquerel's‖. The former supposes that the development of electricity is due to decomposition alone, and in no case to the chemical union of bodies; while the latter contends that it is due to both, and in proof of his opinion shows that when an alkali unites with an acid, with a neutral salt, and in fact with any solution whose natural state is with regard to it electrically negative, a current of electricity will flow from the alkali to that solution. Sir H. Davy¶ has taken a different view of these experiments from Mr. Becquerel, supposing that the electric current is produced by the action of the acid or alkali upon the platinum plates; but the latter has shown that the electrical current is produced equally when no such action could take place, the platinum poles being placed in separate cups filled with water*.

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* Bakerian Lecture, 1809.

† Phil. Trans., Bakerian Lecture, 1826.

‡ Eighth Series, (880).

§ Eighth Series, (927) (928).

‖ Tom. ii. from page 77 to 81.

¶ Phil. Trans., Bakerian Lecture, 1826.

The accuracy then of Mr. Becquerel's experiments having been fully established, the question arises, how are we to reconcile them with other well-known contradictory facts? such as for instance those of Sir H. Davy†,—solid potash and sulphuric acid combining in an isolated platinum crucible, and causing no electrical development. Again, a plate of copper and of sulphur, when heated, have their electrical states increased until chemical action begins, when they cease.

The simplest and clearest course, and that most reconcileable with the laws of statical electricity, seems to me to be:—to consider that no electrical development is caused by the *union* of an alkali with an acid, (the electricity being thereby disguised,) but that, at the *instant before* the union takes place, the particles of the alkali and of the acid, being in opposite electrical states, affect their surrounding particles by induction, causing thereby a feeble current of electricity to circulate from the acid through the galvanometer to the alkali, which supposition is borne out by the fact, that a dry acid and alkali, when in contact, show opposite electrical states.

The same arguments apply equally well with regard to thermo-electricity. The contact of two metals produces in them opposite electrical states. Their chemical union in an isolated vessel gives no electrical development; thus a "solid amalgam of bismuth and lead become liquid when mixed together, without producing any electrical effect‡." Again, "a thin plate of zinc placed upon a surface of mercury, and separated by an insulating body, is found to be positive, the mercury negative; but when kept together a sufficiently long time to *amalgamate*, the compound gives no signs of electricity‡."

These experiments explain why the *contact* of the two extremities of metallic wires, constituting a closed circuit, should, as the potash and nitric acid just mentioned, produce an induced electric current. That the electric states of different metals in contact, when excited by heat, do not follow the law of their natural electrical states, and change on increase of temperature, is no argument against the explanation I have given, for upon what this change in the electrical excitation produced by heat depends, whether upon a peculiar arrangement of the crystalline parts of the metal, or of their compound elementary particles, we are as yet perfectly ignorant.

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* He might have added another experiment, free from all objections—namely, the increased intensity consequent upon an increased number of alternations of acid and alkali.

† Phil. Trans., Bakerian Lecture, 1807.

‡ Ibid.

That the same general law of the contact of metals and of fluids applies equally (although in an inferior degree, owing to their want of conducting power) to the contact of the gases, may be shown by the experiment of Dr. Faraday (Sixth Series) of the union of hydrogen and oxygen by a plate of platinum; the electrical force, which circulates by the interposed platinum plate, facilitating the union of the two gases*.

To return to the source of the voltaic force in the battery. Zinc, when placed in contact with a dry acid, has been found to become positively electrified. When the zinc plate has been immersed in the acid solution, being positive, it attracts oxygen, by union with which its electrical state is disguised, while the hydrogen, set free in a highly positive electrical state, reacts upon the oxide of zinc, rendering it negative by induction. The platinum wire connecting the positive solution with the negative zinc plate, reduces all for the moment to a state of equilibrium, so that the electricity becomes disguised, not transferred bodily from the platinum to the zinc; which state of equilibrium is no sooner restored than it is destroyed, the zinc regaining its positive state, and the oxide being removed by the acid.

If we consider then what takes place, we shall perceive that the zinc plate undergoes alternate states of induction and equilibrium, as do likewise the particles of the solution between the zinc and platinum plates, and, in fine, the platinum plate itself, and that as the number of alternations of zinc and platinum increases, the electrical energy of the zinc plate increases, as does also *the rapidity of its oxidation and deoxidation, and as a consequence the rapidity of change of induction and equilibrium upon which the intensity of the current depends.*

The decomposition of the electrolyte may be considered to be the effect produced by two forces acting upon its particles; the attraction of the poles† of the battery (whether they be metal, water, or air) *originating*, while the electrical states induced upon the particles give the *direction* to the electrolytic action.

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* Aqueous solutions of different gases, when brought into contact, have been found to produce electrical currents.

† In place of *poles*, I should more properly have said *electrodes*, their bounding surfaces. It follows, as a consequence of the theory, that the particles of oxygen in contact with the electrodes should be attracted by, and set free from, those electrodes upon each alternation of the states of induction and equilibrium; and that, when the induced state has not sufficient energy to overcome the affinities already engaged, the current of electricity passes without producing electrolyzation. For a different explanation, vid. Dr. Faraday's Series of Researches, 493, 494, 495, 534, 535, 536, 537, 807.

From what has been said above, we may, I think, presume that an electric current originates in a natural electro-inductive power of bodies when brought into *contact*, and is continued by alternate states of induction and equilibrium, the *rapidity* of change of state constituting its *intensity*. And inasmuch as the accumulation of the electric æther on the surface of the particles by the inductive force, and its recession on each return to a state of equilibrium produces what may be called an oscillation in the æther, the theory may be otherwise stated thus:—the mass of oscillating æther which surrounds the particles constitutes the quantity, while the rapidity of the oscillations constitutes the intensity of an electric current.

The late experiments of Dr. Faraday upon induction (Eleventh Series) showing that an insulated body (the particles of bodies may be presumed to be such) cannot receive an absolute charge of electricity, but only an inductive charge, afford a strong argument in favour of my views.

The theory proposed in this paper, and deduced from the experiments of Sir H. Davy, given in his Bakerian Lectures, is an extension of the views therein developed, reconciles the contact with the chemical theory, and reduces to the laws of *statical* electricity *all* the phænomena of electricity in motion. I will now endeavour to show how the law of the definite nature of electro-chemical decomposition, so beautifully developed by Dr. Faraday, follows as a consequence from this theory. Were the particles of all bodies endued with the same quantity of electricity, and of the same density, it is evident from the laws of statical electricity, that no one body could have an attraction or repulsion for another; consequently, it is an evident fact, that the quantity and density of the electric æther varies in different bodies; and as, from the theory above stated, electricity never leaves the particles, but merely (to use the words of statical electricity) accumulates upon the surface, and returns, it follows that the electrical states of the particles of bodies are constant and unalterable, and therefore it is obvious that the law discovered by Dr. Faraday follows as a consequence from this hypothesis, which is at once clear and simple, which includes all the phænomena, and is but a reference of the laws of *statical* electricity to the *particles* of bodies in place of their *masses*.

Researches on Fluorine. By G. J. Knox, *A.M., M.R.I.A.*[*]

1. *On the Insulation of Fluorine.*

"In a paper on the Insulation of Fluorine which the Rev. Thomas Knox and I had the honour of presenting to the Royal Irish Academy in the year 1837[†], and which was afterwards published in their Transactions, (vol. xviii. p. 127,) we proved that we had obtained fluorine in an insulated state, by showing its action upon bismuth, palladium, and gold; but being unable, from our mode of experimenting, to determine what the nature of fluorine at ordinary temperatures might be, i. e. whether it be a solid, a liquid, or a gas, we suggested that such information might be obtained from the electrolization of a fluoride, using as the positive electrode some substance with which this energetic principle should not enter into chemical combination.

"Finding that, since the publication of our paper, no person had entered upon this field of investigation, I considered that the ultimate solution of this problem devolved as a point of duty upon myself; under which impression I undertook the following experiments.

"A fluorspar stopper was made to fit the mouth of one of the fluorspar vessels described in our former paper; that part of the stopper within the vessel being made of the form of a semi-cone, the vertex of which reached nearly to the bottom of the vessel. Through the stopper were drilled vertically three small holes, one through its entire length, the other two through one-third of its length. In the first was inserted a platinum wire, to be used as the negative electrode; in one of the two small holes was inserted a thin platinum wire, bound round a piece of charcoal, intended to form the positive electrode; in the other hole I put gold-leaf, litmus, or any other substance upon which I wished to try the action of the gas. Matters being so arranged, the fluorspar vessel was about half filled with anhydrous hydrofluoric acid, the chemical purity of which had been previously ascertained. The platinum wire forming the negative electrode was raised a little above the bottom of the stopper, in order to allow the bubbles of hydrogen to rise through the perforation in the stopper, in place of mixing with the fluorine in the vessel; the wires were then placed in contact with the poles of a constant battery of sixty pair of plates, and the action was allowed to continue for the space of two hours; at the end of which time the litmus was found to be reddened, and the gold not acted upon, but a large quantity of subfluoride of iron formed.

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[*] From the Proceedings of the Royal Irish Academy.

[†] See Lond. & Edinb. Phil. Mag., vol. ix. p. 107.

In the next experiment I made use of a piece of charcoal, from which the iron had been removed by boiling it in nitric acid; in this experiment there was no subfluoride of iron formed, but the vessel was found to contain fluosilicic acid gas.

In a third experiment a piece of charcoal was employed, which had been previously freed from all metallic impurities and from silica, by being first boiled in pure nitric acid, and afterwards in hydrofluoric acid. Employing this purified charcoal as the positive electrode, I obtained no *immediate* action upon the litmus paper; but after the action had continued for two hours, it was found to be completely bleached, while the gold had undergone no sensible action. That the bleaching was not due to the action of the vapour of hydrofluoric acid was ascertained, by leaving litmus paper for several hours in the neck of a platinum retort, from which hydrofluoric acid was distilling.

The battery was now kept in action for fifteen hours, at the end of which time the vessel being examined, the litmus had disappeared, and the gold-leaf showed signs of having been strongly acted upon, having assumed a dark brownish colour, and having gathered itself into little balls, as if it had undergone the action of heat. The platinum wire was acted upon in those parts where it was in contact with the charcoal, but nowhere else.

When the platinum wire forming the positive electrode passed through the stopper to the bottom of the vessel, the hydrogen, in place of rising through the perforation in the stopper, as in the former instance, rose now into the receiver, where, upon applying a light, it exploded, showing that it does not enter into combination with fluorine without the aid of heat. The presence of the vapour of hydrofluoric acid in the vessel prevented me from determining by other experiments how far fluorine was a supporter of combustion.

To determine the colour of the gas, a stopper of fluorspar similar to the former was made to fit one of the transparent fluorspar receivers formerly described. The gas evolved in the receiver appeared colourless.

As the action of the gas upon glass could not be determined, owing to the presence of the vapour of hydrofluoric acid, I fused in a bent tube of German glass (such as is used in organic analysis) fluoride of lead. The wire holding the charcoal was made to pass through a cork inserted in one end of the tube, the other platinum wire merely dipped into the fused fluoride. On connecting the wires with the battery, strong electrolytic action commenced, bubbles of gas were evolved rapidly at the surface of the charcoal, which, on arriving at the surface of the fused fluoride of lead, acted instantly upon the glass. The litmus paper was not bleached, nor the gold-leaf or platinum wire acted upon. Whether fluorine would act upon perfectly dry cold glass remains to be proved.

Conclusion.—Fluorine then, when obtained in an insulated state, is a colourless gas, possessing properties analogous in all respects to those of chlorine; having, like it, strong attrative powers for hydrogen and metals, but inferior to it in negative electrical energy.

2. *Note on a Compound of Fluorine with Selenium.*

When the vapour of selenium is passed over fluoride of lead fused in the platinum apparatus which I employed in obtaining the fluorides of carbon and cyanogen, a seleniuret of lead is formed, and crystals similar in form to those of fluoride of carbon are condensed in the cold receiver. These crystals are soluble in strong hydrofluoric acid. They sublime unaltered at a high temperature. They are instantly decomposed by water or acids, in which property they resemble the fluorides of sulphur and phosphorus.

XXXV. *On a simple mode of obtaining from a common Argand Oil Lamp a greatly increased quantity of Light: in a letter from* Sir J. HERSCHEL, Bart.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following simple, easy, and unexpensive mode of greatly increasing the quantity of light yielded by a common Argand burner, has been used by me for some years, and is adapted to the lamp by which I write, to my greatly increased comfort. It consists in merely elevating the glass chimney so much above the usual level at which it stands in the burners in ordinary use that its *lower* edge shall clear the *upper* edge of the circular wick by a space equal to about the fourth part of the exterior diameter of the wick itself. This may be done to any lamp of the kind, at a cost of about sixpence, by merely adapting to the frame which supports the chimney four pretty stiff steel wires, bent in such a manner as to form four long upright hooks, in which the lower end of the chimney rests; or still better if the lamp be so originally constructed as to sustain the chimney at the required elevation without such addition, by thin laminæ of brass or iron, having their planes directed to the axis of the wick.

The proper elevation is best determined by trial; and as the limits within which it is confined are very narrow, it would be best secured by a screw motion applied to the socket on which the laminæ above mentioned are fixed, by which they and the chimney may be elevated or depressed at pleasure, without at the same time raising or lowering the wick. Approximately it may be done in an instant, and the experiment is not a little striking and instructive. Take a common Argand lamp, and alternately raise and depress the chimney vertically from the level where it usually rests, to about as far above the wick, with a moderately quick but steady motion. It will be immediately perceived that a vast difference in the amount of light subsists in the different positions of the chimney, but that a very marked and sudden *maximum* occurs at or near the elevation designated in the commencement of this letter: so marked indeed as almost to have the effect of a flash if the motion be quick, or a sudden blaze as if the wick-screw had been raised a turn. The flame contracts somewhat in diameter, lengthens, ceases to give off smoke, and attains a dazzling intensity.

With this great increase of light there is certainly not a correspondingly increased consumption of oil. At least the servant who trims my lamp reports that a lamp so fitted consumes very little if any more oil than one exactly similar on the common plan.

I have the honour to be, Sir,
Your obedient servant,
Slough, Feb. 15, 1840. J. F. W. HERSCHEL.

XXXVI. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals. By* GEORGE GULLIVER, *F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards.* No. III.*

AN account is now to be given of the blood corpuscles of several other mammalia which I have examined since the publication of my last papers; and similar communications will be continued occasionally, until the observations have been made as complete as possible, when, as already intimated, they will be presented in a systematic form, so as to exhibit a comprehensive view of the results, particularly as regards the size and figure of the blood particles in the different subdivisions of the mammiferous animals.

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* Communicated by the Author.

It is very desirable that the blood of the larger species of the cetaceous animals should be examined; for although the corpuscles of the Mouse (13.) are bigger than those of the Horse (34.), and there is generally no relation between the size of the animal and that of its blood particles, yet they are larger in the Elephant (51.), as far as we at present know, than in any other mammal. The corpuscles of the Goat were the smallest known to physiologists before my observation of the singularly minute blood disks of the Napu Musk Deer *.

In some instances the corpuscles are found to be a little larger in the dead than in the living animal, although they may subsequently become smaller, in consequence of the removal of their colouring matter by the serum. It will be perceived that many observations have been made on the blood after death; these have led me to ascertain that the particles are subject to modifications in size, and in some degree in shape, as compared with those of the living animal; and similar variations are often observable during life in disease. Besides the instances in which these facts are barely indicated in the preceding communications, it may be mentioned that I have seen the changes in the human blood particles. Thus in a man affected with dropsy, in connexion with granular degeneration of the kidney, some blood was drawn from a vein of the arm, and the corpuscles found to differ remarkably from those of the healthy subject. Though examined before the blood was perfectly cold, as well as after the lapse of a day, their size was singularly irregular, generally smaller than natural, having an average diameter of only 1-4400th of an inch. But as the morbid conditions of the blood corpuscles are probably more extensive and important than has been hitherto supposed, this is a novel and interesting subject for further and special inquiry; and it is merely alluded to at present as one of the many circumstances under which the size, form, and general appearance of the disks are liable to variations, which will doubtless attract the attention of pathologists now that the necessity of minute researches concerning the morbid as well as healthy fluids has been so fully recognised.

With regard to the blood corpuscles of the fœtus as compared with those of the mother, I apprehend that I have de-

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* See Dublin Med. Press, No. 27, 1839, and Annals of Nat. Hist., &c. Dec. 1839.

tected a source of error in some of the observations. The statement, therefore, formerly made (13.) is withdrawn for the present; and I hope soon to be able to give the result of another inquiry on the subject. In the mean time I may mention that in the fœtal Guinea pig at the full period of utero gestation the corpuscles corresponded in size with those of the mother; and in a human fœtus at the fifth or sixth month they were smaller than in the adult.

116. Mona Monkey, (*Cercopithecus Mona,*) a male about a third grown. Most frequent sizes of corpuscles 1-3554th and 1-3428th. Extreme diameters, 1-5333rd and 1-2900th. Blood from the left coronary vein as well as from the different cavities of the heart. In the blood of the inferior cava vein the corpuscles were more variable in size, the extremes being 1-4800th and 1-2400th, with most numerous intermediate gradations.

117. Sooty Monkey, (*Cercopithecus fuliginosus*), a female about half-grown. All the following diameters very frequent, 1-3600th, 1-3428th, 1-3368th, and 1-3200th. Extreme sizes 1-5333rd and 1-3000th. Blood from the left ventricle of the heart.

118. Patas or Red Monkey, (*Cercopithecus ruber,*) a female, nearly full-grown. The disks most commonly 1-3330th of an inch in diameter; extreme sizes, 1-4000th and 1-3000th. Blood from the pulmonary artery and vein, a few hours after death.

119. Crown Monkey, (*Cercopithecus pileatus,*) a male about two-thirds grown. The following the most common sizes: 1-3635th, 1-3600th, and 1-3423rd. Extreme diameters, 1-4800th and 1-2900th. Blood from a prick of the forehand.

120. Vervet Monkey, (*Cercopithecus pygerythrus,*) an adult male, 1-3309th, 1-3429th, and 1-3552nd common sizes; extreme diameters 1-4000th and 1-2900th of an inch. Blood from a wound at the end of the tail.

121. Dog-faced Baboon, (*Cynocephalus anubis?*) a female about half-grown. The dried corpuscles 1-3600th, 1-3530th and 1-3428th generally; extreme diameters 1-4000th, and 1-3000th. In the serum there were several disks exactly of the same magnitude, besides many of smaller size, viz. about 1-5333rd of an inch in diameter.

The blood was procured from a wound of the tail, and examined in less than two hours afterwards. The shrinking of some of the corpuscles in their own serum is well exemplified. In another dog-faced Baboon (1.) the corpuscles were a little larger; but as in that instance the blood was obtained from the animal a day or two after death, the variation is within the limits that may occur in the same species.

122. Black-backed Papio, or Indian Ape, (*Papio melanotus*,) a male. Common diameter of corpuscles 1-3432nd of an inch; extreme sizes 1-4570th and 1-2666th. Blood from the axillary vein after death.

123. Wanderoo Monkey, (*Papio silenus*,) a full-grown male. Frequent sizes of corpuscles 1-3600th, 1-3552nd, and 1-3270th of an inch. Extreme diameters 1-4570th and 1-2666th. Blood from the left ventricle of the heart.

124. Chameck Spider Monkey, (*Ateles subpentadactylus*,) a female about two-thirds grown. The following diameters most frequent: 1-3790th, 1-3600th, and 1-3429th of an inch. Extreme sizes 1-4920th and 1-2900th. Blood from a prick of the fore hand.

125. Black Spider Monkey, (*Ateles ater*,) an adult male. All the following sizes very common: 1-3429th, 1-3528th, 1-3555th, 1-3600th, 1-3693rd and 1-3792nd. Extreme diameters 1-4555th and 1-3000th. Blood from a prick of the fore hand.

126. Weeper Monkey, (*Cebus apella*,) a female nearly full-grown. 1-3600th, 1-3554th, 1-3429th, and 1-3368th; most common sizes. Extreme diameters 1-4800th and 1-2666th of an inch. Blood from a prick of the fore hand.

The measurements slightly smaller than afforded by the corpuscles of *C. capucinus* (5.), but as the blood was procured from a dead specimen of the latter, the discrepancy is not greater than may be often observed under similar circumstances in one species.

127. Squirrel Monkey, (*Callithrix sciureus*,) a male about two-thirds grown. The following sizes all very frequent: 1-3790th, 1-3693rd, 1-3600th, and 1-3552nd. Extreme diameters, 1-4800th and 1-3200th. Blood from a prick of the nose.

The blood of the Toque Monkey (81.) was obtained from the heart of a dead specimen. The corpuscles, procured from a wound in the tail, of a healthy full-grown male afforded the following measurements, viz. dry, 1-3764th, 1-3600th, and 1-3552nd. In the serum many disks of the same magnitude were observed, besides a large quantity of smaller size, viz. from 1-6000th, to 1-5333rd of an inch, though the blood was carefully secured in a small glass tube and examined within an hour after it was taken from the animal.

128. The Mole, (*Talpa Europæa*,) recently killed, smallest disks about 1-5000th, the largest about 1-4000th of an inch. Blood from the heart.

For this observation I am indebted to Dr. Davy.

129. Grisly Bear, (*Ursus ferox*,) a female about half-grown. Most common diameters 1-3340th and 1-3552nd of an inch. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the nose.

130. Badger, (*Meles vulgaris*,) an old male. All the following sizes very common: 1-4128th, 1-4000th, 1-3973rd, 1-3810th, and 1-3693rd. Extreme diameters 1-5333rd and 1-3200th of an inch. There were besides some of the very small circular particles as in the genus *Sciurus*. Blood from the integuments of the thigh.

131. Common Jackal, (*Canis aureus*,) an old male. The corpuscles, dried quickly, afforded the following measurements: 1-4000th, 1-3764th, and 1-3840th most frequently, the extreme sizes being 1-4800th and 1-3200th of an inch. In the serum, examined within two hours after they were obtained, 1-4800th was the most common diameter, and the extreme sizes 1-6000th and 1-3200th. Disks of the last, or largest size, as well as of 1-3555th, were not unfrequent, generally collecting together quickly into rouleaux, from which it was seen that the thickness of the edges of these corpuscles was 1-14,000th of an inch. The smaller particles, though much more abundant, did not arrange themselves together by their flat surfaces, and indeed could hardly be seen edgewise.

The blood was obtained freely from a puncture in a vein of the hind leg; and the above facts are merely mentioned as exemplying the variations which may often be observed in the corpuscles only a short time after the blood has been removed, with every care, from various mammals.

132. African Civet Cat, (*Viverra civetta*,) adult male. 1-4615th, 1-4360th, 1-4000th, and 1-3552nd of an inch all frequently observed. Extreme sizes 1-6000th and 1-3200th. The corpuscles in this instance, therefore, very variable in magnitude. Blood from a prick of the tail.

In another adult male, after death, the corpuscles were also extremely variable in size, as the following were all common, viz. 1-5333rd, 1-4760th, 1-4500th, 1-4412th, 1-4365th and 1-4000th; the extreme measurements being 1-6000th and 1-3200th of an inch. Blood from the different cavities of the heart, from the coronary veins, and from the portal vein.

133. Javanese Ichneumon, (*Herpestes Javanicus*,) a full-grown male. All the following sizes common: 1-4800th, 1-4924th, 1-5142nd, and 1-5120th. Extreme diameters 1-6000th and 1-4000th. Blood from a wound at the end of the tail.

134. Chetah or Hunting Leopard, (*Felis jubata*,) an adult

female. All the following sizes common: 1-4365th, 1-4268th, 1-4173rd, and 1-4000th. Extreme sizes 1-5333rd and 1-3555th of an inch. Blood from a prick of the nose.

135. Alexandrian Rat, (*Mus Alexandrinus*, albino, var.,) an adult male. 1-4173rd, 1-4000th, 1-3810th, and 1-3764th, very common sizes. Small corpuscles 1-4800th; the large 1-3200th. Edges of disks 1-14,000th of an inch thick. Blood from a vein of the hind leg.

136. Coendu or Ring-tailed Porcupine, (*Synetheres prehensilis*,) a full-grown male. Common diameters 1-3428th, 1-3309th, and 1-3600th. Extreme sizes 1-4570th and 1-2460th of an inch. Blood from a cut at the end of the tail.

Error in the last paper (No. 2.) p. 108, l. 28, *for* Haller, *read* Harvey.

XXXVII. *Contributions to Electricity and Magnetism. No. III. on Electro-magnetic Induction.* *By* JOSEPH HENRY, *LL.D., Prof. of Natural Philosophy in the College of New Jersey, Princeton*[*].

INTRODUCTION.—SECTION I. *Conditions which influence the induction of a Current on itself.*—SECTION II. *Conditions which influence the production of Secondary Currents.*—SECTION III. *On the Induction of Secondary Currents at a distance.*—SECTION IV. *On the Effects produced by interposing different Substances between the Conductors.*—SECTION V. *On the Production and Properties of induced Currents of the Third, Fourth and Fifth Order.*—SECTION VI. *The Production of induced Currents of the different Orders from ordinary Electricity.*—NOTE *on the investigations of Professor Ettingshausen.*

1. SINCE my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile: and Mr. Watkins has succeeded in exhibiting all the phænomena of hydro-electricity by the same means. Although the principle has been much extended by the re-

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[*] From the Transactions of the American Philosophical Society, vol. vi, having been read Nov. 2, 1838.

searches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phænomenon known in common electricity by the name of the lateral discharge. Circumstances prevented my doing anything further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete, in several points, as I could wish, but as my duties will not permit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts; the first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity*.

5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper riband, which will be designated by the names of coil No. 1, coil No. 2, &c.; also of several coils of long wire; and these, to distinguish them from the ribands, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in fig. 1, which is that of a flat spiral sixteen inches in diameter. It was however sometimes formed

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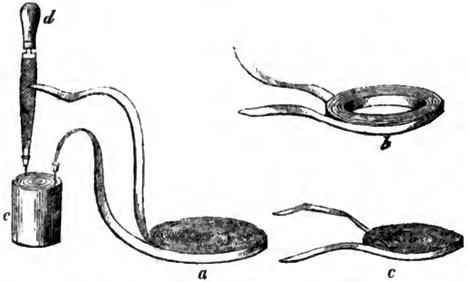
* The several paragraphs are numbered in succession, from the first to the last, after the mode adopted by Mr. Faraday, for convenience of reference.

into a ring of larger diameter, as is shown in fig. 4, Section III.

Fig. 1.

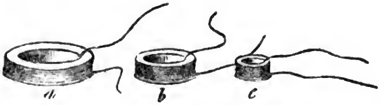


a represents coil No. 1, *b* coil No. 2, and *c* coil No. 3; *e* the battery, *d* the rasp.

7. Coil No. 2. is also formed of copper plate, of the same width and thickness as coil No. 1. It is, however, only sixty feet long. Its form is shown at *b*, fig. 1. The opening at the centre is sufficient to admit helix No. 1. Coils Nos, 3, 4, 5, 6, &c. are all about sixty feet long, and of copper plate of the same thickness, but of half the width of coil No. 1.

8. Helix No. 1. consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{49}$th of an inch in diameter; No. 2, of nine hundred and ninety yards; and No. 3, of three hundred and fifty yards, of the same wire. These helices are shown in fig. 2, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations, seven helices of different lengths. The wire is covered with cotton thread, saturated with bees-wax, and between each stratum of spires a coating of silk is interposed.

Fig. 2.



a represents helix No. 1, *b* helix No. 2, *c* helix No. 3.

9. Helix No. 4 is shown at *a*, fig. 4, Section III.; it is

formed of five hundred and forty-six yards of wire, $\frac{1}{49}$th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire $\frac{1}{123}$th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favoured with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{16}$th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horseshoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and $\frac{2}{3}$ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develope the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in fig. 1. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged; but all the results mentioned in the paper, except those with a Cruickshanks trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp, *b*, is shown in the same figure.

SECTION I.—*Conditions which influence the induction of a Current on itself.*

14. The phænomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current, discovered by Dr. Faraday. The two, however, belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine

more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat riband coil No. 1, ninety-three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are, however, very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five riband coils, making an aggregate length of three hundred feet, and the small battery, fig. 1, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is, however, a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire $\frac{1}{16}$th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short riband coil decreases. With a Cruickshanks trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent. When the current, from ten plates of the above-mentioned trough, was passed through the wire of the large spool (10.), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction when the contact was broken. Also a slight shock in this

arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark, however, with the long wire and compound battery is not as brilliant as with the single battery and the short riband coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10.). A very small compound battery was formed of six pieces of copper bell wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current, however, was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable intensity*, and *small quantity*.

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in fig. 4, is most effectual.

24. In all cases the several spires of the coil should be well insulated; for although in magnetizing soft iron, and in analogous experiments, the touching of two spires is not attended with any great reduction of action, yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would, in some cases, interfere with the action, and render the results more complex.

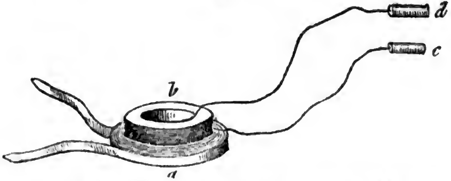
SECTION II.—*Conditions which influence the production of Secondary Currents.*

26. The secondary currents, as it is well known, were discovered in the induction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then, however, attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine. And I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phænomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to ensure perfect insulation; as often as the circuit of No. 1 was interrupted, a powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in fig. 3, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

Fig. 3.



a represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends were joined by the magnetizing spiral (11.), the enclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horseshoe (12.), magnetism was developed; and when the ends of the second coil were attached to a small decomposing apparatus, of the kind which accompanies the magneto-electrical machine, a stream of gas was given off at each pole. The shock, however, from this coil is very feeble, and can scarcely be felt above the fingers.

30. This current has therefore the properties of one of moderate intensity, but considerable quantity.

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4 and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or, in other words, the intensity of the induced current was increased by an increase of the length of the coil, while the quantity was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2. and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects, with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decomposition less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only $\frac{1}{125}$th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11.), and with the whole length of this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the fore-finger and thumb. Sparks and decomposition were also produced, and needles rendered mag-

netic. The wire of this spool is $\frac{1}{16}$th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33.) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism. Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lenz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents which, according to the theory of Ampere, exist in the magnet, are, like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshanks trough of sixty pairs substituted. When the current from this was passed through the riband coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix, No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was there-

fore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from the battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an intensity current can induce one of quantity*, and by the preceding experiments, the converse has also been shown, that *a quantity current can induce one of intensity.*

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favour the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the velocity would be the same. This, however, is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted to give only the general conditions which influence the galvanic induction. To establish the law would require a great number of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity,

and also more perfect means than we now possess of measuring the amount of the inductive action; all these circumstances render the problem very complex.

[To be continued.]

XXXVIII. *Researches of* Mons. R. Piria *on the Combinations of Salicyle**.

EVERY one who is at all acquainted with the gigantic strides made in organic chemistry since the discovery of the real nature of the oil of bitter almonds, and the development of the remarkable combinations of benzoyle, must have hailed with peculiar pleasure the discovery of an analogous series of compounds having for their base a compound radical termed spiroil, from its being present in the oil of the *Spiræa Ulmaria*, or meadow-sweet. This body was discovered by Löwig, who ascertained the volatile oil of the Spiræa to be really an hydracid, consisting of a compound radical analogous to benzoyl, combined with hydrogen. The researches of Löwig have been already presented to the English reader in the pages of the valuable Scientific Memoirs of Mr. R. Taylor. We have now the pleasure of laying before our scientific readers an account of a valuable series of researches of M. Piria on a new compound organic base, bearing considerable resemblance to benzoyl and spiroil, and promising, from this very resemblance, to throw much light on the nature of the respective combinations of these curious bases.

The active principle of the bark of different species of salix has been long known to chemists, and salicin is now an ordinary article of commerce, being employed in medicine as a substitute for quinine, as a remedy in intermittent fever.

Salicine was first obtained in a white crystallizable state, by M. Leroux, and has been submitted to ultimate analysis by MM. Jules Gay-Lussac and Pelouse. Piria has also analysed it, and its per centage composition was in three experiments found to be as follows:—

	Exp. 1.	Exp. 2.	Exp. 3.
Carbon.........	55·68	55·04	55·54
Hydrogen	6·36	6·39	6·43
Oxygen	37·96	38·57	38·03
	100	100	100

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* For this account of M. Piria's researches, the Editors are obliged to Dr. Golding Bird.

Berzelius suggests the probability of the atom of salicine consisting of (2 C, 2 H, O). To ascertain this fact by experiment, Piria dissolved salicine in warm water, added a few drops of ammonia, and then dropped in a solution of the tribasic acetate of lead: a white flocculent precipitate was obtained. This was collected, dried, and submitted to analysis, the result of which proved the *crystallized* salicine to consist of

		Per cent.
21 atoms carbon...... =	126 =	55·76
14 —— hydrogen..	14	6·06
11 ——oxygen......	88	38·18
Weight of atom ...	228	100

The *anhydrous* salicine contains two atoms less of water, and consists of

		Per cent.
21 atoms carbon...... =	126 =	60·49
12 —— hydrogen..	12	5·63
9 —— oxygen.....	72	33·88
Weight of atom	210	100

Döbereiner has long ago shown that salicine, when distilled with oxidating bodies, as a mixture of sulphuric acid and peroxide of manganese, yielded a larger proportion of formic acid than any other known substance. By distilling it, however, with other oxidating agents, taking care to avoid an excess of acid, Piria obtained a distinct product, which consisted of an organic base combined with hydrogen, which, in conformity with the adopted nomenclature, he has named *hydruret of salicyle*: like benzoyl and spiroyl, salicyle has not yet been obtained in a free state. It bears so close an analogy to the former, that they may be considered as having one common radical, as may be seen by comparing their elementary composition.

	Carbon.	Hydrogen.	Oxygen.
Benzoyl =	14 atoms	5 atoms	2 atoms
Salicyle	14	5	4

and thus benzoyle and salicyle may be respectively considered as two oxyds of an hydrocarbon, consisting of (14 C + 5 H).

Dumas has, as is well known, suggested that benzoyle and benzoic acid may be considered as two oxides of this hypothetical hydro-carbon or benzogene, in which case salicyle and salicic acid may be considered as two other members of the same group, thus—

Radical $(14\,C + 5\,H)$.	Oxygen.	
1	2	= benzoyl.
1	3	anhydrous benzoic acid.
1	4	salicyle.
1	5	anhydrous salicic acid.

Hydruret of Salicyle.

This substance when pure is colourless, but when impure is deep red, possessing an agreeable and aromatic odour, to a certain extent resembling that of the oil of bitter almonds: by distillation it is rendered completely colourless, by exposure to air it again acquires a reddish hue. Digested with water a small quantity is dissolved, sufficient however to cause the latter fluid to acquire its odour; its taste is very pungent and acrid, like that of most essential oils. It is without action on litmus paper. Placed in contact with the salts of the sesqui-oxide of iron it assumes a splendid violet colour, which by exposure to air becomes yellow. The salts of the protoxide of iron do not exert any manifest action upon it. In æther and alcohol it is soluble in all proportions: water precipitates it from its alcoholic solutions. Its specific gravity at 56° Fahr. is 1·1731: it boils at a temperature of about 400°. Mixed with alkaline carbonates it slowly decomposes them, causing the evolution of their acid; upon the application of heat this action becomes much more energetic. Caustic alkalies combine with it, producing a considerable disengagement of heat. Chlorine and bromine combine with the base of the hydruret, producing chloride and bromide of salicyle, disengaging hydrochloric and hydrobromic acids; iodine dissolves in the hydruret without suffering any obvious change.

Digested in nitric acid, hydruret of salicyle is converted into a substance termed *nitro-salicide*, which becomes, by continuing the digestion, changed into carbazatic acid.

The hydruret of salicyle may be readily prepared by the following process. Dissolve four parts of bichromate of potass in a sufficient quantity of water, and add three parts of strong sulphuric acid. Then dissolve some salicine in six parts of hot water, place the solution in a retort, and raise it to the boiling temperature. Adapt a carefully cooled receiver, and through the tubulure of the retort add, by small quantities at a time, the acid solution of bichromate of potass to the hot fluid; violent action ensues; the mixture turns green from the formation of green sulphate of chromium, and a milky fluid distils over. The products of this distillation by repose deposit salicyle in the form of oily drops. These drops should be collected, and purified by being distilled with chloride of calcium. It is important that no more acid be used than is here prescribed, otherwise a quantity of formic acid is produced, and a corresponding diminution in the quantity of hydruret of salicyle ensues.

The hydruret consists of

	Carbon.	Hydrogen.	Oxygen.
Salicyle ...... =	14	5	4
Hydrogen ...		1	
	—	—	—
Atomic weight ..	14	6	4

Hence it appears that hydruret of salicyle is isomeric with hydrated benzoic acid; and it is not a little remarkable, that the density of its vapour is identical with that of the latter acid, as determined by Dumas, being 4·276. It therefore consists by volume of

7 volumes vapour of carbon
3 —— hydrogen
1 —— oxygen

The hydruret of salicyle may be considered as a true hydracid, with a compound base like hydrocyanic acid; like that compound it combines with metallic oxides, its hydrogen forming water with the oxygen of the oxyd, and a *salicide* of the metal results. The metallic salicides are isomeric with the corresponding anhydrous benzoates of the oxides, thus placing M for an atom of a metal, and M + O for one of a metallic oxide; a given *benzoate* and *salicide* will consist of

	Carbon.		Hydrogen.		Oxygen		
Benzoate =	(14	+	5	+	3)	+	(M + O)
Salicide =	(14	+	5	+	4)	+	M

So that the hydruret of salicyle bears the same relation to hydrated benzoic acid as oxalic acid, according to the view of Dulong $(\overline{2\,C, 4\,O} + H)$ does to the generally received view of its composition at the present day, or $(\overline{2\,C, 3'O} + H\,O)$.

Metallic Salicides.

The salicide of potassium may be very readily prepared, by mixing the hydruret of salicyle with a strong solution of pure potass. On shaking the mixture, a considerable quantity of salicide of potassium separates in small yellow crystals from the supernatant alkaline fluid. These crystals must be collected, freed from adhering moisture by pressure between folds of bibulous paper, and dissolved in alcohol; by crystallization numerous well-defined square tables of the salicide are obtained. This salt is readily soluble in water and alcohol. When quite dry it undergoes no change by exposure to the air, but when wet it absorbs oxygen, becomes covered with numerous green spots, which ultimately turn black; the whole mass becoming eventually a black sooty powder, which will be alluded to in the course of these remarks on account of a peculiar acid which it contains.

A solution of salicide of potassium is precipitated yellow by salts of lead, silver, mercury, manganese, and barium. Salicide of ammonium is procured by mixing liquid ammonia with the hydruret of salicyle, in the same manner as the corresponding salt of potassium. It crystallizes in yellow needles, and by exposure to the air is soon decomposed into its constituents, ammonia being copiously evolved.

Salicide of barium is obtained by precipitating a solution of salicide of potassium by chloride of barium. It may be obtained in minute needles, but is very slightly soluble in water. It consists of

	Atoms.		Theory.	Exp.
Carbon	14	= 84	40·93	41·15
Hydrogen ...	7	7	3·34	3·41
Oxygen......	6	48	22·96	22·57
Barium	1	68	32·77	32·87
Atomic weight...		207	100	100

it hence contains two atoms of water, and when dried in Liebig's desiccating apparatus, it gives up this quantity, 1·237 parts losing 0·110 parts of its weight.

Salicide of copper may be prepared by dissolving freshly precipitated oxide of copper in hydruret of salicyle, and evaporating to dryness over a water bath; it forms a light yellow powder, which, when heated, partly sublimes in iridescent scales. Submitted to analysis, it was found to consist of

	Atoms.		Theory.	Exp.
Carbon	14	= 84	55·50	55·75
Hydrogen ...	5	5	3·24	3·47
Oxygen......	4	32	20·74	20·70
Copper	1	32	20·52	20·08
Atomic weight...		153	100	100

Salicic Acid.

This compound may be obtained by heating hydruret of salicyle with an excess of potass. The mixture becomes deep reddish-brown, and hydrogen is evolved. Dissolve the resulting mass in water, and add hydrochloric acid in excess; a copious deposit of salicic acid in fine needles occurs. This acid closely resembles the benzoic; it is scarcely soluble in cold water, readily soluble in hot water, as well as in alcohol and æther. By heat it sublimes with great facility. Mixed with alkaline carbonates, salicic acid decomposes them, driving off carbonic acid, and forming a series of salts. Digested with sulphuric acid, the new acid undergoes no obvious change until the mixture is heated, and then the whole turns black. Nitric acid does not affect salicic acid in the cold; on applying heat, however, violent and tumultuous action ensues, dense red fumes are evolved, and a yellow solution is obtained; this, by repose, deposits minute yellow bitter crystals, which appear to be identical with the nitro-salicide obtained by the action of nitric acid on hydruret of salicyle.

Salicic acid contains an atom of combined water, which it loses when it combines with bases; hence the crystallized acid is a salicate of water, or (14 C, 5 H, 5 O, + H O). Submitted to ultimate analysis, this consists of

	Atoms.			Theory.	Exp.
Carbon	14	=	84	61·32	61·10
Hydrogen...	6		6	4·29	4·41
Oxygen	6		48	34·39	34·43
Atomic weight...			138	100	100

Salicate of silver was prepared by precipitating a solution of salicate of ammonia by nitrate of silver; this salt is a white insoluble powder, and consists of

	Atoms.			Theory.	Exp.
Carbon	14	=	84	34·70	34·91
Hydrogen ...	5		5	2·02	2·09
Oxygen	5		40	16·22	16·43
Oxyd of silver	1		116	47·06	46·57
Atomic weight...			245	100	100

Chloride of Salicyle.

This compound may be obtained by a process precisely similar to that used for the preparation of the chloride of benzoyl, by transmitting a current of dry chlorine gas through pure hydruret of salicyle; much heat is evolved, the chlorine unites with the hydrogen of the hydruret to form hydrochloric acid, which is copiously given off in a gaseous state, whilst the salicyle itself unites with more chlorine to form the chloride. A nearly solid crystalline mass is obtained, which should be purified by solution in alcohol and subsequent crystallization. The chloride of salicyle thus obtained is insoluble in water and acids; but in æther, alcohol, and alkaline fluids, as well as ammonia, it readily dissolves, in the latter case undergoing some remarkable changes. From its solutions in the fixed alkalies, acids throw it down unchanged. Submitted to ultimate analysis, chloride of salicyle is found to consist of

	Atoms.			
Carbon ...	14	=	84	54·18
Hydrogen.	5		5	3·16
Oxygen ...	4		32	20·25
Chlorine...	1		36	22·41
Atomic weight...			157	100

so that the atom of hydrogen in the hydruret of salicyle becomes replaced by an equivalent of chlorine, as in the analogous compounds of benzoyl and spiroil.

Bromide of Salicyle.

This compound is produced whenever bromine is added to hydruret of salicyle, heat is evolved, and the whole consolidates into a crystalline mass, which, like the chloride, may be purified by solution in alcohol and subsequent crystallization. In this substance the hydrogen of the hydruret is replaced by an equivalent of bromine, the whole consisting of

	Atoms.			
Carbon ...	14	=	84	42·62
Hydrogen.	5		5	2·48
Oxygen...	4		32	15·94
Bromine ..	1		78	38·96
Atomic weight...			199	100

Combinations of Salicyle with Amidogene.

When a current of gaseous ammonia is passed over chloride of benzoyl, it has been shown by Woehler and Liebig to suffer decomposition; a mixture of chloride of ammonium and benzamide being produced, the latter compound being a combination of benzoyl with amidogene (N + 2 H), analogous to oxamide.

When a current of dry ammonia is passed over chloride of salicyle it becomes absorbed, and a yellow pasty mass results, which must be frequently broken up and again exposed to the gas to effect its entire decomposition. No hydrochlorate of ammonia is formed, the ammonia removing oxygen from the compound without abstracting any chlorine, and forming water, which condenses in the tube in which the experiment is performed. The resulting compound is *Chlorosamide*; it is a yellow crystalline body, soluble in alcohol and æther, nearly insoluble in water; the addition of weak alkalies causes the disengagement of ammonia; by heating it in an acid fluid it is decomposed, a salt of ammonia being formed, and chloride of salicyle being set free. *Chlorosamide* consists of

	Atoms.			
Carbon ...	14	=	84	56·52
Hydrogen.	5		5	3·30
Oxygen...	2		16	10·57
Nitrogen..	$\frac{4}{3}$		18·4	6·23
Chlorine..	1		36	23·38
Atomic weight...			159·4	100

When gaseous ammonia is made to act on bromide of benzoyl, a compound called *bromosamide* results; its composition is identical with chlorosamide, with the substitution of the chlorine by its equivalent of bromine.

Action of the air on Salicide of Potassium.

When salicide of potassium in a perfectly dry state is exposed to the air, or to an atmosphere of oxygen gas, it undergoes, as has been already observed, no obvious change; but if this salt be previously moistened, it, by a similar exposure, alters in colour, its surface becomes covered with green specks, and it darkens in hue until the whole mass becomes completely black. If the moist salicide of potassium be placed in a receiver of oxygen gas inverted over mercury, the latter will be observed to rise rapidly in the glass, from the absorption of its gaseous contents. When the black, changed, salt is digested in water, a certain portion dissolves, and a soot-like powder is left; this should be collected, washed, and dried. This black compound dissolves readily in alcohol and æther, but is insoluble in water; it dissolves in alkaline solutions, forming saline compounds, from which the black powder is thrown down unchanged by acids. This substance appears to possess properties of a decidedly electro-negative character, and from this circumstance, as well as from its colour, it has been termed *melanic acid*, a name unhappily chosen, as it has already been applied to an ingredient in certain morbid animal secretions.

Melanic acid decomposes alkaline carbonates, forming saline combinations, attended with the evolution of carbonic acid gas. When heated this acid burns slowly, but without flame, and leaves no fixed residue.

Melanate of silver was prepared by precipitating a solution of melanate of ammonia by nitrate of silver; the precipitate was dried and submitted to ultimate analysis, and was found to consist of

	Atoms.			Theory.	Exp.
Carbon	10	=	60·0	27·63	27·67
Hydrogen	4		4·9	1·71	1·95
Oxygen	5		40	18·18	18·82
Oxyd of silver	1		116	52·48	51·56
Atomic weight ...			220	100	100

hence the acid itself consists of

	Atoms.			
Carbon ...	10	=	60	58·16
Hydrogen.	4		4	3·80
Oxygen ...	5		40	38·04
Atomic weight ..			104	100

The aqueous solution obtained by washing the decomposed salicide of potassium, during the separation of melanic acid, was submitted to examination, and after evaporating it to a small bulk, mixing it with sulphuric acid, and submitting it to distillation, acetic acid was obtained, whilst sulphate of potass was left in the retort.

From these experiments, it appears, that salicide of potassium, when moistened and exposed to the air, or to an atmosphere of oxygen gas, is resolved into melanic acid and acetate of potass; and for every atom of salicide of potassium decomposed, an equivalent of acetate of potassa is produced. For this decomposition to be understood, we must admit that three atoms of oxygen and the elements of two atoms of water, are appropriated by each equivalent of the salicide of potassium, thus

			Carb.	Hyd.	Oxy.	Potassium.
Melanic acid		=	10 +	4 +	5 +	0
Acetic acid		=	4 +	3 +	3 +	0
			14 +	7 +	8 +	0
Minus	2 atoms water	=		2 +	2 +	0
			14 +	5 +	6 +	0
	3 atoms oxygen	=			3 +	0
			14 +	5 +	3 +	0
1 atom potassa		=			1 +	1
1 atom of salicide potassium		=	14 +	5 +	4 +	1

Decomposition of Salicin by Sulphuric Acid.

When salicin is immersed in strong sulphuric acid it assumes a blood-red tint; but when digested at a boiling temperature in the acid previously moderately diluted with water, the salicin dissolves, forming a colourless solution. If a solution thus prepared, be poured into cold water, a white precipitate falls down of a resinoid character. This substance is turned red by sulphuric acid; like the unchanged salicin, it readily dissolves in alkaline fluids. Submitted to ultimate analysis, this new substance, which from its resinoid characters is termed *saliretin*, was found to consist of

Carbon....	72·96
Hydrogen..	5·83
Oxygen....	21·21
	100

No gas is disengaged during the formation of saliretin; indeed the action of the sulphuric acid appears to be purely catalytic, as in the cases of ætherification, and the formation of starch-sugar. On examining the acid fluid from which saliretin has been precipitated, it was found to contain grape-sugar; so that under the influence of the catalytic action of sulphuric acid, salicin is resolved into saliretin and grape-sugar, from the re-arrangement of its elements; affording another example of the resolution of organic products into new arrangements, under the influence of *catalysis*, or action of presence.

Action of Chlorine on Salicin.

When a current of gaseous chlorine is transmitted through a quantity of salicin diffused in water, solution ensues, and a yellow fluid is formed; and if a sufficient quantity of salicin be present, a yellow crystalline mass is deposited.

These crystals are but slightly soluble in water or alkaline fluids; they possess a very disagreeable odour and pungent taste; submitted to analysis, they were found to consist of

	Atoms.			
Carbon....	21	=	126	42·94
Hydrogen..	12		12	4·00
Chlorine...	2		72	23·65
Oxygen....	11		88	29·41
Atomic weight...			298	100

so that, by this process, salicin loses two atoms of hydrogen, and gains two atoms of chlorine; a fact quite conformant with the doctrine of substitution. If the fluid through which the chlorine is passed be kept at a boiling heat, no crystals are formed, but a reddish oily fluid is produced; this, when analysed, was found to consist of

	Atoms.			
Carbon	21	=	126	38·61
Hydrogen ..	$8\frac{1}{2}$		8·5	2·55
Chlorine ...	$3\frac{1}{2}$		126	37·22
Oxygen....	9		88	21·62
			348·5	100

In this case, the *anhydrous* salicine loses $3\frac{1}{2}$ atoms of hydrogen, and gains an equivalent proportion of chlorine. This new compound is soluble in alcohol and alkaline solutions.

G. B.

XXXIX. *Letter to Richard Taylor, Esq., as Editor of the Philosophical Magazine and Journal. By* R. POTTER, *Esq., B.A., F.C.P.S.*

DEAR SIR,

THE part you have taken in the controversy between Professor Forbes and myself has a good deal surprised me. That you should deprecate "personal imputations" in controversial papers, is what every reader of your periodical must heartily approve. However searching a review of Professor Forbes's "memorandum" was required for the defence of my own investigations, I congratulate myself on having avoided personalities or any imputation of unworthy motives. It is a philosophical and legitimate line of defence to inquire whether your opponent is a competent judge of the matter in controversy, from the consideration he has given to the subject, and whether he is to be considered in the light of an impartial and unbiassed inquirer, or otherwise whether he has imbibed theoretical views so deeply as to place him only in the situation of a partisan of the particular theory adopted by him. This course I have always endeavoured to pursue, and would cheerfully concede to an opponent. I think, however, that I have just reason to complain of the admission of such terms as "gratuitously misinterpreted," and "take a pleasure in misinterpreting my expressions," admitted in the very communication to which your note was appended.

In respect to the unsupported quotation from your unnamed correspondent's letter, which you have adopted, I have only to express my surprise that you did not suspect more than a filial solicitude for the Society's honour on his part. My notice of the proceedings of the Society to which I have the honour to belong, and amongst the leading members of which I have the honour to number so many scientific friends, could arise from no other motive than a desire that its authority and pre-eminent position in the scientific world might be permanent and undiminished, by allowing no analytical essays relating to physical problems to be ushered forth under its auspices, until, in all practicable cases, their accordance with the involved physical facts was ascertained. The notice of one such essay in my former paper was sufficient to show that such had not always been the case. Your correspondent prudently preferred leaning on his influence with you, to challenging me to the proof of my assertion.

I have also to ask at your hands an explanation of the editorial censure contained in your note. I fully bow to your claim of right to moderate any expressions in communications which may be sent to you for publication. The advantage of a review by an impartial editor is great to all parties in a controversy, who in their excitement and sensibility naturally see a poignancy in the expressions of their opponents which they do not suspect in their own. On the other hand, I maintain that it is an unheard of proceeding in an editor who has published a paper without remark or private notice to the author, to pronounce a censure such as is contained in your note; and I claim from you this admission, which I think you will allow to be due under the circumstances.

I remain, dear Sir, yours truly,

Queen's College, February 4, 1840. Richard Potter.

In inserting Mr. P's letter, we have only to state that the Editor's note was written in consequence of the remonstrance of the Member of the Cambridge Phil. Soc., and before the receipt of Prof. Forbes's letter, with which it had no connection; and was indeed intended to have occupied a distinct place in the Number.

We can assure Mr. Potter that his surmise respecting influence with us is unfounded; and willingly express our regret, that from having overlooked some portions of his communication, they should have become the subjects of public instead of private discussion.—R. T.

XL. *Additional Note on the Authenticity of the disputed Passage in the treatise of Boetius* de Geometria *on Numerical Contractions. By* J. O. Halliwell, *Esq., F.R.S., F.S.A., F.R.A.S., &c.*

I TRANSLATE the following extract from a letter which I have recently received from M. Chasles, because the view which he takes of this question is new, and his supporting arguments forcible:—" In a passage found in some MSS. at the end of the second book of Boetius, the expression *mensa geometricalia* occurs; and this calculus is mentioned as having been employed chiefly by the geometers. This explains why Boetius introduced that passage into his treatise on geometry; and in his treatise on arithmetic, which treats on the properties of numbers, no mention of it is made. This latter work is indeed only a new version of the treatise by Nicomachus on the same subject."

This ought to be compared with what has been stated in the number of this Magazine for December, and it will be seen that it is quite destructive of M. Libri's principal argument. I may add, in corroboration of the opinion of M. Chasles, that Abelard's tract in the Leyden library is entitled *de Doctrina Abaci vel radii Geometrici;* the manuscript itself is thus described in the printed catalogue:—"Adolardus, qui statim in principio dicitur philosophorum assecla ultimus, de doctrina abaci, vel radii geometrici, ut ipse scribit quoque vocari. In fine legitur, *Regularum abaci nobilis arithmetici tractatus explicit feliciter.*"

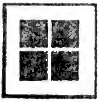
And now a word with M. Libri. When he says, "Si l'opinion de M. Halliwell avait été aussi explicite que le pense le savant géomètre de Chartres, il semble qu'on n'aurait pas dû employer plusieurs pages pour tâcher de le prouver," he had forgotten that the *plusieurs pages* were the produce of his own pertinacity. When I had explicitly stated that the Bodleian manuscripts indicated a knowledge of the value of local position, and that one of them actually made use of the *sipos*, surely no one could reasonably accuse me of withholding my assent from the explanation given by M. Chasles. Much less, in that case, could there have been a necessity for occupying the attention of two meetings of the Institute on a mere question of opinion.

XLI. *On a Pseudomorphous variety of Iodide of Potassium* *By* ROBERT KANE, *M.D., M.R.I.A.*

DURING the crystallization of a large quantity of iodide of potassium, in the manufacturing laboratory of Apothecaries' Hall, I observed a large group of long prisms to be formed, of great lustre and regularity. These prisms were in many cases terminated by four-sided pyramids, formed by the joining of four rhomboidal planes, by which the solid angles of the prism were replaced; and I succeeded in obtaining a series of specimens, in some of which the prism was simple and terminated by a single plane perpendicular to its axis; in others the solid angles were replaced by very minute triangular planes, rendering the terminal plane octagonal, and finally, as the triangular planes increased in size, square, the diagonals of the square being parallel to the sides of the original surface, and hence the gradual conversion of the replacing triangles into rhombs, which effacing the terminal plane formed the pyramidal summit already noticed.

The prisms are straight, with square bases. The replacing triangular or rhomboidal planes form with the lateral edges, to which it is inclined, an angle of 150°, and with the terminal plane an angle of 120°: its angles with the vertical and horizontal axes of the crystal being therefore 60° and 30°, giving the ratio of the axes therefore as 1 : 1·73. The angle formed by two adjacent rhomboidal planes of the pyramid was found to be 105°, and that of the summit formed by two opposite rhomboidal planes was 60°. The angle across the summit, measured on the edges of the rhomboidal planes, was 80°, and that of a rhomboidal plane, on the adjacent side, was 140°. The angles of the rhomboidal plane were 60° and 120°.

In these measurements I could not obtain greater accuracy than within a degree, from the circumstance that the replacing surfaces were not, in reality, planes, but portions of spherical or at least curved surfaces of great radius, so that the adjacent edge had different inclinations to different portions of the rhombic surface. In addition to this peculiarity, other marks of a complex or macled structure were very evident. The smaller crystals, although equally well marked as to form and replacements with the larger, differed from them in being wholly clean and transparent. The larger ones, on the contrary, consisted of three distinct portions, the external being a hollow shell of perfectly transparent material, the next being a core of opake white substance, apparently porous and granular, as if formed of a congeries of minute crystals independent of the case in which they were contained, whilst in the centre there was to be seen a delicate but well-defined transparent rectangular cross, the arms of which generally penetrated quite through the opake substance and united with the external transparent shell.



A section of such a crystal had in fact the appearance represented in the little sketch, the white opake portion being shown shaded.

These crystals possessed single refraction. They had no action on polarized light transmitted along their axis; and hence, although with a pyramid belonging to the square prismatic system, they belonged really to the regular system, by a congeries of minute crystals (probably cubes) of which they must be formed. In solubility they were the same as the common iodide of potassium, with which their composition likewise identified them. From their having formed on the top of a large cross of common crystals, they must have been generated under circumstances either on a lower temperature or a less concentrated solution than that by which the common variety is produced. If the new-formed crystals be dissolved in water, it is under the ordinary form that they recrystallize.

When iodide of potassium is crystallized in leaden or tinned iron vessels, Mr. Scanlan has informed me that the crystalline form is altered, from the presence of a minute quantity of iodide of tin or lead; but what the alteration is exactly, I do not believe has been determined. Having heard the fact, however, from my friend Mr. Scanlan, I sought for metallic impurity in the crystals now described, but in vain. They are chemically pure.

XLII. *Proceedings of Learned Societies.*

ROYAL IRISH ACADEMY.

Nov. 11, 1839. REV. Dr. Dickinson gave a verbal account of a remarkable waterspout, which he had observed at Killiney during the last summer.

Towards the end of the month of July, about 10 A. M., while standing on the shore of the bay of Killiney, his attention was directed by a friend to a waterspout, distant about a quarter of a mile from the land. It was not similar in form to the representations of waterspouts usually given, and may therefore deserve to be noticed. It was shaped like a double siphon, the whole being suspended at a considerable elevation in the air; the longer end of the siphon reached towards the sea, and appeared to approach it nearer and nearer, till, at length, its waters were distinctly seen rushing into the deep. The loop gradually lowered, as if sinking and lengthening by its own weight, while the upper part of the siphon seemed not to lose in elevation. At length the loop burst, and there were three streams of water pouring into the sea, two of those streams still continuing united by the arch at the top. The breadth of these streams gradually diminished till they became invisible, but their length seemed undiminished as long as they were at all seen. The quantity of water poured down must have been very considerable, as the bubbling of the sea beneath could be distinctly observed.

Dr. Dickinson was informed that a waterspout fell a few days after inland, towards the Three-Rock mountain. It is said to have done some injury; but his informant did not see it, and he could not, therefore, ascertain its shape.

November 30, 1839. Mr. Clarke read a paper "On Atmospheric Electricity."

The author commenced his paper with a description of the apparatus which he had employed in the experimental investigation of this subject. He showed the inapplicability of the electrometers hitherto employed, and exhibited a highly insulated galvanometer, containing about three thousand turns of very fine wire covered with silk, varnished and baked,—which instrument, although exquisitely sensitive to the feeblest voltaic electricity, was not at all acted upon by atmospheric electricity of the low tension which exists during serene weather in this country. Mr. Clarke added, that although the application of such an instrument would be a great desideratum in experiments on atmospheric electricity, and in this point of view had been recommended by the highest scientific authorities in Europe, yet he had reason to think that it had never, in any country, been deflected by atmospheric electricity in serene weather.

The author then exhibited the electrometer which he had devised for, and used in his experiments on this subject. It consisted of a bell of glass, seven inches in diameter, through the side of which passed a sliding graduated rod, furnished with a vernier, which indicated the distance, in hundredths of an inch, through which a single pendent slip of leaf gold was attracted towards the rod which was in connexion with the earth. The slip of leaf gold was attached to a vertical and well-insulated rod, which passed through a collar of leathers, and could therefore be raised or depressed, as required by the varying intensity, so that the lower end of the leaf should always, when electrified, be a tangent to the ball terminating the graduated rod.

The author then alluded to the received opinion, that the Aurora Borealis is an electric discharge of considerable intensity occurring near the polar regions, at great heights in the atmosphere, where the air is necessarily rare, and where, consequently, the electric light (as shown in our artificial imitation of the phænomenon) must be very much diffused and ramified. Hoping to throw light upon this subject, he had made a series of observations on the electric intensity of the twenty-four hours, commencing at mid-day on the 12th of Nov, 1838, and continued at intervals of fifteen minutes,—except during the appearance of the Aurora, when they were made every five minutes, and even oftener. The results of these observations were laid down in a chart, which exhibited the intensity of the electric fluid during these twenty-four hours, a period including that of the magnificent crimson Aurora, which was observed on the night of the 12th, and morning of the 13th of November, 1838, over every portion of the globe. It appeared, by this chart, that the electric intensity during the existence of this magnificent display of Auroral light was but little above the mean electric intensity of that hour during the month; from which the author inferred that this phænomenon, if at all electric, occurred at such a distance as to be unable to affect the apparatus.

The author then proceeded to give an account of the extended series of experiments which he had undertaken at the recommendation of the Academy, and which he had continued during twelve months, at intervals of fifteen minutes, during at least ten days, and from three to seven nights in each month. He stated, that when he had undertaken this series of experiments, he had the following objects in view—namely, to determine the mean amount of electric intensity existing in this country, at the different hours of day and night, and the periods of maxima and minima; and, secondly, to endeavour to trace the cause of this varying intensity to the influence of some of the recognised agents in nature,—such as the variations of atmospheric pressure; the variations of temperature; or the varying quantity of vapour in our atmosphere.

He was happy to announce, that he had not only determined the mean monthly and annual force of electricity at the several hours of the day and night, but also had succeeded in establishing its dependence upon two, out of the three agents, with which he had originally proposed to investigate its connexion. The two with which he has established its connexion and proved its dependence are, *temperature*, and the total *quantity of moisture* present in the air, as shown by the dew-point. Indeed these two phænomena, as the author remarked, are referrible to each other, the temperature producing evaporation, and the force of electricity at any period being shown to be almost exactly proportional to the tension of the vapour so produced.

The hour of the first electric minimum was shown to be about 3 A.M., the electricity increasing with the temperature until 10 A.M., when a slight decrease occurred: the electric tension again commences rising at about 11 A.M., and continues to increase until about 2^h 45^m, P.M.—all these movements being in exact proportion to the elevation of the dew-point and temperature. At 3 P. M. the dew-point and temperature begin gradually to lower, as does also the electricity (but not so quickly); but from 5 to 7 P. M., the electric intensity rises, being acted upon and increased by the precipitation of the evening dew, which has set free the latent electricity of the condensed vapour, in conformity with the experiment of Volta. Again, from 7 P. M., the electric intensity weakens rapidly, and descends in common with the dew-point and temperature, until they all reach their minimum about 3 A. M.

Thus the patient investigation of this subject has laid bare the cause of the varying diurnal intensity of the electric fluid,—showing it to be the result of evaporation, which, besides its agency in carrying the electric fluid from our earth to the upper regions of the air, daily returns it to us by the conducting power of this vapour, in the direct proportion of its quantity.

Dr. Apjohn read a note by George J. Knox, Esq., "On the Oxidating Power of Glass for Metals, and on the want of Transparency in ancient Glass."

"In a late work, which treats of the manufacture of glass, an experiment of Guyton Morveau is mentioned, in which six per cent. of copper filings having been mixed with pounded glass, and the compound completely melted, it was found to have assumed a red colour uniformly diffused throughout the mass, so deep as to render the glass nearly opake. The experiment originated from a workman in the glasshouse having dipped a heated copper ladle into a pot of fused glass. The copper ladle was melted; the casting and annealing of the plates were proceeded with as usual; and on their completion the workmen were surprised to find, that not only were grains of metallic copper imbedded in the substance of the glass, but bands uniformly coloured of a fine bright red, were distributed throughout the mass.

"The experiment of Guyton Morveau, being but a repetition of the accidental one made by the workman, seems to have but little engaged his attention, the colour being conceived to be due to an *imperfect* state of oxidation, as oxide of copper imparts to glass a greenish colour.

"It appeared to me, at first sight, that the red colour was due to the actual solution of the copper in the metallic state, the globules of copper imbedded in the mass having been deposited from a state of solution, upon cooling. To determine this, I mixed in different proportions with powdered glass, iron, lead, copper, silver, bismuth, antimony, tin, gold, platinum, in a minute state of division; and found that glass, when mixed with iron filings, will oxidate and dissolve almost as much iron, when mixed with it in the metallic state, as if it were mixed with it in the state of oxide. Of copper, only a small proportion is oxidated and dissolved, imparting a green colour to the glass, while the rest remains disseminated throughout the glass in globules of copper and red streaks, which are probably the protoxide; whereas lead (for whose oxide glass has such a strong affinity) oxidates but a small portion, when mixed with it in the metallic state, the rest being found imbedded in globules throughout its mass. Tin, antimony, and bismuth, are more easily oxidized and dissolved than lead. Gold, when fused with glass, imparts to it a light greenish tinge, increasing in depth with the relative proportion of silica in the glass,—producing a deeper colour with the bisilicate than the silicate of potash, and still deeper when German glass (which contains a large proportion of silica) is employed; globules of gold are found (as in the analogous cases of lead and copper) disseminated throughout the mass. If the heat be increased, and the crucible containing the gold be left for some hours in the furnace, the glass assumes a pinkish hue, which is the colour imparted to it by the protoxide of gold. When platinum sponge is fused with glass, it sinks to the bottom of the crucible unaltered, owing to its infusibility. When charcoal is heated with glass, a large proportion is oxidated, the remainder presenting the appearance of a mechanical mixture.

"From these experiments it appears, that glass, at high temperatures, not only has the property of oxidating the metals, and forming a chemical compound with the oxide, but moreover, when the chemical affinity is satisfied, of dissolving the oxides, and probably the metals themselves when in a state of fusion; the latter, on the cooling of the glass, being deposited in globules throughout its interstices, (at least the appearance presented by the glass seems to favour such an opinion.)

"The colours produced by the fusion of metals with glass, being different in many cases from those obtained when their oxides were employed, and presenting the dull untransparent appearance which is so remarkable in ancient glass, led me to suppose that the ancients did not employ any colouring matter unknown at the present day, but that, being unacquainted with the mineral acids, they employed the metals either in the metallic state, in filings, or else in an imperfect state of oxidation. To determine the probability of this conjecture, I selected three specimens of mosaic glass, analysed by Klaproth; and substituting for the oxides, in the same relative proportion, the metals in a minute state of division, I obtained coloured glasses of nearly the same colour as the mosaics, while the colours produced when the oxides were employed were not only perfectly different, but the glasses were clear and transparent.

"One of a lively copper red, opake and very bright, contained, in 200 grains, silica 142, oxide lead 28, copper 15, iron 2, alumina 5, lime 3.

"Another, of a light verdigris green, contained, in 200 grains, silica 130, oxide copper 20, lead 15, iron 7, lime 13, alumina 11.

"A specimen of blue glass contained, in 200 grains, silica 163, oxide iron 19, oxide copper 1, alumina 3, lime ½."

December 9, 1839.—Mr. Clarke read a supplement to his paper "on Atmospheric Electricity."

The author gave in this supplement a more detailed description than he had before done of the mode of insulating the apparatus for experiments on atmospheric electricity, which he had used in the course of his recent researches.

He then described an experiment by which he had shown the absence of decomposing agency in the electricity of serene weather, and stated his opinion of the cause.

Mr. Clarke next directed attention to the fact, that the curve representing the diurnal variation of the barometric column was the reverse of the electric, thermometric, and hygrometric curves. He considered that such a result was to be expected; for the barometric column should naturally be lower from midday to 3 P. M. than at midnight, in consequence of the greater quantity of aqueous vapour which exists in the atmosphere at the former than at the latter time,—air charged with aqueous vapour being known to be of less specific gravity than dry air. Thus the barometric and hygrometric curves would be the inverse of each other, the maxima of the one corresponding to the minima of the other; and as the author had previously shown that the hygrometric, thermometric, and electrometric curves were in accordance, the barometric curve would be the inverse of the thermometric and electrometric curves also. The author remarked, that if this character of the horary oscillations of the barometer in Ireland be confirmed by the experiments of other observers, it will either lead to new views of this phænomenon generally, or show that the quantity of aqueous vapour existing in Ireland is so great as to cause the horary barometric oscillations to present themselves in a different form from that in which they are recognised in drier climates.

The author adverted, in the last place, to the hypothesis of Priestley and Beccaria,—that the upper regions of our atmosphere were the chief depositories of the electric fluid,—an opinion which he conceived must fall, if the origin of atmospheric electricity be due (as his experiments prove) to the existence of vapour; as these elevated parts of our atmosphere are far above the region of permanent vapour, or even of vapour at all.

Professor MacCullagh read a paper "on the Dynamical Theory of crystalline Reflexion and Refraction."

In a former paper, presented to the Academy in January, 1837, and printed in volume xviii. of the Transactions, the author had reduced all the complicated phænomena of reflexion and refraction at the surfaces of crystals to the utmost regularity and order, by means of a simple rule, comprised in his theorem of the *polar plane*. This rule, which was verified by its agreement with exact experiments, he had deduced from a set of hypotheses relative to the vibrations of light in their passage through a given medium, and out of one medium into another; but he had not attempted to account for his hypotheses, nor to connect them together by any known principles of mechanics; and the only evidence in favour of their truth, was the truth of the results to which they led. He had observed, however, that these hypotheses were not independent of each other; he had ascertained that the laws of reflexion at the surface of a crystal were connected with the laws of propagation in its interior; and he had thence been led to conclude that all these laws and hypotheses "had a common source in other and more intimate laws not yet discovered." He became impressed, in short, with the idea, "that the next step in physical optics would lead to those higher and more elementary principles by which the laws of reflexion and the laws of propagation are linked together as parts of the same system."

This step the author has now made; and the present paper realizes the anticipations scattered through the former. Setting out with the general dynamical theorem expressed by the equation

$$\int\int\int dx\,dy\,dz\left(\frac{d^2\xi}{dt^2}\delta\xi+\frac{d^2\eta}{dt^2}\delta\eta+\frac{d^2\zeta}{dt^2}\delta\zeta\right)=\int\int\int dx\,dy\,dz\,\delta v, \quad (1).$$

where ξ, η, ζ, are the displacements at the time t of a particle whose co-ordinates are x, y, z, and where the density of the æther is supposed to be unity, as being constant for all media, the author determines the form of the function v, for the particular case of luminiferous vibrations, by means of the property which may be regarded as distinguishing them from all others—namely, that they take place entirely in the surface of the wave. From this property he shows, in the first place, that v is a function of the three differences

$$\frac{d\eta}{dz}-\frac{d\zeta}{dy},\ \frac{d\zeta}{dx}-\frac{d\xi}{dz},\ \frac{d\xi}{dy}-\frac{d\eta}{dx};$$

and, in the next place, that the only part of it which comes into play is of the second order, containing the squares and products of those quantities, with of course six constant coefficients. Then, supposing the axes of coordinates to be changed, he proves that the usual formulæ for the transformation of coordinates apply also to the transformation of those differences; so that, by assuming the new axes properly, the terms in the function v which depend on the products of the differences may be made to vanish, and v will then contain only the three squares, each multiplied by a constant coefficient. The axes of coordinates in this position are defined to be the principal axes, (commonly called the axes of elasticity); and when we put, with reference to these axes,

$$-2\mathrm{v}=a^2\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)^2+b^2\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)^2+c^2\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)^2, \quad (2)$$

it turns out that a, b, c, are the three principal velocities of propagation within the crystal.

To find the laws of propagation in a continuous medium of indefinite extent, we have only to take the variation of v from the expression (2), and, after substituting it in the right-hand member of equation (1), to integrate by parts, so as to get rid of the differential coefficients of the variations $\delta\xi, \delta\eta, \delta\zeta$. Then equating the quantities by which these variations are respectively multiplied in the triple integrals on each side of the equation, we obtain the value of the force acting on each particle in directions parallel to the principal axes. The double integrals which remain on the right-hand side of the equation are to be neglected, as they belong to the limits which are infinitely distant. The resolved values of the force thus obtained lead to the precise laws of double refraction which were discovered by Fresnel, with this difference only, that the vibrations come out to be *parallel* to the plane of polarization, whereas he supposed them to be *perpendicular* to it.

When there are two contiguous media, and the light passes out of one into the other, suppose out of an ordinary into an extraordinary one, and we wish to determine the laws of the reflected and refracted vibrations, it is only necessary to attend to the double integrals in the equation of limits; but the integrations must now be performed with respect to other coordinates. Taking the separating surface of the two media for the new plane of xy, the axis of x being in the plane of incidence, let the principal axis x of the crystal make with these new axes the angles α, β, γ, while the principal axes y and z, in like manner, make with them the angles α', β', γ', and $\alpha'', \beta'', \gamma''$, respectively. Then, marking with accents the quantities relative to the new coordinates, we have

$$\left.\begin{aligned}
\frac{d\eta}{dz}-\frac{d\zeta}{dy}&=\left(\frac{d\eta'}{dz'}-\frac{d\zeta'}{dy'}\right)\cos\alpha+\left(\frac{d\zeta'}{dx'}-\frac{d\xi'}{dz'}\right)\cos\beta\\
&\qquad+\left(\frac{d\xi'}{dy'}-\frac{d\eta'}{dx'}\right)\cos\gamma,\\
\frac{d\zeta}{dx}-\frac{d\xi}{dz}&=\left(\frac{d\eta'}{dz'}-\frac{d\zeta'}{dy'}\right)\cos\alpha'+\left(\frac{d\zeta'}{dx'}-\frac{d\xi'}{dz'}\right)\cos\beta'\\
&\qquad+\left(\frac{d\xi'}{dy'}-\frac{d\eta'}{dx'}\right)\cos\gamma',\\
\frac{d\xi}{dy}-\frac{d\eta}{dx}&=\left(\frac{d\eta'}{dz'}-\frac{d\zeta'}{dy'}\right)\cos\alpha''+\left(\frac{d\zeta'}{dx'}+\frac{d\xi'}{dz'}\right)\cos\beta''\\
&\qquad+\left(\frac{d\xi'}{dy'}-\frac{d\eta'}{dx'}\right)\cos\gamma''.
\end{aligned}\right\}\quad(3)$$

Now if we take the variations of these expressions, and substitute them in the value of $\delta\,\mathrm{V}$ derived from equation (2), then multiply by $dx'\,dy'\,dz'$, and integrate between the limits $z'=0$ and $z'=\infty$, neglecting to take account of the latter limit, as well as of the integrations with respect to x' and y', of which both the limits are infinite, we shall get, in the equation which holds at the separating surface, a term of the form

$$\int\!\!\int dx'\,dy'\,(\mathrm{Q}\,\delta\,\xi'-\mathrm{P}\,\delta\,\eta'),\qquad(4)$$

where

$$\left.\begin{aligned}
\mathrm{P}&=a^2\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\cos\alpha+b^2\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)\cos\alpha'\\
&\qquad+c^2\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)\cos\alpha'',\\
\mathrm{Q}&=a^2\left(\frac{d\eta}{dz}-\frac{d\zeta}{dy}\right)\cos\beta+b^2\left(\frac{d\zeta}{dx}-\frac{d\xi}{dz}\right)\cos\beta'\\
&\qquad+c^2\left(\frac{d\xi}{dy}-\frac{d\eta}{dx}\right)\cos\beta''.
\end{aligned}\right\}\quad(5)$$

This term, along with a similar but simpler one arising from the ordinary medium, must be equal to zero; and as the variations $\delta\,\xi'$ and $\delta\,\eta'$ are independent, this condition is equivalent to two. Moreover, the quantities ξ' and η' are to be put equal to the corresponding quantities in the other medium, and thus we have two more conditions, which are all that are necessary for the solution of the problem.

The four conditions may be stated by saying, that each of the quantities P, Q, ξ', η', retains its value in passing out of one medium into another. Hence it is easy to show that the *vis viva* is preserved, and that ζ' likewise retains its value. These two consequences were used as hypotheses by the author in his former paper, and accordingly all the conclusions which he has drawn in that paper will follow from the present theory also.

It will be perceived that this theory employs the general processes of analytical mechanics, as delivered by Lagrange. The first attempt to treat the subject of reflection and refraction in this manner was made by Mr. Green, in a very remarkable paper, printed in the Cambridge Transactions, vol. vii. part 1. After stating the dynamical principle expressed by equation (1), (though with a different hypothesis respecting the density of the æther,) Mr. Green observes, that, supposing the function v to be known, "we can immediately apply the general method given in the *Mécanique Analytique*, and which appears to be more especially applicable to problems that relate to the motions of systems composed of an immense number of particles mutually acting upon each other." Such is certainly the great advantage of starting with that general principle; but the chief difficulty attending it, namely, the determination of the function v, on which the success of the investigation essentially depends, has not been surmounted by Mr. Green, who has consequently been led to very erroneous results, even in the simple case of *uncrystallized* media, to which his researches are exclusively confined. In this case Mr. MacCullagh's theory confirms the well-known formulæ of Fresnel, one of which Mr. Green conceives to be inaccurate, and proposes to replace by a result of his own, which, however, will not bear to be tested numerically. The present theory applies with equal facility to all media, whether crystallized or not, and is distinguished throughout by the singular elegance and simplicity of its analytical details; a circumstance which the author regards as a strong indication of its truth.

A paper was read by Mr. J. Huband Smith, descriptive of certain porcelain seals, amounting to upwards of a dozen, found in Ireland within the last six or seven years, and in places very distant from each other.

He exhibited to the Academy one of these seals, with impressions of several others in sealing-wax. He stated that they were all uniform, consisting of an exact cube, having by way of handle, some animal (probably an ape) seated upon it; and that they were so precisely similar in size and general appearance as to be undistinguishable, except by the characters on the under surface. Little is known respecting these seals beyond the mere fact of their having being found in this country.

An extract from the Chinese grammar of Abel-Rémusat showed that the inscriptions on these seals are those of a very ancient class of Chinese characters, "in use since the time of Confucius," who is supposed to have flourished "in the middle of the sixth century, before J. C." The remote period to which these characters are assigned, leaves open a wide field for conjecture as to the time in which these porcelain seals found their way into this country.

The situations in which some of them have been found are remarkable. One was discovered in ploughing a field near Burrisokane, county of Tipperary, in 1832; another was found last year at Killead, in the county of Down; another in the bed of the river

Boyne, near Clonard, in the county of Meath, in raising gravel; and a fourth was discovered many years ago at a short distance from Dublin.

From the extreme degree of heat to which they appear to have been subjected, and the consequent vitrification which has in some measure taken place, they are quite as capable of resisting the attacks of time as the glass and porcelain deities and ornaments found in the mummy cases of Egypt, and may have lain for an indefinite period beneath the surface of the earth. It is therefore, at least, possible that they may have arrived hither from the East, along with the weapons, ornaments, and other articles of commerce, which were brought to these islands by the ships of the great merchant-princes of antiquity, the Phœnicians, to whom our ports and harbours were well known.

Mr. Smith then called the attention of the Academy to the remarkable discovery, by Rosellini, Lord Prudhoe, and other recent travellers, of unquestionable Chinese vases in the tombs of Egypt. He read a passage from Davis's China, in which some of them were described; and also an extract from Wilkinson's Ancient Egyptians, from which it appeared that the number of Chinese vases found at Coptos, Thebes, and elsewhere, amounted to seven or eight, and that the inscriptions on them had been translated by Chinese scholars to mean, "The flower opens, and, lo! another year," being a line from an ancient Chinese poem.

From this the trade of China with distant countries, at a period of the remotest antiquity, being clearly proved, Mr. Smith submitted to the Academy that a case of strong probability had been made out, that the porcelain seals found their way into Ireland at some very distant period. In fact, if they be not of modern introduction into this country—a supposition which the situations in which several of them have been found seems utterly to preclude—their arrival here must of necessity have been most ancient.

January 13, 1840.—Sir Wm. R. Hamilton, LL.D., President, in the chair. Professor Mac Cullagh made a communication respecting the optical Laws of Rock-crystal (Quartz).

In a paper read to the Academy in February 1836, and published in the Transactions, (vol. xvii. p. 461), he had shown how the peculiar properties of that crystal might be explained, by adding, to the usual equations of vibratory motion, certain terms depending on differential coefficients of the third order, and containing only one new constant c. This hypothesis, which was very simple in itself, not only involved as consequences all the laws that were previously known, but led to the discovery of a new one—the law, namely, by which the ellipticity of the vibrations depends on the direction of the ray within the crystal. He was not able, however, to account for his hypothesis, nor has it since been accounted for by any one.

But the theory developed in the paper which he read at the last meeting of the Academy, now enables him to assign, with a high degree of probability, the origin of the additional terms above-mentioned, and, if not to account for them mechanically, at least to advance a step higher in the inquiry. In that theory it was supposed, (and the supposition holds good in all known crystals, except quartz,) that the molecules of the æther vibrate in right lines, the displacements remaining always parallel to each other as the wave is propagated; and it was shown that the function v, by which the motion is determined, then depends only on the *relative displacements* of the molecules. But when this is not the case,—when, as in quartz, each molecule is supposed to vibrate in a curve —then it is natural to conceive that the function v may depend, not only on the relative displacements, but also on the *relative areas* which each molecule describes about every other more or less advanced in its vibration. This idea, analytically expressed, introduces a new term v into the value of the function 2 v; and, if the plane of the wave be taken for the plane of xy, it is easy to show that

$$v = \mathrm{c}\left(\frac{d\eta}{dz}\frac{d^2\xi}{dz^2} - \frac{d\xi}{dz}\frac{d^2\eta}{dz^2}\right).$$

Now if we integrate by parts the expression

$$\int\!\!\int\!\!\int dx\,dy\,dz\,\delta v,$$

so as to get rid of the variations of differential coefficients, the reduced form of the triple integral will be

$$2\mathrm{c}\int\!\!\int\!\!\int dx\,dy\,dz\left(\frac{d^3\eta}{dz^3}\delta\xi - \frac{d^3\xi}{dz^3}\delta\eta\right);$$

from which it appears that the quantities

$$\mathrm{c}\frac{d^3\eta}{dz^3}, \quad -\mathrm{c}\frac{d^3\xi}{dz^3},$$

are to be added to the usual expressions for the force in the directions of x and y respectively. These are the very terms in the addition of which the hypothesis before alluded to consists.

The Secretary read a paper by James Orchard Halliwell, Esq., F.R.S., &c., entitled "an Inquiry into the Period of the first Use of the Zero by those Writers who adopted the Notation of the Boetian numerical Contractions*."

The author referred, at the commencement of this communication, to the opinion which he had formerly expressed on the nature of the change from the use of the abacus, to that of local position, and the cipher. This opinion is contained in the following extract:—"It would be impossible, with the few materials yet brought to light, to conjecture with any great probability, how far these Boetian contractions may have influenced the introduction, or cooperated with the Arabic system, to the formation of our present numerical notation. It appears to me highly probable that the two systems became united; because the middle age forms of the figure

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* Papers by Mr. Halliwell on subjects immediately connected with the above, will be found in the last and present vols. of Lond. and Edin. Phil. Mag.

five coincide with the Boetian mark for the same numeral, and those of two others are very similar. The idea of local position, again, may have had an independent European origin; the inconveniences of the abacus on paper would have suggested it by destroying the distinguishing boundaries, and inventing an arbitrary hieroglyphic for the representation of an empty square."

The author then proceeded to adduce evidence from some documents recently discovered in support of these views. He showed from the Mentz MS. in the Arundel collection, in what manner the mode of operation with the abacus had been improved, so as to lead naturally to the present system. He then brought forward some passages from MSS. illustrative of the first employment of the zero; and concluded by adducing an instance from a MS. of the translation of Euclid by Athelard, of the fourteenth century, belonging to the Arundel collection, in which the number 15 is written in these contractions, and without a division.

XLIII. *Intelligence and Miscellaneous Articles.*

PRECIPITATION OF IRON BY ZINC.

M. CAPITAINE states that when a plate of zinc is immersed in a neutral solution of protochloride of iron, the zinc in a short time, especially if heated to ebullition, obeys the magnet and becomes brittle, and on continuing the immersion there remains only a friable fragment of pure iron. Nevertheless, as it may be suspected that some zinc may remain unacted upon, he has invented a very simple arrangement to avoid this inconvenience. It consists in immersing into the solution of iron a plate of copper perfectly cleaned and soldered at one end to a piece of zinc. It is very nearly the same apparatus as employed to obtain the lead tree, and it acts unquestionably in the same manner. The iron is deposited on the copper in a thin friable layer, having a metallic splendour, but without any appearance of crystallization: this mode of operating has no other inconvenience than its slowness; but in whatever manner it is conducted, there is always a disengagement of hydrogen, which continues as long as the metallic precipitation.—*Journal de Chimie Med.* January 1840.

ACTION OF CHLORINE ON THE CARBURETTED HYDROGEN OF ACETATES.

M. Dumas has read a notice to the Academy, of which the following is an abstract. Acetic acid treated with chlorine yields chloroacetic acid, and this under the influence of the alkalies is converted into carbonic acid and chloroforme. If there exist, as I have announced, a similarity of type between acetic and chloroacetic acid, the first ought to give with the alkalies a carburetted hydrogen $C^4 H^8$, corresponding to chloroforme $C^4 H^2 Cl^6$. The production of this carburet under the influence of the alkalies is not known; but of the carburet $C^4 H^8$ produced by the acetates corresponding to chloroform $C^4H^2Cl^6$, it ought to give rise by means of chlorine to the following series :

$C^4H^6Cl^2$ hydrochlorate of methylen
$C^4H^4Cl^4$ chloride of hydrochlorate of methylen
$C^4H^2Cl^6$ chloroforme
$C^4\quad Cl^8$ chloride of carbon.

"I have made many attempts," says M. Dumas, "to ascertain the production of these various bodies;" and he adds, "according to the details of the experiments, the gas of the acetates acts under the influence of chlorine as the law of substitutions and the theory of types had previously indicated, for the body C^4H^8 is converted into C^4Cl^8 ; it being well understood that this conclusion relates only to the gas of the acetates, no experiments having been made with pond gas; and he has but little experimented with the gas from alcohol, which may be merely a mixture. M. Dumas maintains purely and simply his preceding conclusions : acetic acid and chloroacetic acid belong to the same type, and the same exists with respect to chloroforme and the carburetted hydrogen of the acetates; for acetic acid produces the carburetted gas, under the circumstances in which chloroacetic acid yields chloroforme; and the chloroforme, as well as the carburetted gas of the acetates, is converted by the action of chlorine into a chloride of carbon C^4Cl^8, which belongs to the same type as they.—*L'Institut*, No. 318.

HYDROCARBURET OF BROMINE.

M. Pelouze and M. Millon by subjecting to the action of bromine the carburetted hydrogen obtained from the decomposition of alcohol by barytes, hydrocarburet of bromine corresponding to the liquor of the Dutch chemists, M. Pelouze stated it as his opinion that this compound, which is perfectly identical with that obtained from olefiant gas, could not be explained by the law of substitutions. He adds, that in his opinion, this law, when it is well known, is only a particular case of the theory of chemical equivalents; and that he has undertaken with M. Millon, some experiments to support his opinion in this respect.—*L'Institut*, No. 318.

NATIVE SULPHATE OF MAGNESIA.

Indiana, one of the United States, contains a great number of grottos ; one of these, near the Ohio, is celebrated for the masses of Epsom salt which are found in it. The mountain in which it is placed is 400 feet high, and is formed of limestone. The summit is covered with cedars and oaks. The entrance to the grotto is halfway up the mountain ; it is from 12 to 15 feet wide, and 3 to 4 in height. The descent is easily made into a spacious chamber, about a quarter of a mile long; its height varies from 4 to 20 feet, and its width from 10 to 20. The roof is sometimes flat and sometimes arched. At the extremity of the grotto it bifurcates ; the bifurcation on the right side is short ; that on the left hand leads by some steps of stones to a stage of ten feet high, and is in a south-east direction. Here the roof begins to form a regular arch, the height of which from the floor varies from five to eight feet, the grotto being from six to twelve feet wide, to the part called the *crawling place*, a name which is given to it in consequence of travellers being obliged to crawl, in order to reach another large neighbouring chamber; from this to the spot, in which a pillar is found, for a mile and a quarter, there occurs an alternating succession of large and small chambers. Sometimes the way is flat, at other times enormous blocks of rock must be climbed, which have been detached from the roof, and then the pillar occurs in the form of a magnificent white column, which reflecting the sombre light of the torches, has a majestic and dazzling aspect. Visiters rarely proceed further than from 100 to 150 fathoms. The pillar or column is 15 feet in diameter and thirty high; it is regularly fasciculated from the summit to the base. Not far from it are several other pillars of the same form, but of smaller dimensions: it is composed of carbonate of lime.

The date of the discovery of this grotto is not known; it is known only that it was visited in 1807 by some persons who found in it a bed of salts from 6 to 9 feet thick on the bottom of the grotto, where they observed enormous blocks scattered over it, whilst the walls were covered with saline products. The sulphate of magnesia abounds throughout this grotto in different forms, and sometimes in masses of one pound to ten. The soil has a brilliant appearance on account of the numerous portions of this salt disseminated in it. This sulphate lines the walls at various distances; if it be removed it is reproduced in four or five weeks in needle-form crystals. The poorest earth which has been washed gave four pounds per bushel, and the richest from 20 to 25 pounds. The salt which next occurs in the greatest quantity is nitrate of lime, and afterwards nitrate of alumina, which yields as much nitrate of potash as the nitrate of lime. Sulphate of lime also occurs, with traces of sulphate of iron and of carbonate and nitrate of magnesia. The sulphate of magnesia is not pure, as will be readily conceived.—*Journal de Chimie Medical*, January 1840.

MANUFACTURE OF CHLORATE OF POTASH.

M. Pelouze has communicated a new and advantageous mode of preparing chlorate of potash. Hitherto carbonate of potash has always been decomposed by chlorine: M. Pelouze describes the inconveniences of this process, which he proposes to remedy by substituting soda for potash; by this chlorate of soda and common salt are obtained, and the chlorate of soda is converted by double decomposition in chlorate of potash by one of the cheap salts of potash which occur in commerce.

M. Pelouze also proposes to pass chlorine into milk of lime, by which chloride of lime is obtained, and this is then decomposed by chloride of potassium.—*L'Institut*, No. 318.

DIABETIC BLOOD AND URINE.

M. Muller observes that the opinion that diabetic blood contains sugar has been many times contradicted, because it has not always been procurable from it.

The following are the results of M. Muller's experiments on diabetic blood obtained by venæsection, and also of the urine :—

Twelve ounces of the blood gave :	Oz.	Dr.	Grs.
Chloride of sodium	0	0	24·5
——— of potassium	0	0	13
Sulphate of potash	0	0	9
Carbonate of potash	0	0	17
——— of lime	0	0	6·75
——— of magnesia	0	0	9
Phosphate of magnesia	0	0	10
Carbonate of soda	0	0	11
Phosphate of soda	0	0	0·5
——— of iron	0	0	22·25
Sugar	0	1	5
Albumen	1	3	27
Hæmatosin	1	5	24
Liquid fat	0	0	19
Crystallizable fat	0	0	33
Fibrin	0	0	26
Extractive matter	0	0	22·5
Carbonate of lime	0	0	7·5
Water	8	1	33
	12.		

Fifty ounces of the urine of the same patient contained :

	Oz.	Dr.	Grs.
Diabetic sugar	2	3	37
Urea	0	0	1½
Extract of a very disagreeable odour	0	5	40
Mucous matters	0	0	5
Gum	0	2	26
Albumen	0	0	7
Sulphate of potash	0	0	5
Chloride of sodium	0	0	13
——— of potassium	0	0	3·5
Phosphate of lime	0	0	6
Hydrochlorate of ammonia	0	0	8
Phosphate of soda	0	0	26·25
——— of magnesia	0	0	0·75
Silica	0	0	1
Oxide of iron	trace.		
Hippuric acid	trace.		
Water	46	3	0

Ibid.

SIR JOHN F. W. HERSCHEL'S NEW RESEARCHES ON THE SOLAR SPECTRUM AND IN PHOTOGRAPHY.

The following are some of the points of novelty which occur in a paper by Sir John Herschel, now in course of reading before the Royal Society:—

1. Detection of luminous rays, and a new prismatic colour beyond the extreme violet.
2. Discovery of a chemical spectrum *beyond the extreme red rays*.
3. Assumption, according to circumstances, of either an oxidizing or a de-oxidizing action by the chemical rays at either end of the spectrum.
4. Formation of photographic impressions of the spectrum, exhibiting the prismatic colours in imitation of the colours of those rays by which they are produced; and a variety of other tints.
5. Photographic effects produced by the simultaneous action of two rays differing in refrangibility, which neither of them, acting alone, are capable of producing at all.
6. Action of the spectrum on vegetable colours.
7. Discovery of a process of secret photographic painting, in which the image may be preserved *ad infinitum* in an invisible state, capable of being at any moment rendered visible.
8. Account of a process for fixing photographic pictures on glass plates.
9. Analysis of the absorbent action of various media on the chemical rays.
10. Account of a self-registering photometer for meteorological purposes.

METEOROLOGICAL OBSERVATIONS FOR JAN., 1840.

Chiswick.—Jan. 1. Overcast: fine. 2. Very fine. 3. Fine: slight rain. 4. Rain. 5. Cloudy and fine: frosty at night. 6. Frosty. 7. Clear and frosty: severe frost at night. 8. Severe frost. 9. Overcast: fine. 10. Overcast: frosty at night. 11. Sharp frost. 12. Frosty: fine. 13. Clear. 14. Hazy. 15. Drizzly. 16. Fine. 17. Foggy. 18. Frosty and foggy: rain. 19. Boisterous, with heavy rain. 20. Rain: fine: boisterous at night. 21. Very boisterous with rain. 22. Cloudy: clear at night. 23. Rain: windy at night. 24. Boisterous. 25. Overcast: rain: fine. 26. Stormy and wet. 27. Clear and cold. 28. Rain: boisterous. 29. Very fine. 30. Hazy. 31. Very fine.

The frost was, for a short time, very intense between the 7th and 8th, being 20° below freezing.

Boston.—Jan. 1. Cloudy. 2. Fine. 3, 4. Cloudy. 5. Fine. 6. Fine: little snow P.M. 7. Fine. 8, 9, 10. Cloudy. 11, 12, 13. Fine. 14, 15. Cloudy. 16. Fine. 17. Rain. 18. Cloudy. 19, 20. Cloudy: stormy with rain P.M. 21. Stormy: thunder and forked lightning with rain A.M. 22. Cloudy. 23. Rain. 24. Stormy: rain P.M. 25. Fine: snow A.M. 26. Rain: rain early A.M. 27. Fine. 28, 29. Rain. 30. Fine. 31. Cloudy: rain early A.M.

Applegarth Manse, Dumfries-shire.—Jan. 1. Fine morning: rain P.M. 2. Very wet A.M.: showery all day. 3. Quiet day with slight showers. 4. Fine day and fair: aurora borealis. 5. Clear day: hard frost. 6. Fine frosty day. 7. Dull and cloudy. 8. The same: thaw. 9. Frost again. 10. Still frosty but cloudy. 11. Wet and stormy. 12. The same all day. 13. Fair, but threatening rain. 14, 15, 16. Wet and boisterous, 17. Clear and tending to frost. 18. Rain again and wind. 19. Heavy rain A.M.: showery all day. 20. Frequent showers. 21. Wind very high. 22, 23, 24. Boisterous weather. 25. The same: slight showers. 26. Moderate but showery. 27. Succession of snow showers. 28. Frost A.M.: snow: thaw P.M. 29. Frost A.M.: fine winter day. 30. Frost early A.M.: change P.M. 31. Slight showers A.M.: fine day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Jan.	Barometer.						Thermometer.									Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·724	29·944	29·699	29·30	29·46	29·35	49·8	50·5	44·3	55	46	51	44	39	s.	s.	calm	E by S.	...	·02	...	...	44
2.	29·738	29·941	29·757	29·21	29·25	29·41	47·6	48·0	47·6	52	39	47	47	42½	sw.	sw.	sw:	sw.	...	·06	...	...	43
3.	30·000	29·999	29·924	29·47	29·73	29·95	43·2	43·6	41·8	46	40	43	45½	42	w.	sw.	calm	w—N.	·025	·17	...	...	40
● 4.	29·956	29·975	29·908	29·60	29·90	29·89	39·7	40·6	40·6	44	32	38	39	32	NE.	NE.	calm	NE.	·072	...	...	...	40
5.	29·896	29·968	29·902	29·52	29·84	29·87	36·3	36·6	36·7	40	24	30	35½	27½	N.	N.	calm	N.	·205	...	...	...	37
6.	30·000	30·253	30·000	29·60	29·99	30·18	33·6	34·0	31·5	38	19	31	34	27½	NW.	NE.	calm	NE.	...	...	...	0·60	32
7.	30·282	30·313	30·259	29·92	30·10	29·98	29·0	32·0	29·0	32	12	27	32	19½	ENE.	E.	calm	NE.	...	...	...	...	28
8.	30·156	30·219	30·182	29·73	29·93	30·03	22·8	29·4	22·3	32	26	31	38	29	sw.	sw.	calm	NW.	...	...	...	...	17
9.	30·224	30·389	30·264	29·80	30·09	30·21	33·2	32·8	22·3	37	29	35	35	27	sw.	w.	calm	NE.	...	...	...	...	28
10.	30·476	30·588	30·511	30·20	30·26	30·25	34·2	34·8	32·6	35	15	33·5	34½	25	E.	SE.	calm	wsw.	...	...	...	...	26
11.	30·516	30·557	30·413	30·13	30·14	30·07	27·4	37·2	26·0	37	17	26	41½	34	SE.	s.	calm	SSE.	...	...	...	...	27
☽ 12.	30·286	30·352	30·288	29·94	29·85	29·87	31·5	35·3	27·3	42	22	27	44	38	SE.	s.	w.	s.	...	...	...	...	27
13.	30·144	30·192	30·003	29·83	29·71	29·47	34·9	39·8	31·4	40	29	31	45	42	SE.	s.	w.	SE.	...	...	...	...	28
14.	29·924	30·078	29·944	29·55	29·54	29·68	40·9	41·5	34·8	46	41	37	46	41½	s.	s.	SE.	SE.	...	·01	...	0·20	33
15.	30·142	30·131	29·879	29·68	29·70	29·36	43·2	44·7	40·2	46	38	43	46½	43	ssw.	sw.	SE.	s.	...	·06	...	...	39
16.	29·718	29·797	29·760	29·23	29·30	29·39	40·7	46·8	40·8	43	36	40	46½	40	sw.	w.	w.	w.	·044	·15	...	...	39
17.	29·490	29·858	29·518	29·18	29·42	29·71	41·5	46·6	40·8	43	24	39	42	35	s.	NE.	calm	NNE.	·127	·02	...	...	40
18.	29·972	29·990	29·726	29·54	29·43	29·28	36·3	43·8	34·2	50	40	33·5	49½	28	s.	sw.	calm	sw.	·033	·14	·03	...	36
○ 19.	29·436	29·697	29·358	28·94	28·88	28·80	49·4	50·7	35·6	54	40	46	46½	41	s.	w.	calm	wsw.	·122	·37	...	...	44
20.	29·618	29·712	29·425	29·15	29·20	29·03	42·6	54·2	41·4	54	44	42·5	42½	36	s.	w.	w.	w by s.	·282	·20	·09	...	41
21.	29·312	29·726	29·325	28·73	28·73	29·05	51·3	53·7	41·4	52	43	51	44	35½	s.	w.	w.	ssw.	·361	·09	·11	2·40	45
22.	29·684	29·838	29·652	29·10	29·22	29·45	48·3	53·2	45·0	51	38	47	44	37½	s.	w.	w.	w.	·133	·04	·12	...	43
23.	29·808	29·841	29·622	29·25	29·10	29·05	47·7	51·4	41·2	54	49	41·5	49	35½	s. var.	sw.	w.	s.	·022	...	...	...	41
24.	29·346	29·366	29·010	28·73	28·78	28·41	52·0	53·5	47·4	53	37	55	49	40	s. var.	sw.	s.	ssw.	...	·49	...	...	47
25.	29·316	29·382	29·352	28·76	28·77	28·95	43·0	53·3	39·6	54	38	39	39	36	s.	w.	w.	sw.	·394	·05	·20	...	42
☾ 26.	28·938	29·311	28·742	28·43	28·44	28·62	50·5	52·7	40·6	52	34	48	44½	35½	s. var.	w.	w.	wsw.	...	·34	·08	...	45
27.	29·444	29·758	29·499	28·91	29·00	29·28	37·8	38·5	37·0	46	35	36	39½	29½	sw.	w.	w.	w.	·481	·02	·05	...	38
28.	29·422	29·427	29·182	29·05	29·05	28·85	42·8	43·4	37·0	54	42	36	37½	32	s.	w.	calm	sw.	·016	·17	...	1·41	40
29.	29·500	29·830	29·562	29·	29·35	29·54	44·8	45·5	42·6	47	26	37	39½	33	w.	NW.	N.	wsw.	·272	...	·36	...	41
30.	29·938	29·962	29·685	29·53	29·30	29·45	34·4	35·0	34·6	45	30	34	39	27	ssw.	E.	calm	SE.	...	·02	·02	...	36
31.	29·574	29·584	29·539	29·15	29·20	29·30	43·4	44·3	34·8	50	29	41	42	33½	s.	s.	s.	SSE.	·044	·06	·19	...	40
Mean.	29·806	29·931	29·732	29·36	29·440	29·474	40·4	43·5	36·9	45·93	32·64	38·6	42·	34·3					Sum. 2·633	2·48	1·25	4·61	Mean. 37·0

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XLIV. *Letter from* M. Kreil, *Director of the Observatory at Milan, to* M. Kupffer, *Director General of the Physical Observatories in Russia, containing a succinct Account of the principal Results of* M. Kreil's *Magnetic Observations at Milan.** *Communicated by* Major Sabine, *R.A.*

THE observations arrange themselves under three heads: i.e. absolute observations, observations of the periodical changes, and of the perturbations of the magnetic forces. The absolute observations have shown that a correction must be applied to the declinations previously published, which had been determined within the confines of the Palace of Brera, where the Astronomical Observatory is placed, and where masses of iron appear to have exercised a disturbing influence. A series of observations made last spring in an open meadow 640 metres from the observatory, showed that the previous determinations were 23′ 16″ in excess. A second series made in the botanical garden adjoining the observatory, on a spot which is 47 metres from the palace, but which was not available before, gave an error of 21′ 51″. I have taken the mean of these determinations, i.e. 22′ 33″·5, as the quantity by which all the declinations hitherto published are to be diminished.

The apparatus employed in these observations was made in Göttingen by M. Meyerstein, and is furnished with two needles, similar in form and weight, but of unequal magnetism. Under circumstances precisely similar, one of them (No. 4.) makes a vibration in 25″·2; the other (No. XVII.) in 29″·6.

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* From the *Bulletin Scientifique publié par l'Acad. Imp. des Sciences de St. Pétersbourg*, vol. v. No. 21.

If these two needles are employed for determining the declination precisely in the same manner as described in p. 146 of the 1° *Supplemento alle Ephemeridi di Milano*, a constant difference of nearly 8 minutes is found between their results, the weaker needle (No. XVII.) giving the greatest declination. With needles of still weaker magnetism, some of which have been made here, this difference amounts even to half a degree, not only when the observations are made within the influence of the iron of Brera, but also in the open meadow. All the observations of declination hitherto published have been made with the stronger needle (No. 4.). I have not been able to discover any reason for this phænomenon, unless it is caused by temporary magnetism. However it showed itself in the first year too plainly to be doubted; and I should have mentioned it before, if I had not been desirous of obtaining the greatest certainty which repeated observations could give, and of waiting to see whether the same circumstance might not have been remarked by other observers. In respect to the horizontal intensity I have not as yet been able to perceive any difference between the determinations made with these two needles.

The observations for the periodical changes were made six times a day; they included in the first year observations of declination and of the time of vibration of the horizontal needle; in the second year the inclination was observed in addition to the above phænomena; and in the third year the time of oscillation of the dipping-needle was also included. The result has shown that no clear view of these complicated phænomena can be obtained unless they are *all* observed. The results derived from our observations are the following:

1. In Milan, and at the present epoch, the horizontal portion of the magnetic force reaches its least daily intensity between 8 and 10. 30 a.m., it then immediately begins to increase rapidly and attains its greatest intensity between 4. 30 and 7. 30 p.m., after which it decreases. An irregularity shows itself in the increase of the force, which is still rapid between 1 and 2 o'clock, becomes almost imperceptible between 2 and 4, is again more rapid between 4 and 6, and is then changed into a decrease; we shall see presently the explanation of this.

2. These epochs of the maximum and minimum of the horizontal intensity, which as they are obtained from all the observations may be termed mean epochs, are not constant. In the summer months the minimum is earlier and the maximum is later than in the winter.

3. The difference between the maximum and minimum is

greatest near the time of the summer solstice and least in December.

4. The intensity of the horizontal force increases from January to June, and decreases from July to December.

5. The declination begins to increase at 8 in the morning, and increases rapidly until 1 or 2 in the afternoon, when it attains its highest amount; it then decreases more slowly until it reaches its lowest value: in the winter months the declination is usually less at 11 in the evening than at 8 in the morning, but only occasionally so at other seasons of the year.

6. The mean value of the difference between the greatest and the least declination is 12′ 2″. The difference is greatest in the month which follows the vernal equinox, and least in December.

7. This diversity is the consequence of the annual change in the declination, which at different hours in the day follows an opposite course; in the forenoon, it decreases in spring and increases in autumn; in the afternoon, it increases in spring and decreases in autumn; hence it follows that there must be some hour in the day which is free from the annual change in the declination and which is therefore most suitable for insulated experiments. At Milan and at the present epoch, this hour is between 10 and 11 a.m.

8. The total force attains its least intensity at 8 in the morning or earlier, and its greatest intensity between 1 and 4 p.m. We are still in want of a sufficiently extended series of observations to furnish with equal certainty other facts relating to this element.

9. The inclination increases in the morning till towards 10 o'clock, when it decreases, but not uninterruptedly, for it increases again in the afternoon, and at 4 attains a second maximum, after which it decreases without interruption till near midnight. This alteration in the inclination retards the time of the maximum and minimum of the horizontal force by two or three hours; and its second maximum explains the anomaly mentioned in No. 1.

10. The amount of the alteration of the inclination seems to be also dependent on the season of the year. A greater alteration (above a minute) was observed in summer, and a less (about half a minute) in winter.

11. The times of the absolute maximum and minimum are very variable, but even in this variability a law is plainly manifest. In January and February the maximum was observed at 4. 30′ p.m., in March and April at 10. 30′ a.m., from May to August at 8 a.m., in September and October at 10. 30′ a.m., in November at 1 p.m., and in December at 4. 30′ p.m.

The minimum of inclination was at 8 a.m. in the winter months (i.e. November to March) and at 11 p.m. in the remaining months, with the exception of June and July, when it took place as early as 7. 30′ p.m.

These results are deduced from the general or monthly means of the different hours of observation. Another combination of the observations in daily means, i.e. the averages of all the observations taken on the same day, ought to show those alterations which have a period longer than a day and less than a year: a monthly period is thus shown; but as yet it is only in the horizontal elements that it can be recognised with certainty. The observations with the inclinatorium have not been brought into the calculation, because they were frequently interrupted, and because at first the axis of oscillation of the needle was too far removed from its centre of gravity.

12. If the daily means of the times of vibration of the horizontal needle reduced to the temperature of 0, are combined together in such series that the middle of each shall coincide with a phase of the moon, and if the means of these series are freed from the influence of the loss of magnetism of the needle by being reduced to the same epoch, we then see that the total means of all the times of vibration observed near the new moon, and during the first quarter, are less than those near the full moon and in the last quarter. If we compare the different months with each other, we see that the phænomenon, as it is here enounced, is only found in the eight months from November to June, and that in the four remaining months, i.e. July to October, the contrary takes place; for in the latter interval the longest times of vibration coincide with the new moon and the first quarter, and the shortest times of vibration with the two other phases.

13. This phænomenon might be thought to be an effect of the rotation of the sun round its own axis, which, supposing the sun to be magnetic, would cause sometimes one and sometimes the other of the poles of its magnetic axis to be turned towards the earth; and this hypothesis would also explain the alternations of the phænomena according to the different seasons of the year, as the earth is opposite to one or the other of the solar hemispheres according as she is in the summer or in the winter half of her orbit: but this will not hold good. The epoch of the least value of the intensity is open to the objection that the time of rotation of the sun is two days shorter than the time of the synodic moon, and this difference of time combined with the different positions of the earth relatively to the sun, would cause the phænomenon to

be nearly the same in the summer and in the winter months. We must therefore give up the idea of the effect being produced by the sun, and must seek its cause rather in the position of the moon's path, by which in winter the moon when new is but little raised above the horizon, whereas in summer when she is in this phase she approaches the zenith. If she has a sensible influence on the horizontal needle, it must be greatest when she is near the horizon, and thus the alternation of the phænomena at different parts of the year would be explained. If this be the true reason, it must show itself also when the observations are combined by another method, that is to say, when they are arranged according to the moon's declination. The daily means were therefore formed into series, one of which always comprehended all the observations of the same or of two successive months during which the moon's declination was south, and the other, all those during which the moon was north of the equator. These series, when freed from the gradual increase of the time of vibration caused by the diminishing magnetism of the needle, and collected into two general means, showed that the time of vibration (which is nearly 22′ 5″ mean time) is less by 0″·00168 when the moon has south declination than when she is north of the equator, which confirms the above-mentioned hypothesis.

14. As the influence of the moon differs so sensibly according to the difference of her position in the heavens, it seemed worth while to examine whether her greater or less distance from the earth might be indicated by our magnetic needles. The daily means were collected for this purpose into series, in such manner that the middle of one series should coincide with the moon's apogee, and the middle of the next with her perigee, then proceeding as before. The general means showed that the times of vibration at the time of the perigee were 0″·00198 less than at the time of the apogee, agreeing also with what has been said above.

15. If the intensity of the magnetic force is so sensibly subject to the influence of the moon, it is highly probable that the direction of our needles may also be altered by it, and it must be possible, by a suitable mode of combination of the observations, to recognise such an alteration. It is plain that the daily means are inapplicable for this purpose. The observations of each hour must be considered apart, and divided into series according to whether at the time of observation the moon was east or west of the magnetic meridian. It is true that in single months, the effect of this influence is obliterated by the annual alteration in the declination, which

as we have seen in (7.) is different at different hours of the day. But in a longer series of observations, on account of the periodicity of the last named alteration, the effect of a constant though much weaker cause acting according to quite a different law must be traceable, as the result has shown. The following are the differences at the different hours of the day between the declinations found with moon east and moon west.

Hours.	☾ East.— ☾ West.
20 0	+10″·8
22 30	+27 ·5
1 0	+ 9 ·1
4 30	+25 ·9
7 30	+ 8 ·3
11 0	+ 8 ·0
Mean	+14 ·9

It is true that these numbers are not yet corrected for the secular decrease of the declination, but it does not appear to have been great enough to affect the result essentially. In the year 1836 the observations were not made exactly at the same hours, and cannot therefore be employed for determining the secular alteration, but from 1837 to 1838 it was 59″·8, which would give a correction of 2″·5. This if doubled would not alter any of the signs in the above table. We conclude hence, that in this country the declination is always greater when the moon is east of the magnetic meridian, and less when it is west.

16. All the results which we have derived from our observations in reference to the effects produced by the moon agree in presenting her as a body exercising magnetic influence, and in which the prevailing magnetism on the hemisphere turned towards the earth attracts that pole of our needles which is turned towards the south, and increases the magnetic intensity of our hemisphere.

The observations of perturbations have manifested the following facts:

17. Great disturbances frequently occur on the same day, or nearly on the same day, in different years. Thus the greatest disturbances which took place in the year 1836 were on the 22nd and 23rd of April, and on the 18th of October, and both these were repeated on the same days in 1837. In 1838 several of the disturbances took place a few days later

than they had done in 1837, as is shown by the following comparison:

	Jan.	Feb.	Mar.	April.	June.	July.	Aug.	Sept.	Oct.	Dec.
1837.	25	13 18	22	6 22 27	2	2 28	25	14 16	18	14 19
1838.	28	16 21	17	11 29 30	4	12 34	23	14 15 16	17	20 28

These coincidences would show a new fact, namely, the periodical nature of these phænomena, if equally considerable disturbances had not occurred, (ex. gr. those of the 12th, 14th, and 15th November 1837, and of the 17th January 1838,) without any corresponding disturbances being traceable in other years. The subject requires to be further elucidated by continued observation.

Another point to which the attention of the observer should also be directed is, the symmetrical arrangement of these disturbances in the same year, many of them being nearly six months apart, for example:

In the year 1836 and
1837 the disturbances on the 22nd April and 18th Oct.
1838 — — 17th Jan. and 12th July.
do. — — 21st Feb. and 23rd August.
do. — — 29th April and 31st October.
do. — — 4th June and 5th December.

18. All hours of the day do not appear to be equally favourable to the development of this phænomenon, at least its first indications occur much more frequently in the evening than in the morning hours. If we take from the 105 disturbances marked in our day-book, those which were obviously continuations of perturbations which had begun on preceding days, as well as those which were first remarked during the intervals between the regular hours of observation, then the first indications of

19 were at $20^{h}\ 0'$
1 — at 22 30
9 — at 1 0
21 — at 4 30
16 — at 7 30
11 — at 11 0

Here it is to be remarked, that the observation hour, 11, precedes the longest interval namely, the night, and that the greater number of the more considerable disturbances extend over several hours or even days; therefore perturbations which may have begun in the early hours of the night ought to be taken from those which are ascribed to the first hour in the morning when the apparatus was observed. It follows that much fewer disturbances begin at that hour than in the evening. It is remarkable that a disturbance hardly ever begins in the latter hours of the forenoon.

19. The perturbations appear to commence for the most part suddenly, as by shocks. At least it was so in those cases in which the phænomenon began under our eyes, that is to say, at the time of an observation. It was so on the 18th of February 1837, on which day an aurora showed itself with a magnificence unusual in this country. Two series of transits of a division of the scale across the wire had been observed as usual. In the first series at 4^h 38^m these passages agreed to a tenth of a second of time;—a proof that the needle was still performing its minute vibrations with perfect regularity; nor in the preceding observations of the same day was any trace of disturbance to be discovered. In the second series, which was made 12 minutes later, there were differences of 2 seconds of time, and the needle was visibly drawn backwards and forwards by the disturbing forces. I may be permitted to mention one more among many similar cases. It is that of the very great disturbance of the 14th of November 1837, at which time we were making magnetic observations for three days uninterruptedly from 5 to 5 minutes, and when necessary at still shorter intervals, for the purpose of examining whether the periodical phænomenon of the falling stars was or was not connected with magnetism. Between 10 and 11 in the evening the needle appeared pretty tranquil, although earlier in the day it had been much disturbed. The observer, Sig. Della Vedova, was engaged in observing the passage of a division of the scale across the wires, and had chosen for that purpose a division nearly in the middle of the arc of vibration, which was then about $7'$, when all at once, at 11^h $7'$, he saw that this division did not come to the wire, although it had taken its direction towards it; but before reaching it, the needle had turned the opposite way, and immediately after the scale disappeared from the field of view, which was left quite dark. The observer, thinking that the lamp which illuminates the scale had gone out, was about to rise in order to light it again, when he saw the scale suddenly reappear, move rapidly across the field, and disappear on the opposite side. The rapidity of the movement indicated a much larger arc of vibration. The arc had in fact increased 40 minutes without any apparent cause. As the observations could not be exact whilst the vibrations were so great, Sig. D. Vedova was about to employ a magnetic bar which is always

at hand to be used in quieting the needle; but he was anticipated, for the arc of vibration suddenly diminished to less than a minute, so that the needle appeared quite stationary; at the same time the declination increased so rapidly, that its alteration in the course of a minute of time amounted to 6′ of arc, a quantity which at this season of the year is hardly traversed by the needle in the course of an entire day.

The influence of the disturbing forces usually affects all the elements at the same time, but it also happens not unfrequently, particularly in minor disturbances, that their effect only reaches the most sensitive of the three elements, i. e. the declination; and some cases, though rare ones, have occurred in which the time of vibration of the horizontal needle underwent considerable alteration without the declination being affected at the same time; an example of this occurred on the 15th of November 1837 between 6 and 10 a.m.

20. The greatest change of declination during a perturbation yet observed in Milan was on the 14th of November 1837. It amounted to 1° 11′, which is nearly ten times the mean daily alteration in this month: by reason of its great variability this element returns to its usual value sooner than the other elements. More examination is still required to manifest, whether the general tendency of the perturbations is to increase or diminish the average amount of declination, and whether their occurrence is connected with the hour of the day.

21. The time of vibration of the horizontal needle is always increased by a disturbance, i. e. the force is lessened; but even in this respect there are such fluctuations, especially soon after the beginning of a disturbance, that sometimes very small times of vibration occur, though this is only of very short continuance. The greatest change of this kind was observed on the 17th of January 1838, when it amounted to nearly 0″·3, the time of one vibration being 22″·3. In the greater disturbances it is sometimes the second or third day before the time of vibration returns to its previous value.

22. During a disturbance the inclination always seems to be greater, but it is subject to as great fluctuations as the other elements. The greatest alteration which we observed was on the 21st of February 1838, when it amounted to 8′ 45″; whereas the mean diurnal alteration in this month is only 1′ 6″·6. The dipping-needle usually returns to its ordinary direction on the following day, but sometimes not for a few days.

23. The disturbances likewise increase the times of vibration of the dipping-needle, showing that their influence ex-

tends also to the total force. The needle in our inclinatorium completes one vibration in nearly 14 seconds: during the disturbance of the 17th of January 1838, it underwent an alteration of $0''\cdot074$. In February of the same year the mean of the observed times of vibration for the month was $13''\cdot874$, but for the 21st day of the month it was 13·932. I need not remark how important it would be to examine such kindred phænomena as those of the atmosphere and auroras, with the same exactness which is now applied to magnetic investigation.

I must mention one other phænomenon which claimed our attention in a very high degree, and which perhaps may deserve that of other observers. I mean the vertical oscillations which show themselves so often in the dipping-needle, and which also appear to be connected with determinate laws. At least they are much more frequent in November and December than at other seasons, and they occur most commonly in wet weather. For this reason I do not think that they can be ascribed to a tremulous motion of the building, from which the part of it which contains the apparatus is quite free. Neither can they be attributed to the effect of currents of air, because they occur less frequently at the time of the equinoctial gales and other storms than at the above-named times. Perhaps they arise from very weak shocks of earthquake, which may be revealed to us by this highly sensitive apparatus, and which probably occur much more frequently than the more considerable shocks which are recognised by our senses, and by other effects. At least the greatest vertical oscillations of the needle have almost always coincided with considerable earthquakes, often having their seat in remote countries. One striking instance of this kind, after we had experienced other similar ones, occurred on the 23rd of January 1838. Between $7^h\ 33^m$ and $7^h\ 47^m$ p.m., Milan mean time, the needle began to oscillate so strongly that its arc of vibration appeared, by the vertical scale attached, to amount to 27 millimeters, or nearly 10 minutes of arc; there were no other indications of an earthquake of any kind. Twenty days afterwards the newspapers contained accounts of considerable damage caused by an earthquake on the same evening at Bucharest, Jassy, Odessa, and other places. According to these accounts the shock was felt at Jassy at $7^h\ 42^m$, and at Odessa at $7^h\ 45^m$ Milan mean time, agreeing with the beginning of the phænomenon observed by us.

Milan, Jan. 9, 1839.

XLV. *Observations on the supposed Formation of inorganic Elements during Fermentation.* *By* Mr. J. Denham Smith.*

IN the October Number of last year's Phil. Mag., [present series, vol. xv. p. 329] there appeared an abstract of various papers read at the meetings of the Royal Society, one of which, entitled "Additional Experiments on the Formation of Alkaline and Earthy Bodies by chemical action when carbonic acid is present, by Robert Rigg, Esq., F.R.S.", attracted my particular notice, from the novel and most extraordinary nature of the results announced in it.

Although the Royal Society has always carefully disclaimed any participation in, or support of, the theories and observations brought before or published by it, yet the circumstance of a paper being read to the first learned society of these kingdoms, and the author of that paper a Fellow of the Society, gives weight and sanction to the observations adduced. This circumstance was one of the chief reasons which led me to make the following experiments on the subject of this novel formation of inorganic elements by catalytic action.

I much regret that in the abstract of the paper which appeared in this Magazine, no details of any of the experiments are given; the substances used, the apparatus, and the results obtained, only being mentioned. It appears that "the author gives a detailed account of several experiments in which sugar, water, and yeast only were employed, and from which he deduces the conclusion that alkaline and earthy matters are formed by chemical action. In one set of experiments, some of which were made in silver, others in china, and others in glass apparatus, after the vinous fermentation had gone on during five days, the quantity of ashes obtained was, *in the silver apparatus eighteen, in the china nineteen, and in the glass fifteen times greater than the previous quantity*†. A further examination of these ashes showed that they consisted of potash, soda, lime, and a residue not acted upon by muriatic acid."

Thus having no data of the respective quantities of sugar, water, and yeast Mr. Rigg used, I may not have employed these substances in the proportions with which he experimented; if this should be the case, I presume, however, that this circumstance will not in any way tend to vitiate the results I have obtained; the question being whether inorganic matter is produced during vinous fermentation.

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* Communicated by the Author.

† These sentences are not printed in italics in the original.

To satisfy myself respecting the correctness of Mr. Rigg's statement that *the quantity of inorganic matter in a liquid is increased from fifteen to nineteen times when carbonic acid is present*, I dissolved 1500 grs. of the best refined sugar in 1½ pint of distilled water, and added 200 grains of risen beer yeast, then thoroughly mixed them by agitation. This solution was passed through fine cambric to separate any insoluble impurities which the solution contained, and divided into three exactly equal portions. Of these, two portions were respectively placed in German glass jars, and immediately covered with unglazed paper covers; the paper was of a close texture, and was carefully gummed down round the exterior of the jars, to prevent any inorganic matter, as dust, &c. getting into the solutions. These jars were placed in a warm situation in the laboratory, the temperature varying from 60° to 70° Fahrenheit, and the fermentation allowed to proceed. The third portion was then put into a flask and boiled, occasionally adding pure nitric acid; this acid left no stain when a portion was evaporated to dryness in a porcelain capsule; the flask was kept in an oblique position, to prevent any of the liquid being ejected by the action of the nitric acid. During the ebullition of the liquid, nitrous acid fumes were slowly formed and the solution assumed a primrose yellow colour; numerous spherules of liquid were formed, apparently on the surface of the boiling fluid, and coursed about hither and thither with great velocity, the larger spherules seemingly attracting the smaller; and when by this union the globule had attained about the size of a coriander seed, it disappeared, being again united to the bulk of the boiling solution. I imagine that this singular and interesting phænomenon is owing to small portions of the liquid being ejected from its bulk, by the rapid action of the nitric acid on the organic matter in the liquid; and that these particles on again approaching the surface of the fluid, there meet with a stratum of nitric oxide gas or a mixture of this gas and steam, which prevents their contact with the subjacent liquid, and upon which stratum of vapour they float, until by the increase of size, and consequently of weight, the buoyant power of this stratum of gas or vapour is insufficient to prevent their coming into contact with the mass of the liquid, and that they then reunite with it, disappearing instantaneously.

Oxalic acid was formed, and then decomposed by the continued action of the nitric acid; and the residue of the liquid, after evaporation to dryness in a platinum crucible, weighing 630·4 grains, was ignited to redness in a gas furnace, with the occasional addition of a few drops of nitric acid; an ash of a light buff colour remained, weighing, with the crucible, 631·97 grs. −630·4 = 1·57 gr. of inorganic matter contained in 500 grs. of sugar and 66·6 grs. of yeast before fermentation. On examination, this ash was found to consist of an alkaline carbonate, traces of a chloride and of a sulphate, phosphates of lime and magnesia in large proportions, and minute traces of silica and oxide of iron.

At the expiration of six days, one of the portions which had undergone the vinous fermentation, and which presented the agreeable odour accompanying this stage of fermentation, was evaporated in a mode, and with precautions, exactly similar to the above, and the same phænomena were observed during the operation. The residual liquid evaporated to dryness in the platinum crucible weighing 630·38 grs., and ignited over a gas lamp to full redness as in the first experiment, afforded an ash similar in appearance to the former, which with the crucible weighed 631·97 grs. −630·38 = 1·59 grs. of inorganic matter yielded by 500 grs. of sugar and 66·6 grs. of yeast, after undergoing the vinous fermentation. This ash was similarly constituted with that obtained in the first instance.

From these experiments we find that whilst 500 grs. of sugar and 66·6 grs. of yeast afford previous to fermentation 1·57 grain of inorganic matter; when fermented they give 1·59 gr., an increase of $\frac{1}{50}$ gr., or of about 1¼ per cent.; an increase so trifling that I do not hesitate to refer it to an error of experiment, and not to the formation of inorganic elements during vinous fermentation, which Mr. Rigg asserts is the case. I therefore conclude, contrary to the views entertained by Mr. Rigg on this subject, that *there is no formation of inorganic matter during the progress of vinous fermentation.*

I am at a loss to offer any feasible explanation of the enormous increase of inorganic matter observed by Mr. Rigg; the only mode by which this could have taken place, which at present occurs to me, and that an unlikely one, is that sufficient precautions were not taken to prevent the introduction of foreign matters by securely covering the solutions of sugar and yeast whilst fermenting; and that a quantity of dust, the constant plague of a laboratory, became mixed with his solutions, and thus led Mr. Rigg to suppose that the alkalies and earths were absolutely formed during fermentation. I may remark that the paper covers with which my fermenting solutions were protected from the dust, were so thickly covered with it, that had the precaution of covering the solutions not been taken, I must have obtained a very considerable increase in the weight of the ash after fermentation, although I do not imagine it would have been to the extent of fifteen to nineteen times the weight of the ash previous to it.

Duke Street, Liverpool, March 1840.

XLVI. *Contributions to Electricity and Magnetism. No. III. on Electro-magnetic Induction. By* JOSEPH HENRY, *LL.D., Prof. of Natural Philosophy in the College of New Jersey, Princeton.*

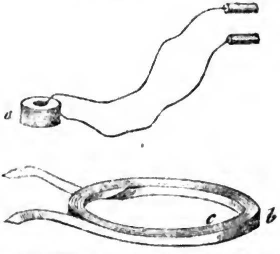
[Continued from p. 210.]

SECTION III.—*On the Induction of Secondary Currents at a distance.*

45. IN the experiments given in the two preceding sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of about two feet in diameter, and helix No. 4 placed as is shown in the figure. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect, however, was still greater when the helix was moved from the centre to the inner circumference, as at *c*: but when it was placed without the ring, in contact with the outer cir-

Fig. 4.



a represents helix No. 4, *b* coil No. 1, in the form of a ring.

cumference, at *b*, the shocks were very slight; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favourable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the further side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribands of coils No. 1, 2, 3 and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the riband conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connexion, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper riband, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole

course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the face.

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner, the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horseshoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

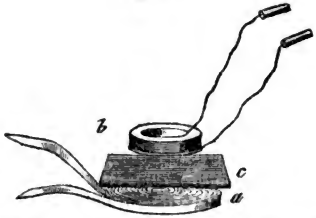
55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.—*On the Effects produced by interposing different Substances between the Conductors.*

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and nonconductors *. The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, fig. 5, and a plate of sheet iron, about $\frac{1}{10}$th of an inch thick, interposed. With this arrangemeut no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

Fig. 5.



a represents coil No. 1, *b* helix No. 1, and *c* an interposed plate of metal.

58. It was at first thought that this effect might be peculiar to the iron, on account of its temporary magnetism; but this idea was shown to be erroneous by substituting a plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c.: and it was found that all the perfect conductors, such as the metals, produced the screening influence; but nonconductors, as glass, wood, &c. appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{10}$th of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through

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* Philosophical Transactions, 1821. [or Phil. Mag., First Series, vol. lviiii.]

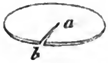
it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschel, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in fig. 6. With the plate in this condition, no screening was produced; the shocks were as intense as if the metal were not present.

63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

Fig. 6.

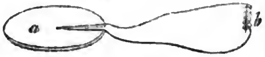


a represents a lead plate, of which the sector *b* is cut out.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in fig. 7. By this arrangement the latent current was drawn out, and its direction obtained by the polarity of a needle placed in the spiral at *b*.

65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

Fig. 7.



a represents a lead plate, *b* the magnetizing spiral.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was

put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first disposed to consider the phænomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible; but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in fig. 12, which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoining conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next section, I may here state that the phænomenon mentioned, paragraph 54, in reference to helix No. 4, is connected with the neutralizing ac-

tion. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or, in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine; since the plates of metal which sometimes form the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Calland, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connexion with several other points, in another communication to the Society.

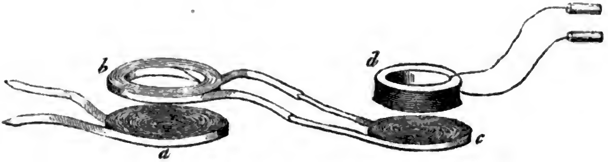
77. The results given in this section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62.), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.—*On the Production and Properties of induced Currents of the Third, Fourth, and Fifth Order.*

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in fig. 8 furnishes a ready means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2 is placed over this to receive the induction with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without

Fig. 8.



a coil No. 1, *b* coil No. 2, *c* coil No. 3; *d* helix No. 1.

the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *a*, *b* are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in fig. 9, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists, as it were, of a single wave of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse; and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue,

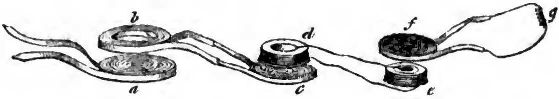
when helix No. 1 was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, a *quantity current can be induced from one of intensity, and the converse.* By the arrangement of the apparatus as shown in fig. 9, these different results are exhibited at once. The induction from coil No. 3 to helix No. 1 produces an intensity current, and from helix No. 2 to coil No. 4 a quantity current.

Fig. 9.



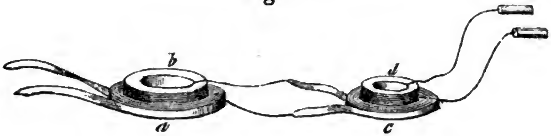
a coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1, *e* helix No. 2 and 3, *f* coil No. 4, and *g* magnetizing spiral.

88. If the ends of coil No. 2, as in the arrangement of fig. 8, be united to helix No. 1 instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2 appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in fig. 10. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connexion with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and

Fig. 10.



a coil No. 2, *b* helix No. 1, *c* coil No. 3, and *d* helix No. 2.

ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous that the inductive effects at the beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which, however, may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11.) was attached to the ends of coil No. 4, fig. 9, and by the polarity of the needle it was found that this current was in the same direction with the secondary and primary currents*. By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion, however, caused me much useless labour and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders commencing with the secondary.* This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current	+
Secondary current	+

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* It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

Current of the third order . . . −
Current of the fourth order . . . +
Current of the fifth order . . . . −

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phænomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series): but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribands together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary

in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement of the last paragraph, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected, as it were, through a small distance of air.

97. We would also infer that two contiguous secondary currents, produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result, however, was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

[To be continued.]

XLVII. *On the Natural Products which originate from the action of the Atmosphere on Iron Pyrites. By* TH. SCHEERER*.

IT is a well-known fact, that iron pyrites, in the finely divided state in which it occurs in alum slates, is easily oxidized by the atmosphere, causing the parts exposed to acquire a reddish brown colour; nevertheless, the products of this decomposition are seldom to be found, from the rain washing them away. In a spot near Modum in Norway, I met with a cavity in the mountains where they were deposited as incrustations, safe from all destructive influences. Three distinct layers were evident.

The first and upper layer is a dark brown massive mineral with which the slate is impregnated: A.

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* An extract obligingly communicated by the author from the original paper published in Poggendorff's Annalen, vol. xlv. p. 188.

The second layer forms a light yellow mass distinctly separated from the first, and forming incrustations similar to those which occur in (dolomitic) limestone caverns: B.

The third is clothed with a layer of small white crystals: C.

The analysis of A gave:

80·73	peroxide of iron
6·00	sulphuric acid
13·57	water
100·30	

which corresponds to a combination of 14 atoms of peroxide of iron, 2 atoms of sulphuric acid, and 21 atoms of water, which is expressed by the formula,

$$2\,(\underline{\dddot{\mathrm{Fe}}}^{7}\,\dddot{\mathrm{S}}) + 21\,\underline{\dot{\mathrm{H}}};$$

or,

$$2\,(\underline{\dddot{\mathrm{Fe}}}\,\dddot{\mathrm{S}}^{3} + 20\,\underline{\dddot{\mathrm{Fe}}}) + 63\,\underline{\dot{\mathrm{H}}},$$

according to the manner in which the formula is written. This iron-salt may be called, after the nomenclature of Berzelius, the twenty-fold basic sulphate of the peroxide of iron; it is the most basic salt as yet known. The oxygen of the oxide amounts to the double of that in the water. It is perfectly insoluble in water.

Two analyses of the substance B gave the following results:

1	2	
49·37	49·89	peroxide of iron
32·42	32·47	sulphuric acid
5·03	5·37	soda
13·13	13·09	water
99·95	100·82	

The soda was found in both analyses to contain a small quantity of potash, which, however, is of no importance for the formula, which may be thus expressed,

$$4\,\underline{\dddot{\mathrm{Fe}}}\,\dddot{\mathrm{S}} + \dot{\mathrm{N}}\,\dddot{\mathrm{S}} + 9\,\underline{\dot{\mathrm{H}}};$$

namely 4 atoms of peroxide of iron, 5 atoms of sulphuric acid, 1 atom of soda, and 9 atoms of water.

The substance C was found to be pure gypsum.

In explaining the commencement and continuation of this process, it must be supposed that the sulphate of the protoxide of iron was at first formed by the oxidation of the iron pyrites; this became gradually oxidized, and was deposited as the first brown layer of the salt A described. Yet the deposition of this basic iron-salt must have happened under singular circumstances, for it is a well-known fact that a solution of iron-vitriol, oxidized by the atmosphere, is precipitated as a *five-fold* basic salt. It is likewise difficult to explain how the yellow layer containing alkali *suddenly* succeeded the dark-brown; it may indeed be supposed, that at the commencement of this decomposition of the iron pyrites the alum-slate resisted for some time all action, until it was attacked, and its alkali dissolved by the sulphuric acid, which commenced the formation of a new salt. But if this mode of explanation has much appearance of probability, the *sudden* cessation of the one product of decomposition, and the commencement of the second, is a strange fact. That the gypsum, as the more easily soluble substance, is found on the inferior part of the ceiling of the cavern is, on the other hand, easily conceived. The lime in it undoubtedly acted no unimportant part at the deposition of the iron salts described, aiding in their precipitation by saturating the acid.

XLVIII. *Experiments and Observations on Light which has permeated coloured Media, and on the Chemical Action of the Solar Spectrum.* *By* Robert Hunt.*

M. GAY-LUSSAC, when speaking of the beautiful discovery of M. Daguerre, said, "The palette of the painter is not very rich in colour, black and white compose the whole. The image in its natural and varied colours may remain long, perhaps for ever, a thing hidden from human sagacity †."

However, the production of a coloured picture of the spectrum by Sir John Herschel, and some effects produced by Mr. Talbot, together with some delicate tinting which I observed, when, during the summer of 1839, I was engaged in copying some flowers of Nature's richest painting, led me to think coloured photographs within the range of probabilities, and induced me to pursue a train of experiments from which, although little has resulted to heighten my first hopes, I have gathered much that is curious and certainly instructive.

Photographic Papers.

1. By saturating paper with different chlorides and mu-

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* Communicated by the Author.

† "The History and Practice of Photogenic Drawing, &c., by L. J. M. Daguerre. Translated by J. S. Memes, LL.D."

riates, always keeping in view the definite proportion required for the quantity of the nitrate of silver used; it will be found that almost every variety of shade, from a rich dark purple to a full red, and a few other tints, may be produced at pleasure.

2. The effects of light, passing through coloured glasses on various papers, are singularly diversified. The following are a few of the most striking results. (The glasses are, a deep cobalt blue, a full laurel green, an amber yellow, and a rich orange red. They are so framed that all the papers can be exposed at the same time to the solar influence.)

Colour of Glass.

	Blue.	*Green.*	*Yellow.*	*Red.*
Salt used.	Effects produced.			
a. Chlor. of sodium.	Purple.	Blue.	Violet.	Chocolate.
b. Chlor. of potassa.	Light purple.	Sky blue.	Light violet.	Tinted red.
c. Muriate of lime.	Rich violet.	Faint blue.	Blue.	Reddish.
d. Muriate of iron.	Red.	Colourless.	Faint red.	Leaden hue.
e. Mur. of peroxide of iron.	Blue.	Yellowish.	Straw color.	Yellow brown.
f. Mur. of baryta ...	Purple red.	Lilac.	Chocolate.	Pink.
g. Muriate of manganese............	Rich brown.	Reddish.	Rose hue.	Yellow.
h. Mur. of ammonia	Olive brown.	Pale brown.	Brown.	Dull orange.

3. I have found but a modified action from the interference of coloured fluids. In a few instances, under a solution of carmine in ammonia, I have obtained the richest crimson dye; but I cannot, by any means I have used, succeed in fixing the colour on the paper.

4. A paper prepared, by first washing it with a solution of twelve grains of the iodide of potassium in one ounce of water, and then with a solution of ten grains of the crystallized nitrate of silver in the same quantity of fluid, is very sensitive. When exposed beneath a solution of the ammonia-sulphate of copper to sunshine, it changes to a rich *light blue*. Acetate of copper produces a *brown*. Muriate of the peroxide of iron imparts a *green tinge*, and solutions of carmine a *brown red*.

5. The paper *f* becomes *red*, when acted on by rays passing through nitrous acid gas, and is tinged *yellow*, by the light which has been subjected to the interference of chlorine and its protoxide.

6. To have as full a volume as possible of iodine and bromine vapour, carefully closed vessels containing a small portion of these bodies, were placed upon a plate of copper warmed by water.

The paper *h* was laid beneath them, and exposed to luminous influence. Under the bromine it was unchanged, but beneath the iodine the paper became richly iridescent. The colours changed to a uniform violet tint upon a few minutes' exposure to direct sunshine.

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7. Papers already darkened by sunlight during prolonged exposure to the influence of the dissevered rays of the spectrum, assume a variety of colours. The same changes may be effected by carefully arranging glasses, and placing the photographic preparations beneath them. I shall copy exactly the memoranda of my journal.

Dec. 12, 1839.—I placed under blue, green, yellow, and red glass the following papers:

A. *Muriate of ammonia*, with two washings of solution of the nitrate of silver, darkened by exposure to a rich chocolate.

B. *Muriate of manganese.* Silver, two washings, darkened to a full brown.

C. *Iodide of potassium.* Silver, one washing, darkened to a yellow brown.

D. *Iodide of potassium* and silver, two washings, darkened to a red brown.

E. *Chloriodic acid.* Silver two washings, darkened to a rich bronze.

F. *Chloriodic acid* with *Liquor potassæ.* Silver, two washings, darkened to a blue-brown.

Dec. 13.—After twelve hours exposure to the dull light of rainy weather, the paper E has become blue under the blue glass. No change is apparent on the others.

Dec. 27. *Colours of Glass.*

	Blue.	*Green.*	*Yellow.*	*Red.*
A. has become	Olive.	Deep green.	Dirty yellow.	Red.
B.	Deep brown.	Bat colour.	Blue brown.	Red.
C.	Do.	Darkened.	No change.	Red brown.
D.	Black.	Light brown.	Rich brown.	Brick red.
E.	Blue black.	Darkened.	Darkened.	Dusky red.
F.	Black brown.	Dull plum.	Bluish.	Reddened.

Jan. 2, 1840.—All the papers go on increasing the distinctness of their colours, except E and F, which have assumed different shades of blackness.

(E *and* F *were removed, and a paper* G, *prepared with muriate of baryta and two washings of silver, darkened to a chocolate, substituted.*)

Feb. 7. *Colours of Glass.*

	Blue.	*Green.*	*Yellow.*	*Red.*
A.	Rich olive.	Green.	Yellow.	Purple.
B.	Black.	Chocolate.	Light brown.	Red.
C.	Do.	Red brown.	Do.	Brown.
D.	Chocolate.	Umber brown.	Black.	Red brown.
G.	Bright olive.	Yellow brown.	Pale olive.	Reddish.

The two papers A and G exhibit much more sensitiveness to luminous influence than any others I have yet tried.

8. The paper A, when washed with a weak solution of the hydriodate of baryta, gives under the pencil of light a beautiful picture, whether used in the camera or for surface drawings.

These pictures exhibit the peculiarities mentioned by Mr. Talbot at the British Association*. Sunshine changes "the colour of the object delineated from reddish to black with great rapidity." This gentleman adds, "after which no further change occurs." I much regret I have not been fortunate enough to succeed thus far in fixing my drawings. The continued influence of light in a few months obliterates the impression.

A singular change follows the exposures of these pictures to coloured light.

If placed under vessels containing coloured fluids (4.) and exposed either to sunshine or to diffused light, in a few days the picture becomes a full *red* under the blue; a *rose hue* under the green; a *light blue* under the yellow, and a *deep blue* under the red. These colours after deepening for some time gradually change to different shades of *green* under the blue and green fluids, to a *pink* under the yellow, and a *red* under the red fluid (25.). After this, the colours alter no more, and the picture bears exposure to light much better than at first; but I doubt if it is rendered perfectly permanent, for the dull light of January and February has spread a downiness, like a mist, over those photographs which have been constantly exposed.

Daguerreotypes.

9. Exposing a plate, over which some lace was carefully placed, under four coloured glasses (2.) for three minutes to diffused light, I obtained, under the blue glass a beautiful copy; no trace of a drawing beneath the green; a tolerable impression beneath the yellow; but the mercury would not attack the space beneath the red.

10. A plate similarly arranged beneath four bottles of coloured fluid (4.) exposed to diffused light for fifteen minutes, was found on being acted upon by the mercurial vapour to present the same appearance as above (9.), excepting that a faint design was evident over the space the carmine fluid had covered.

11. I arranged a dark chamber, to which no other rays could pass but such as had permeated *two inches* of coloured fluid.

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* Athenæum, No. 618.

Having filled my trough with a saturated solution of the bichromate of potassa, I exposed a plate for five minutes to its influence in full sunshine. *There was not the slightest action.*

12. In one hour on a similar plate, under the same circumstances, I obtained a faint, but still defined outline of a dried fern.

13. I exposed a bare iodidated plate for two hours to the same influence. On removing it from the chamber no difference was apparent; but I found it was no longer sensitive to light, and the iodide adhered more closely to the metal than it did (28.).

This is a reverse action, for after the exposure of a prepared daguerreotype plate to light, the sensitive film is most easily rubbed off* (28.).

14. Red solutions impart a very decided rose hue, or more strictly speaking the influence of red light on the iodidated plate occasions that peculiar arrangement of the mercurial particles, which is necessary to the production of red colour.

15. Green solutions act with more or less effect in obstructing the passage of the so-called chemical rays according to their depth of colour. But in no instance have I found them to produce that close combination, which the yellow and sometimes the red fluids do, of the iodide and the under surface of unattacked silver (28.). By examining the effects produced by green media (2, 7, 16.) a peculiar order of interference will be remarked (19.).

Germination and the growth of Plants.

16. I planted in a box some curled cress seed, and so arranged bottles of carmine fluid, chromate of potassa, acetate of copper, and the ammonia sulphate, that all but a small space of the earth was exposed to light which had permeated three-fourths of an inch of these media.

For some days the only apparent difference was that the earth continued damp under the green and blue fluids, whereas it rapidly dried under the red and yellow. The plumula burst the cuticle in the blue and green lights, before any change was evident in the other parts.

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* On this principle I now polish my silvered plates, by which the troublesome process with nitric acid and pumice is got rid of. I wash the surface of silver over with a solution of the iodide of potassium holding a little iodine free, and rub it lightly until all the parts are equally attacked. I then expose the plate to light for a few minutes, and polish off with dry cotton. In five minutes by this process the most perfect lustre may be given to the silver, and it has the advantage of rendering the plate more susceptible to the influence of the iodine vapour.

After ten days, under the blue fluid there was a crop of cress, of as bright a green as any which grew in full light, and *far more abundant.*

The crop was scanty under the green fluid and of a pale unhealthy colour (15.).

Under the yellow solution but two or three plants appeared, yet they were less pale than those which had grown in green light. Beneath the red bottle the number of plants which grew was also small, although rather more than in the spot the yellow covered. They too were of an unhealthy colour.

17. I now reversed the order of the bottles, fixing the red in the place of the blue, and the yellow in that of the green. After a few days' exposure the healthy cress appeared blighted, while a few more unhealthy plants began to show themselves, from the influence of the blue rays, in the spot originally subjected to the red.

It is evident from this that the red and yellow rays not merely retard germination, but positively destroy the vital principle in the seed. Prolonged exposure uncovered, with genial warmth, free air, and indeed all that can induce growth, fails to revive the blighted vegetation.

I have repeated the experiment many times, varying the fluids, but the results have been the same. At this time I have the above facts strikingly exemplified where the space covered by the bichromate of potassa is without a plant.

These results merit the attention of those who are engaged in the study of vegetable œconomy. Do they not point at a process by which the productions of climes more redolent of light than ours may be brought in this island to their native perfection?

Dr. Draper's "experiments" (Philosophical Magazine, Feb. 1840, pres. vol. p. 81) appear at variance with mine.

Under the influence of a nearly tropical sun permeating half an inch of solution of the bichromate of potassa, cress grew of a green colour, whilst it took five days to give a sensitive paper a faint yellow green colour. From this Professor Draper argues the existence of two classes of rays, a different class being necessary to produce the green colouring of vegetable foliage from that which darkens chloride of silver.

With submission to one whose facilities for such inquiries are so much greater than my own, I would suggest a repetition of the experiments with some of the recently discovered photographic preparations. The papers *f* and *h*, both under coloured glass and great thicknesses of yellow fluid are deepened to a plum-brown in less than an hour *.

Under three inches of the bichromate of potassa the paper, *f*, became in eight hours sunshine of a full blue-brown.

18. The fact of cress and pea plants growing green, under the influence of such powerful light as penetrated Professor Draper's yellow media, will not appear at all surprising when we examine the rays which pass through such fluids.

This I have done by forming a spectrum, interposing the coloured body between the prism and the sun. The following are the effects of a February sun at Devonport.

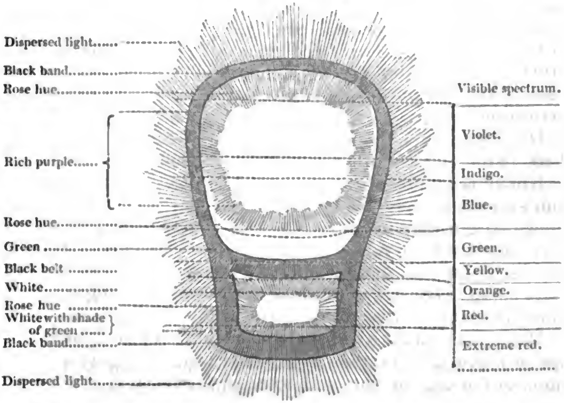
Through a deep blue solution of the ammonia-sulphate of copper, the violet, indigo, blue, and a portion of the green rays pass.

Through solutions of the muriate, acetate and nitro-muriate of copper with iron, the green ray, and a considerable portion of the yellow; a trace of the blue also is evident.

Through solutions of the bichromate and chromate of potassa, the chloride of gold and decoction of turmeric, the red, the yellow and the green rays are seen, *and by taking their impression on a daguerreotype plate a line of the blue is distinctly marked.*

Through nitro-muriate of cobalt in ammonia, carmine in ammonia, and sulphuric acid and decoction of cochineal, the red and yellow rays alone appear to penetrate.

THE SPECTRUM.



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* The papers which accompany this article were exposed under the glasses and three-fourths of an inch of fluids for forty minutes. The order of interference and consequent colouring is plainly shown.

19. It will be observed, that the light which has passed through a green medium (2, 7, 9, 10, 15, 16) acts less powerfully in darkening photographic papers, and occasions vegetable leaves to be even paler than that which has been subjected to the interference of a yellow medium.

I am led to suspect that the band of rays formed by the meeting of the yellow and the green has an influence similar to the extreme red, in neutralizing the powers of the other adjacent rays, as was first noticed by Sir John Herschel, (22.), (23.), (26.).

20. The figure on the preceding page represents the solar spectrum, as it impresses itself on a daguerreotype plate, not in shadows merely, but in colours, which have the peculiar appearance of the down upon the nectarine.

The most refrangible portion of the spectrum is represented in full colours, shading from indigo to a delicate rose, which is lost in a band of pure white.

21. Beyond this a protecting influence is powerfully exerted, and notwithstanding the chemical effect produced over the plate, by the dispersed light, a line is formed free of mercurial vapour, and which consequently appears black.

22. The green portion of the spectrum is represented in its true colour, but it is considerably less in size than the space occupied by these rays.

23. The yellow rays are without action, or rather they do not prepare the silver for the reception of the mercury, and consequently a black belt marks the space on which they fell, and extends a little beyond it into the green (19.).

24. A white line marks the place of the orange light.

25. The red is represented by a well-defined rose colour, bounded, as were the more refrangible rays, by a white line, shaded at the lower extremity with a green.

This passing of the red into a green and of the blue into a rose colour (20.) is strikingly similar to the effect produced, by the interference of coloured media, on some photographic drawings (8.).

26. The lowest dark space on the picture is a beautiful illustration of the influence of the extreme red rays in protecting the silver from luminous action (19.) (21.).

27. What appears more surprising to me than even the detection of the *negative*? rays at each end of the prismatic spectrum, is the continuation of the dark line throughout *its whole length*, evidently showing the influence of the same cause as is so effective at the least refrangible extremity.

This band is not equally defined throughout its entire circumference. It is the most strikingly evident from the extreme red to the green; it fades in passing through the blue and increases in intensity as it leaves the indigo, until, beyond the invisible chemical rays, it is nearly as strong as it is at the calorific end of the spectrum.

Does not this protected surrounding band appear to indicate the existence of rays of a peculiar and unknown order, proceeding from the extreme edge of the sun?

28. By lightly rubbing a daguerreotype picture of the prismatic rays, it is obliterated, except over the space of the yellow and red portion. This effect corresponds with my experiments on media of these colours (11. 12. 13).

Until we have more experience than we now have of the effects of the solar rays individually and collectively, we can offer no satisfactory explanation of the process in action, on a daguerreotype plate, by which the subtle painter LIGHT impresses such delicate designs.

The existence of two iodides of silver, is, I think, certain. In my photometric experiments I have always observed the formation of an iodide which speedily darkens, and of another portion which is unalterable by light*.

The sensitive film on the silver plate appears to be the former of these iodides. Throughout the range of the chemical spectrum, *particularly so called*, the iodide is I imagine converted into an oxide of silver; that a partial oxidation takes place numerous experiments have rendered certain; whilst the influence of the rays of least refrangibility is to form the unchangeable iodide of silver. Experiments, however, are wanting to prove this satisfactorily.

An attentive consideration of the facts I have enumerated, will, I think, satisfy all, that we can no longer with propriety attach the name of chemical to the most refrangible rays only. Every ray has its particular chemical office, either of composition or of decomposition; and although Seebeck has attributed the acquirement of a rose hue by chloride of silver when put into the red ray, to the heating power of that portion of the spectrum, it is now proved to be dependent upon some other influence, for where it has been shown the most calorific rays exist this salt undergoes no change.

Devonport, February 29, 1840.

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* [See Mr. Talbot's account of the processes employed in Photogenic Drawing, Lond. and Edinb. Phil. Mag., vol. xiv. p. 210 (2).—EDIT.]

XLIX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.* *By* THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*

> "Men believe that reason governs their words, but words have often power enough to react upon reason."—*Bacon.*

THE following remarks, bearing on the mineral structure of a considerable portion of the south of Ireland, are drawn from me by certain representations made by Mr. Griffith on the same subject, respecting which my first impression was that I might be content to suffer them to pass without public comment, persuaded that no intelligent geologist, who would duly sift and weigh the evidence adduced on both sides, could be at a loss in coming to a correct decision. But as it is not every reader who will take this trouble, and as unquestioned assertions not unfrequently pass as established truths with the unwary, further reflection has taught me that I owe it both to the Geological Society and to myself not to remain silent. However adverse to controversy, and however irksome the task of entering upon it, a man should always be ready to give a reason for the faith that is in him.

It has so happened that my Memoir on the Geological Relations of the south of Ireland* (which in its more material points was communicated to the Geological Society in the year 1830), and Mr. Griffith's Outline of the Geology of Ireland, with its accompanying Geological Map†, were brought before the public eye much about the same time, namely, in the early part of 1838. The marked discrepancies observable not only between our respective maps of the south of Ireland, but also in Mr. Griffith's own map itself, could not fail to strike other geologists as well as myself; while, on referring to the written outline, so far from finding an elucidation of the discordances, the outline itself appeared at variance with the map. I have reason to know that other geologists felt equally unable to discover the precise purport and extent of Mr. Griffith's meaning, and hence some explanation seemed necessary. Doubtless, aware of such an impression, Mr. Griffith has been led to give that explanation, accompanied by many changes in his views: 1st, in a communication made to the British Association at Newcastle, in August, 1838‡; 2nd, in a letter addressed to the Rev. Dr. Buckland, Pres. Geol.

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* Geol. Trans., vol. v., second series, 1837.

† Appendix to the Report of the Irish Railway Commissioners, 1838.

‡ Report of the Eighth Meeting of the British Association, 1839.

Soc., in May, 1839*; and, 3rd, in a paper read before the Geological Society of Dublin, on the 13th June, 1839, the last-mentioned being accompanied by two sections, one referring to the south-eastern portion of the island, and the other to a part of the extreme west, in the county of Kerry†. The views of Mr. Griffith thus appearing in an authenticated form before the public, and the author having in his last production, while attempting to explain his own positions, found it necessary to assail mine, the time has fully arrived for adverting to those views. Being thus put on my defence, I shall proceed to consider, in connexion, the Outline with its Geological Map, and the three later written communications, with the two sections supplied by the author; and in so doing, I shall feel no difficulty in showing that many of Mr. Griffith's representations are not only irreconcilable with the facts, but that those representations contradict each other ‡.

The question at issue lies more particularly between the older stratified rocks of the south of Ireland and the old red sandstone properly so called, which occurs in different portions in that quarter of the island.

In the Outline (at p. 7), Mr. Griffith professes to distinguish the older stratified rocks as consisting of an older and a newer transition series, the latter, which is coloured purple, being said usually to repose unconformably on the former, which is coloured grey; or, as it is later expressed in another place§, usually resting unconformably on the greywacke slate or Silurian series; and the old red sandstone, which is said to succeed, being distinguished partly by a reddish-brown and

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* Proceedings of the Geological Society, 22nd May, 1839.

† Journal of the Geological Society of Dublin, vol. ii. part 1., 1839.

‡ This paper was drawn up before I had seen another Geological Map of Ireland put forth by the author, on a large scale, in 1839 *; and to this latter map, it appears, Mr. Griffith's two last-mentioned communications refer also in part. If the discrepancies in the map appended to the "Outline" were startling, the numerous arbitrary alterations introduced in the new map are no less striking; and it may fairly be inquired what reliance is to be placed on either of them, disagreeing largely as they do with each other. I can perceive, in the new map, an approximation in some parts to my own views, but an utter discordance in other parts. As, however, the alterations which have been made in the new map do not materially interfere with the course of my argument, which in the first instance bears directly on the map attached to the "Outline," I have left the text unchanged, merely adding incidentally a few notes in reference to the new map, for the purpose of continuing the comparison. The discrepancies between Mr. Griffith's two maps and my own map of the south of Ireland, will thus become doubly apparent.

§ Journal of Geol. Soc. of Dublin, vol. ii. p. 85.

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* Hodges and Smith, Dublin, and James Gardner, London.

partly by a yellowish-brown colour. Such is the statement. Now, how are these positions established?

Mr. Griffith admits* the correctness of the view, which I gave more than twenty years since†, of the relative position of the old red sandstone, as occurring in detached portions in the county of Wexford, on both banks of Waterford harbour up to the confluence of the Barrow and the Suire, and again along the valley of the latter river west of Waterford, flanking both its sides, that on the north extending into the counties of Tipperary and Kilkenny, and that on the south bordering the river and constituting higher up the Monavoullagh group; in all these cases reposing unconformably on the older stratified rocks. They are thus all placed in the same class as the old red sandstone formation: and this being the case, it may be asked, why is the reddish-brown colour, as indicative of old red sandstone, confined alone to those portions of it which occur in the county of Wexford, extending to the eastern side of Waterford harbour, while on the opposite or western side of that harbour and of the river Barrow, all the other districts specified, and acknowledged to be old red sandstone, are coloured purple, as indicative of a newer transition series? Here is a direct contradiction, both in colours and terms, as well as in the reputed order of succession.

The author having thus in the first place admitted that the old red sandstone of the Monavoullagh range is of the same age as that of the valley of the Suire, of the Barrow at its confluence, and on the eastern and western confines of Waterford harbour, &c., proceeds next to show that it belongs to a newer transition series: and how is this accomplished? by representing the old red sandstone of the Monavoullagh range, the stratified structure of which is admitted to vary only a few degrees from the horizontal‡, as dipping in its southern prolongation suddenly to the south at a high angle, and extending down to the coast, namely, to Ballyvoil Head; thus meaning to identify the old red sandstone of the Monavoullagh with the sandstone conglomerate, sandstone, and red slate which occur on that part of the coast in association and interstratified with transition rocks at a high angle. Here, doubtless, lies the main source of Mr. Griffith's misconception, and the consequent train of errors and inconsistencies into which it has led him; and on the faith of which he ventures to state[*] that, "had Mr. Weaver, who represented the conglomerate of Monavoullagh as a mountain cap resting on greywacke slate, made a careful section of the strata, either from Monavoullagh or Ballyvoil Head, he would have been convinced of his error, and probably have arrived at the same conclusion as mine;" a singular conclusion certainly, inasmuch as there is no apparent connexion whatever between the horizontal sandstone conglomerate of the Monavoullagh range and those beds of conglomerate, sandstone, and red slate of the coast which extend eastward from the vale of Dungarvan in several separate discontinuous bands interstratified with other transition rocks; all these dipping throughout at a high angle, chiefly to the south, but also to the north, which latter position may be seen in the red sandstone conglomerate and the associated rocks in Tranamoe head, within a short distance of Bonmahon river. I have described elsewhere that these rocks of the coast are connected on the north and west with varieties of clayslate (black, blue, green, yellow, red, and purple), alternating with greywacke, quartz rock, hornstone, red sandstone, and conglomerate, and comprising also subordinate beds of greenstone with porphyritic varieties of the rocks which I have enumerated[†]. This statement was not lightly given, having carefully examined the interior in many directions, as well as the whole line of coast extending from Dungarvan harbour on the west to Waterford harbour on the east; and having in the course of my researches (in 1824) discovered transition fossils in the series, I had the greater pleasure in exploring the district, and in ascertaining with exactness the composition and structure of the rocks, as I had just completed and published my account of the Tortworth transition district in Gloucestershire[‡].

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* Journal of Geol. Soc. of Dublin, vol. ii. pp. 85, 86.

† Geol. Trans., vol. v., first series, part 1, 1819. Memoir on the East of Ireland.

‡ Eighth Report of the British Association—Transactions of the Sections, p. 82.

But it is not necessary to rely on my own testimony alone. The interesting remarks of Mr. Holdsworth[§] on the eastern part of the county of Waterford, extending from the Bonmahon coast to the Monavoullagh range on the west, illustrated by a map, come in aid of my views; and they may be considered the more valuable as proceeding from an unbiassed observer. And had Mr. Griffith fully attended to them,

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[*] Journal of Geol. Soc. of Dublin, vol. ii. p. 86.

[†] Geol. Trans., vol. v., second series—Memoirs on the south of Ireland. §§ 15 to 20 inclusive.

[‡] Geol. Trans., vol. i., second series, 1824.

[§] Journal of Geol. Soc. of Dublin, vol. i., part 2. 1834.

(putting my own descriptions out of the question), he must have seen that Mr. Holdsworth places in the map and speaks in the text of the clayslate formation, as being observable in different places along the eastern border of the Monavoullagh range of conglomerate; and especially as being of considerable extent in the vicinity of Stradbally, where it is in a highly-inclined position, and in places nearly vertical, stretching thence to the westward toward the vale of Dungarvan, and there coming in contact with the carboniferous limestone of that valley. In its range, therefore, the clayslate formation evidently occupies the district lying between Ballyvoil head on the south and the Monavoullagh range of conglomerate on the north; and Mr. Holdsworth observes*, that in this part the base of the Monavoullagh mountain range approaches close upon the clayslate formation. With respect to the Monavoullagh conglomerate itself, he remarks†, that having examined this mountain range in several places along its line from north to south (which extends many miles), and from top to bottom, it nowhere appeared as a conformable rock. Now, let us return to the coast, stretching eastward from Ballyvoil Head. What is the purport of Mr. Holdsworth's observations in this quarter, and more inland, in reference to the occurrence of red sandstone conglomerate and red slate as forming beds included in the clayslate formation? He says, that in various places along this line of coast, and particularly around Bonmahon, he met with the conglomerate usually accompanied by red micaceous slate, both being in an inclined position, and having the appearance of being a conformable formation, inasmuch as these strata, where they occur, appear bounded on each side by the common rocks of the coast. And the conglomerate is described as being both coarse and fine-grained, the latter passing by insensible gradations into the red sandstone slate; and a coarse sandstone with ferruginous marks also occurring. The red sandstone slate passes, he says, sometimes into grey slate, portions of which occur abundantly in the composition of the conglomerate of the Monavoullagh range. Mr. Holdsworth, in concluding, touches upon the question as to the source and origin of the conglomerate range of the Monavoullagh and those beds of conglomerate which are conformably associated with the common rocks of the coast and in the interior, which latter he also terms, "detached dyke-like masses;" and he inquires whether they may have been contemporaneously raised to their present level; a speculation into which I do not think it necessary to enter, the established fact of the difference of position and association, and consequently of date as to origin, being sufficient for my present purpose. Other matter is to be found scattered through Mr. Holdsworth's observations, which comes very well in corroboration of the general view which I have given of the structure and composition of that district in my memoir on the south of Ireland; although Mr. Holdsworth nowhere draws the distinction himself between the transition conglomerate and sandstone, and the overlying conglomerate and sandstone of the Monavoullagh range, further than by considering the former as a conformable, and the latter as an unconformable formation. On the contrary, he gives it as his opinion that all the conglomerates and sandstones which he has noticed in the eastern part of Waterford are identical, excepting perhaps those occurring on the north-west of the city of Waterford and those around Dunmore on the western side of Waterford harbour, which he conceives may be of more recent formation, but no reason is assigned for this opinion. The identity spoken of, however, seems mainly to refer to the similarity of mineral composition, respecting which there can be no difference of opinion.

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* Journal of Geol. Soc. of Dublin, vol. i., part 2, p. 89. † *Ibid.*, p. 97.

But to return to Mr. Griffith. It is clear from the preceding that he has confounded together the old red sandstone formation, properly so called, with the red conglomerate, sandstone, and slate, which occasionally occur as constituent beds in a transition country.

The horizontal position of the Monavoullagh sandstone conglomerate is quite in accordance with what is observable in the detached portions of the same formation which are studded over the northern border of the clayslate table-land, overlooking from the south the valley of the Suire, and where in the year 1814, when first exploring that country, I found the sandstone conglomerate reposing unconformably on the truncated edges of the subjacent clayslate. Mr. Holdsworth appears to have made a similar observation in a quarry adjacent to the road between Kilmacthomas and Portlaw, where the junction of the clayslate and the conglomerate is very distinctly marked, the slate there being thrown up nearly perpendicular*.

Indeed, when we consider the long-drawn range of the Monavoullagh from north to south, being nearly at right angles with the eastern and western strike of the bordering older stratified rocks, both on its eastern and western confines, whose dip varies from south to north, how could we expect any conformity between them?

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* Journal of Geol. Soc. of Dublin, vol. i., part 2, p. 95.

If we now pass to the westward of the Monavoullagh sandstone conglomerate range by ascending from the vale of the Blackwater, from Lismore for example, to the table-land on the north, on which are planted the Knockmildown masses of the old red sandstone formation, we traverse in our course the older stratified rocks, still possessing the eastern and western strike, and for the greater part the southerly dip, and in which occur numerous slate quarries. The Monavoullagh sandstone conglomerate range extends nearer to the declivity leading down to the vale of Dungarvan than the Knockmildown sandstone conglomerate range does towards the Blackwater valley, from which it recedes considerably to the north, and nowhere does that formation approach and reach the vale until we enter upon the region of the Kilworth mountain and the adjacent hills, where it descends toward that valley and supports the carboniferous limestone; while in the defiles and ravines by which that mountain is furrowed, and whose course is toward the Blackwater, the clayslate formation is exposed to view in strata nearly vertical, supporting the old red sandstone formation in unconformed position*. But Mr. Griffith represents the conglomerate of the Monavoullagh as underlying these older stratified rocks, although they lie mainly to the west rather than to the south of the Monavoullagh range; so that, according to his view, the whole of the schistose country lying to the west of the Monavoullagh range, and extending to Kilworth, together with the superimposed ranges of sandstone and conglomerate of that region, are swamped in one general formation, coloured purple, which colour is made to extend as far south as to a line drawn from near Ringabella inlet, adjacent to the southern entrance of Cork harbour, to the head of Bantry bay, and thence to the western coast opposite to Dursey island, the said colour denoting a newer transition series, while the included bands of limestone are considered as belonging to the carboniferous series, although it is admitted that the adjacent clayslate is interstratified occasionally with the limestone, such slate, however, to meet the occasion, being called a "carboniferous slate *."

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* Memoir on the South of Ireland, in Geol. Trans., vol. v., second series, § 47, in which I have remarked that the formation, as there exhibited, is quite analogous to the old red sandstone of England, presenting the same varieties in colour and composition, and consisting of compact and slaty beds of firm sandstone, associated with others of a looser texture, with sandstone conglomerate, and with beds of indurated clay and slaty clay; the reddish-brown colour predominating in the series. Similar relations are also well displayed in the old red sandstone of Kerry Head and the Slieve Meesh range, as well as in the ranges of Knockfeernagh, Kilcruaig, and Kilmeady, taken in connexion with the chain of the Seefin, Slieve Riagh, Slievenamuck, and Gaultees mountains. *Ibid.* §§ 48, 49, 50. The parallel might be continued still further north, *Ibid.* § 69, as well as in the various quarters indicated in my memoir on the east of Ireland, Geological Transactions, vol. v. first series.

Nor does the dilemma end here. The eastern part of Waterford is coloured grey, as indicative of an older transition series, and the rest of the schistose and conglomerate tract, extending to the western extremity of Kerry and north of the line already noticed, are coloured purple, as signifying a newer transition series†.

Now, in the year 1824 I discovered (as before mentioned) transition fossils in the south-eastern part of the county of Waterford, and in 1833 Mr. Holdsworth observed the same: and in 1829 I found other transition fossils in the western part of the Dingle peninsula in Kerry; and in 1837 Mr. C. W. Hamilton discovered them also in another adjacent locality, to the list of which Mr. Griffith has since added others. Those collected by myself appeared chiefly referable to such as are found in what have since been denominated in England the Cambrian and Silurian systems, and the same inference appears deducible from those later collected. These tracts therefore clearly belonging to an ancient transition series, it may reasonably be inquired why is the Waterford district coloured grey as an older transition series, and that in Kerry purple as a newer transition series ‡?

Yet in reference to Kerry and part of Cork Mr. Griffith also contends §, that "the red conglomerate of Cahirconree and Carrantoohill (Gurrane Tual) mountains, together with the coarse red slate of which Tomies and Glenaa mountains at Killarney are composed, belong to the old red sandstone series; and that the limestone of Killarney, Kenmare, and Bantry, belongs to the carboniferous and not to the transition series." Having advanced so far, if Mr. Griffith had gone a step further (which he might have done with just as much propriety,) and included the patches of limestone near Skibbereen and Courtmacsherry bay also in the carboniferous series, the result according to Mr. Griffith's view would be

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* In the new map it will be seen that in the eastern quarter of this tract the purple colour is now converted into reddish-brown, as indicative of old red sandstone, and in the western quarter chiefly into grey, as denoting greywacke slate.

† But remodelled in the new map, with reddish-brown as old red sandstone in the eastern quarter, and grey as greywacke slate in the western, as already mentioned.

‡ In the new map, the greater part of Kerry and a small part of Cork are now coloured grey, as greywacke slate.

§ Eighth Report of the British Association, pp. 83, 84.

that we have no transition limestone at all in the south of Ireland *; and by parity of reasoning, that all the schistose and conglomerated rocks associated with these bands of limestone in that part of the island, belong to the old red sandstone formation, since he cannot deny, and indeed admits, that those bands of limestone are interstratified on their confines with the adjacent rocks.

But in another place †, Mr. Griffith observes, "the schistose strata of the counties of Waterford, Cork, and Kerry, which form the base of the entire district, consist of greywacke and Silurian rocks;" while in a third place he remarks ‡, "I am of opinion that eventually the greater part, if not the whole, of the schistose rocks of the counties of Kerry and Cork coloured greywacke slate on the map, will prove to be Silurian, with the exception of a narrow stripe of black clayslate which extends from the western base of Cahirconree mountain to Ballinguard bay east of Dingle harbour."

This and the preceding quoted passage, when compared with the map of the "Outline," are unintelligible, as the only part of Kerry coloured as greywacke slate is this very band of blackish-grey clayslate, and the only part of Cork so coloured lies south of the line already noticed, as drawn by Mr. Griffith from the coast opposite Dursey island to the head of Bantry bay and thence toward the southern entrance of Cork harbour§.

The blackish-grey clayslate to which Mr. Griffith seems to attach so much importance, is only a member of the general transition series, like the greenish-grey, the reddish-brown, and other coloured slates, all which may be readily found in

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* In the new map this is now accomplished, the limestone being included and merged in what he is pleased to call "carboniferous slate," supported by "yellow and old red sandstone," which assuredly do not exist there.

† Journal of Geol. Soc. of Dublin, vol. ii. p. 81.

‡ *Ibid.*, p. 82.

§ The two passages noticed doubtless have reference to the new map, from the different colouring of which they can alone receive illustration; and here it is to be observed, that the southern part of Cork coloured in the map of the "Outline" as greywacke slate, is now in the new map most gratuitously distributed into parallel bands of what Mr. Griffith is pleased to call "old red sandstone" and "carboniferous slate," to which appellations the strata in question have certainly no pretension. Take, as an example, the region of the Audley copper mines in the county of Cork, described by me in §§ 38, 39, of my Memoir on the south of Ireland, which is coloured by Mr. Griffith as old red sandstone. With equal justice might the region of the copper mines in the county of Wicklow be designated as old red sandstone, since in both cases the constituent rocks are clayslate, quartzy clayslate, and quartz-rock. This is one instance, among many, in which hypothetical views are permitted to supersede the dictates of common sense.

Kerry, Cork, and Waterford, and of which I have given sufficient descriptions elsewhere[*].

In following Mr. Griffith's progress thus far, what is the result? It amounts to this: that with the exception of the eastern part of Waterford lying between the Monavoullagh range and the coast, and the southern part of Cork lying south of the line drawn from near Dursey island past the head of Bantry bay to Ringabella inlet, both of which (coloured grey) are referred to an older transition series or to a Silurian series; all the rest of the schistose and conglomerated rocks, extending from the county of Waterford on the east through that of Cork into and through Kerry on the west (coloured purple), are viewed, and designated by Mr. Griffith in different parts of his communications, as the old red sandstone formation, as a newer transition series, as a Silurian series, (all these three being also called an old red sandstone series), and lastly, as an older transition series; while all the bands of limestone included in these schistose and conglomerated rocks, from the limestone band in the valley of the Bride on the north, to that occurring in Bantry bay on the south, are referred to the carboniferous limestone, although admitted to be in direct association with the rocks by which they are bounded[†].

All this is undoubtedly sufficiently perplexing to a geological inquirer, since it leaves him no secure footing anywhere.

I now turn to the sections; yet another anomaly claims previous attention. All the old red sandstone tracts lying north of the city of Limerick, as those of the Bilboa and Slieve Bloom mountains, similar districts in the counties of Clare and Galway, and others yet more north, are coloured reddish and yellowish brown; and with this I do not quarrel further than that the adoption of two colours to designate the same formation seems superfluous, making that to appear complex which is simple, it being well known that both the colours and composition of the old red sandstone formation are very variable; and as to occasional interstratification on the borders with the carboniferous limestone, that may be very well expressed by words. The anomaly I advert to is this, that the old red sandstone of the extensive range of the Gaultees should be coloured as a newer transition series (purple), while the Slievenamuck and the other ranges more west, which rise in the vale of Limerick (although in fact all connected and belonging to the same formation), are alone coloured as old red sandstone (reddish and yellowish-brown*.)

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[*] Geol. Trans., vol. v., second series, Memoir on the South of Ireland, generally in §§ 8 and 15, and in particular in §§ 7 to 33, and §§ 34 to 41.

[†] These are the legitimate inferences deducible from the "Outline," with its map and the three later written communications. The various modifications and alterations given further to these views, as designated by the new map, I have already adverted to in preceding notes.

A few remarks here upon the construction of sections may not be misplaced. In my sections through the south of Ireland I have endeavoured to combine two objects, namely, to give outlines of the actual forms of the surface presented by nature, and to represent faithfully the relative position of the successive mineral masses, employing as nearly equal scales for heights and distances as the engraving would permit, the difference being only as four to three; but I have not attempted to introduce the differently modified structure of their respective stratification, and for two reasons: 1st, that to do it correctly upon any given transverse line according to scale and measure, would require considerable labour, and even when accomplished might not exactly accord with the results of an examination conducted on parallel lines in different longitudes, in a country where the stratification is more or less fluctuating; 2nd, that unless done correctly, such sections, instead of conveying precise information, tend rather to mislead the judgement. In such cases, it appears more judicious to supply the deficiency by adequate description.

Now, it is for the second of these reasons that I must object to Mr. Griffith's sections. They appear to me in many respects drawn rather according to the conceptions of their author than the occurrences in nature†. As some proof of this, I will in the first instance take the section which is drawn through the south-eastern part of the island, beginning at the valley of the Suire (where it first intersects the carboniferous limestone), and following it to its extremity in the county of Cork. In this section the vertical scale is to the horizontal scale as twelve to one; which necessarily produces great distortions in the relative position of the mineral masses*.

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* In the new map the Gaultees are now also coloured reddish brown, as old red sandstone.

† The same remark applies to the sections drawn in Kerry by Mr. C. W. Hamilton also, but especially so to the section No. 3 in his "Outline of the Geology of part of the county of Kerry," in the Journal of the Geological Society of Dublin, vol. i. pp. 276 to 285, in which there is nowhere drawn a clear distinction between the conglomerates and sandstones of the transition series and those of the old red sandstone formation, the one being confounded with the other. Moreover, in No. 3 section, the old red sandstone and the coal measures are confounded together, the latter (north of Tralee) being made to underlie and break through the carboniferous limestone instead of reposing upon it, and the text in p. 284 is to the same effect. This is the paper referred to by Prof. Sedgwick and Mr. Murchison in the Lond. and Edin. Phil. Mag. in April 1839, at p. 260, and to which I had occasion to advert in the same Magazine for August, 1839, at p. 122. Two later sections in Kerry, given by Mr. C. W. Hamilton in the Lond. and Edin. Phil. Mag. for Dec. 1839, are not only open to the

Let us first consider that portion of the section which lies between the valley of the Suire and the vale of Dungarvan, the latter of which extends westward to the Blackwater. With respect to the relative position of the carboniferous limestone of the valley of the Suire as reposing on old red sandstone, both on its northern and southern sides, there is no question. South of the river, the old red sandstone, which there forms a narrow border, reposes unconformably on the clayslate formation, the former dipping north and the latter south. These relations may be observed in many places, and are well exemplified in passing from the river Suire up the defile which leads to the Glenpatrick slate quarries, about three miles to the east of Clonmell, and which unconformable position is admitted by Mr. Griffith himself†. The same relative position is to be observed upon the range eastward also, and to the south of Carrick, through which Mr. Griffith's section passes; but Mr. Griffith there makes the clayslate of the hill of Carrick, as well as the clayslate extending towards the Monavoullagh range, to dip north, though just contrary to the fact, the dip being south; and by interposing a bed of conglomerate, all north of this, extending to the limestone of the valley, is called an "old red sandstone series," than which assuredly nothing can be more incorrect. For the details I have given, as well as general views affecting this region, I must refer to the 144th section of my Memoir on the east of Ireland‡. I have already remarked that the older stratified rocks on which the Monavoullagh range rests, dip generally south, yet are subject to local inflections to the north; but Mr. Griffith in his section represents the whole of these older rocks as vertical in the centre, from which the strata are made to incline on the southern side to the south, and on the northern side to the north, the latter being, as I have already stated, contrary to the fact. I have also

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same objection, but they are at variance with themselves, the author representing what he terms "old red sandstone" to dip in fig. 1 generally south, and in fig. 2 generally north. They are also at variance with his former sections, given in the Journal above referred to, but especially so with the matters of fact. An unrestrained indulgence of fancy, with a loose application of the terms *Cambrian, Silurian, old red sandstone,* &c., can tend little to the advancement of Geology.

* The deceptive effect of such sections has been ably shown by Mr. De la Beche in his Sections and Views illustrative of Geological Phænomena.

† Journal of Geol. Soc. of Dublin, vol. ii. p. 86.

‡ Geol. Trans., vol. v., first series, part 1. 1819.

observed that the Monavoullagh conglomerate range approaches so far towards the vale of Dungarvan, that the schistose and conglomerated rocks lying south of the Monavoullagh range, and north of the carboniferous limestone of the valley, do not reach to the surface to any great extent; but Mr. Griffith brings into his section as south of the Monavoullagh the older stratified rocks which lie west of the latter, namely, such as we traverse north of Lismore when proceeding from the Blackwater toward the Knockmildown chain, and which in fact are merely the western continuation of the older stratified rocks that are exposed on the eastward of the Monavoullagh range. In the series of strata north of Lismore, and which he designates as the newer transition slate series*, there occur, he observes, in some localities, abundant marine exuviæ, and even vegetable remains, e. g. calamites; and he adds that it is possible the whole may belong to the Silurian system†; and yet this series is represented as in direct association with the unquestioned carboniferous limestone of the Blackwater valley, which is quite contrary to my observations‡. The appearance of vegetable remains here is interesting, as bearing analogy to a similar occurrence in the transition series at Dunmore in the county of Kerry, observed by the late Mr. A. Nimmo, and as elsewhere remarked since by Mr. Griffith, which will be noticed in the sequel.

The ridge intervening between the limestone of the vale of the Blackwater and of Dungarvan, and that of the river Bride, is said to partake of a similar composition to that of the northern side of the Blackwater near Lismore, namely, that of green clayslate, yellow sandstone, and coarse red slate, forming in the centre of the ridge an anticlinal axis, the sandstone containing calamites§. The anticlinal axis I have not seen, the dip which I observed being throughout to the south; while in the composition of the ridge I found moreover numerous varieties of greywacke, slate, quartz-rock, and sandstone, frequently of a reddish-brown hue, also of a yellowish and whitish cast, and many coarse conglomerates, containing fragments and pebbles of considerable size||.

Having arrived at the valley of the Bride, it is time to advert more directly to the narrow stripe of brownish-yellow,

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* But which in the new map is now called "old red sandstone."

† "Outline," p.7, note, and Eighth Report of the British Association, p. 82.

‡ Both in the valley of the Blackwater and that of the Suire, Mr. Griffith appears to me to have confounded together the slate clay of the carboniferous limestone with the clayslate of the transition series.

§ Eighth Report of the British Association, pp. 82, 83.

|| Memoir on the South of Ireland, Geol. Trans., vol. v., second series, § 26.

which (as indicative of old red sandstone) is made everywhere to surround in symmetrical order the isolated bands of limestone which occur in the south of Ireland, (save and except those of Bantry bay, Skibbereen, and Courtmacsherry *), and which is also introduced in the vale of the Blackwater and Dungarvan, as underlying the limestone there. Old red sandstone is not within my cognizance in the positions indicated; and the occurrence of a grey or yellow sandstone with some *calamites* is another question. For I contend that the old red sandstone formation, in the legitimate sense of that term, nowhere passes to the south of the river Blackwater; and the two districts of that formation which occur in Kerry, namely, in the Slieve Meesh range and Kerry head, lie north of that parallel.

Doubtless influenced by similar systematic views, Mr. Griffith represents the whole series occurring between the valley of the Suire and Cork head as consisting of a regular sequence; which taken in an ascending order between the Suire and the Blackwater, is said to be composed of greywacke and slate, conglomerate, quartz-rock, red slate, yellow sandstone and sandstone-slate, green slate and limestone; and that in traversing the country further south, between the vale of the Blackwater and Cork head, we meet only with a repetition of the same succession between the different bands of limestone encountered in our progress†. That all these rocks are to be found in the sequence there is no doubt; but I know of no such regularity of order as is proposed, and excepting wholly the old red sandstone formation from the series, south of the Monavoullagh range. I must also remark that the disposition of the rocks as given does not correspond with the results of my researches, and that anticlinal and synclinal lines appear to be introduced where there is no proof of their existence. I advert in particular to the three bands

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* But in the new map, these limestones are also bounded by the brownish-yellow stripe, which likewise is made a border to what is designated as reddish-brown "old red sandstone," wherever the latter is arbitrarily introduced, e. g., at the heads of Kenmare and Bantry bays, &c., where certainly no old red sandstone has been seen by me. Again, on the western side of the Lower Lake of Killarney, extending westward on the south side of the river Laune, Mr. Griffith confounds with the old red sandstone, the red conglomerate, sandstone, and red slate of the transition series, which prevail in that quarter, and are well exposed in the pass to Dunloe Gap, all dipping south, yet subject to some inflections[a].

† Proceedings of the Geol. Soc., May 22, 1839, p. 137; and Journal of the Geol. Soc. of Dublin, June 13, 1839, pp. 86 to 88.

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[a] Geol. Trans., vol. v., second series, Memoir on the South of Ireland, § 10.

of limestone, with their bounding schistose and conglomerated rocks, which in their range traverse Cork harbour, and which according to my observations are all in parallel position, with a dip to the south; but Mr. Griffith represents them as forming successive troughs, with corresponding anticlinal and synclinal dips, of which I certainly have no knowledge. In one essential point, however, we are agreed, namely, that these parallel bands of limestone show, wherever exposed, an interstratification with the adjacent clayslate, both rocks exhibiting in general the same organic remains; which are also contained in the clayslate still further south, on the north side of Ringabella inlet, and again on the same parallel to the west, adjacent to the road leading from Cork to Brandon. But in the clayslate and sandstone subordinate to it are found also vegetable remains, showing a further analogy to similar rocks north of the Bride, to those north of the Blackwater, and at Dunmore head in Kerry. Mr. Griffith has given us a list of transition fossils which he had collected at Ferriter's cove; and another list of those he had obtained from the limestone and clayslate of Cork harbour, at Rostellan, Rinniskiddy, &c. Of the latter Mr. Sowerby has pronounced some species to be the same as occur in the carboniferous limestone, and others to resemble such as are found in the older stratified rocks of Devonshire; and it might be added also such as occur also in Ferriter's cove and in the Silurian system, e. g., *Leptæna* (*Producta*) *lata*, *Leptæna depressa.* I may also add, as a further analogy, that in the older stratified rocks of Devon and Cornwall, vegetable remains are likewise met with; e. g., in Devon as observed by Major Harding, the Rev. D. Williams, Professor Sedgwick, and Mr. Murchison; and in Cornwall by Professor Sedgwick, as noticed by Mr. Ansted in his paper on *Endosiphonites* * (the *Clymenia* of Count Münster.)

Every addition to our knowledge is valuable, and I trust Mr. Griffith will continue to employ hands in the south of Ireland in collecting fossils, for which he possesses so many opportunities.

I have now to advert to Mr. Griffith's section in the Dingle peninsula, in the west of Kerry, which in no part exactly accords with my observations. In this section the vertical scale is to the horizontal scale as five to one. But the portion which more immediately claims attention is that which extends from the summit of the old red sandstone of the Slieve Meesh range † to the carboniferous limestone in the vicinage of Castle island. The former is represented as constituting merely a cap or sheet formed upon an inclined plane from west to east, the strata corresponding, and succeeding each other in that direction to the junction with the carboniferous limestone. I know of no such arrangement. On the contrary, the strata of the old red sandstone are accumulated to a great depth, and certainly in some quarters at least to the level of the sea, if not deeper, being disposed in a gently arched form from north to south, as may be well observed in the defiles and glens which penetrate fron the north into the interior of that mountain range*. In this series I have not observed a general dip to the east, the strata even in the most eastern quarter (including beds of red clay and red slaty clay) still preserving the flat arched arrangement from north to south; and it is only at the eastern foot of Slieve Meesh that other beds appear dipping to the east of south, and which from their dissimilarity altogether to the old red sandstone formation of the Slieve Meesh range, I could only view as a protruding portion of the subjacent transition rocks continued from the west. I know of no organic remains to invalidate this conclusion. The *Spiriferæ*, *Productæ*, *Terebratulæ*, and *Crinoidea* that I met with were too indistinct to admit of determining the species, but I am mistaken if there be not an *Orthis* among the number; while the *Favosites* which I noticed I apprehend to be *F. fibrosa.* This eastern foot of the Slieve Meesh is represented by Mr. Griffith as composed of a succession of beds of yellow and grey quartzy sandstone, dark grey clayslate, sandstone, dark grey clayslate, grey quartzy sandstone, alternating limestone and greenish clayslate, against which the carboniferous limestone is exhibited as abutting in unconformable position †. The sandstone is stated to contain calamites, and the general series to abound with the casts of fossils, whose distinctive characters, it is admitted, it is difficult to recognise, but among them are *Productæ*, *Spiriferæ*, *Terebratulæ*, *Crinoidea*, and *corals*; and the upper beds of the greenish grey clayslate are said to be identical with those which occur alternating with limestone in the peninsula of Muckruss. I confess I have not traced any such uninterrupted succession as is here described, the country in general being well covered up; but if we suppose it to be correct, there is no direct proof that such succession belongs to the carboniferous series. From the quarry which I have described at Riversville on the right bank of the Maine *, a low ridge crosses that river and extends some distance beyond it, in which I could discover traces only of the greywacke formation, and around which the carboniferous limestone of the vale appears to sweep on the north, east, and south, in nearly horizontal position, wherever exposed in the adjacent quarries †. For these several reasons my view necessarily differs from that taken by Mr. Griffith.

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* Cambridge Phil. Trans., vol. vi. 1838.

† Called Cahirconrée by Mr. Griffith, but which denomination I conceive to apply strictly to the mountain range stretching to the west of Bartrigoun.

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* Memoir on the South of Ireland, §§ 10, 13, 49, in Geol. Trans., vol. v., second series.

† Journal of Geol. Soc. of Dublin, vol. ii. pp. 82, 83, and the lower section in plate iv.

From the preceding it will be inferred that I do not consider Mr. Griffith's representations as in any respect invalidating the conclusions to which I have been led, as exhibited in my memoir on the south of Ireland. The day is past when it might be authoritatively pronounced that such and such a limestone is carboniferous merely because it contains some fossils that are common to the latter. I have entered at some length into this subject in my memoir on the south of Ireland and in the Lond. and Edin. Phil. Mag. for August 1839, to which I beg to refer. But I cannot avoid noticing in this place, as a case in point, the communication made by Mr. Austen to the British Association, at Birmingham, in August, 1839, respecting the fossil remains of the limestones and slates of South Devon ‡, which in its general views so well corresponds with the tenor of my publication in the Lond. and Edin. Phil. Mag. of the same month, although our respective observations and inferences were made independently of each other. Mr. Austen conceives that a great identity of species can be established between the *Radiaria*, *Mollusca*, and *Crustacea*, of a portion of the Rhine and those of South Devon; and he states that both districts present many forms of animal structure, such as in this country we should call carboniferous, and that of *forty* species which were enumerated, some were hardly, and some not at all, to be distinguished from those of our mountain or carboniferous limestone. Mr. Austen considers as the geological equivalents of the slates and limestones of South Devon those of the Rhine and Eifel, and that strata in the south of Ireland are of the same age; observing also that many of the same fossils occur at Nehou and St. Sauveur in Lower Normandy. He remarks also that *old red sandstone* is an unfit name to designate the limestones and roofing slates of South Devon, or the white sandstones of Lower Normandy and Brittany. To which I may add, that it is also inapplicable to the older stratified rocks of Ireland.

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* Memoir on the South of Ireland, § 13. † *Ibid.*, § 51.

‡ See Athenæum of August 31, 1839, p. 661.

I may here appropriately advert to the remarks made by Capt. Portlock, R.E., late President of the Geol. Soc. of Dublin, on Mr. Griffith's arrangement of the strata in the south of Ireland, as well deserving of the attention of the latter*. He observes, that Mr. Griffith's transference of those strata which were formerly designated as of the transition epoch to the old red sandstone, must be considered as springing from the generalizations in Devonshire of Professor Sedgwick and Mr. Murchison. To those generalizations I have objected in the paper referred to above in the Lond. and Edin. Phil. Mag. for August 1839; and I have already stated that Mr. Austen started similar objections in the same month. But we may proceed a step further, and show that evidence is not wanting to prove that the older stratified rocks of Devon and Cornwall belong to an ancient transition series. The genus *Clymenia*, discovered and determined by Count Münster in the Fichtelgebirge, occurs also in Cornwall and Devon according to the observations of Mr. Ansted, Professor Sedgwick, Mr. Murchison, Mr. De la Beche, and Professor Phillips. In the Fichtelgebirge the Clymeniæ are accompanied by *Goniatites* also, and Count Münster enumerates fourteen species of the former and twenty-six species of the latter, together with one hundred and eighteen species of other fossils; namely, *Trilobites* 14 species, *Serpula* 1, *Bellerophon* 3, *Orthoceras* 22, *Gasteropoda* 31, *Conchifera* 43 (among which are species of *Orthis* and *Terebratula*, but no *Spirifera*, according to M. Von Buch †), *Crinoidea* 4. This tract is referred by Count Münster to an ancient transition series, the *Clymeniæ* being confined to it, and not occurring in the upper strata of the country, namely, in the carboniferous limestone‡. Von Buch also considers this tract and the environs of Prague as belonging to the more ancient strata of the transition epoch, and as being perhaps the oldest of that class to be found in Germany§. And M. Beyrich makes the general observation, that *Clymeniæ* appear restricted to the older transition strata, not having hitherto been met with either in the later transition limestones or in the carboniferous limestone; while *Goniatites* on the other hand, he remarks, are widely spread in strata both of the transition and carboniferous epochs, extending in the latter series into the coal formation*. Now, at South Petherwin in Devon, Professor Phillips has ascertained that at least four species of *Clymeniæ* occur, undoubtedly belonging to the same group as is found in the Fichtelgebirge, and among these one is identified as the *Clymenia lævigata* of Count Münster. With these he notices also two species of *Goniatites*, one of which belongs to the same group that occurs in the Fichtelgebirge†. But we need not rest here: other decided transition fossils are met with in the older stratified rocks of Devon and Cornwall, e. g., various species of *Terebratula* and *Atrypa*, namely, such as occur in the transition tracts of Sweden, in Gothland, &c., together with species of *Orthis*, *Pterinea*, *Leptæna lata*, &c., for the general list of which I refer to the work of Mr. De la Beche already cited, as well as to the observations of Professor Sedgwick and Mr. Murchison‡. I cannot doubt that more extended researches in Devon and Cornwall will vastly increase the list, and yet bring to light many facts of high interest§.

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* See pp. 25 to 27 of the President's Address, February 14, 1839, in Journal of Geol. Soc. of Dublin, vol. ii. To Capt. Portlock we are indebted for the discovery of a small transition district in the county of Tyrone, which embodies several fossils common both to the Silurian region and to certain transition tracts in North America, *Ibid.* pp. 28, 29.

† *Bulletin de la Société Géologique de France—Séance de Mars*, 1836. Tome vii. p. 156.

‡ Bayreuth, 1832; and *Annales des Sciences Naturelles*. Séconde série. Tome ii. 1834.

§ *Bulletin de la Société Géologique de France—Séance de Mars*, 1836. Tome vii.

While on the subject of the distribution of organic remains, I will add a few remarks on that of *Goniatites* in particular, in transition and carboniferous strata. If we compare together the labours of M. Von Buch, Count Münster, M. Beyrich, and Professor Phillips, on this subdivision of the tribe of the Ammonites, the general result is that in the older transition rocks the Goniatites met with possess a *simple* dorsal lobe, whilst those which occur in the carboniferous series have a *divided* dorsal lobe. If this observation were rigidly exact, it might be adopted as a distinctive character between strata of the transition and carboniferous epochs respectively; and it might also be inferred that limestones which contain Goniatites, some of which possessed a simple and others a divided dorsal lobe, might be considered as occupying an intermediate station, that is, as belonging to the later portion of the transition series. But in neither case does the rule appear to hold without exceptions. Thus, of the 26 species of Goniatites noticed by Count Münster in the Fichtelgebirge, the 22 (four being doubtful) which are strictly defined and figured, have all a simple dorsal lobe; but two other species are figured by M. Von Buch, as derived from thence, with a divided dorsal lobe, namely, *Ammonites inæquistriatus*, tab. ii. fig. 10, and *A. semistriatus*, tab. ii. fig. 12*. Of 8 other species described and figured by Von Buch, and referred to but not figured by M. Beyrich, 5 are found in transition tracts, and four of these have a simple dorsal lobe, and one a divided dorsal lobe; while of the three species found in the carboniferous series, two have a simple and one a divided dorsal lobe. M. Beyrich has described 18 species of Goniatites, and 14 of these are figured, but in 12 only are the dorsal and lateral lobes given, and of 10 of these which are derived from transition tracts, five have a simple and five a divided dorsal lobe; while in the two found in the carboniferous series the dorsal lobe is divided. M. Beyrich enumerates (including the eight new species designated by himself) altogether 42 species of Goniatites, the greater number being derived from the researches of Count Münster and M. Von Buch, and a few from the descriptions given by Professor Goldfuss and MM. Martin and Sowerby. Of this list four or five species only are referred to the carboniferous series. Professor Phillips enumerates 33 species of Goniatites as occurring in the carboniferous system of Great Britain and Ireland†. Of these he has represented the dorsal and lateral lobes of 24 species, out of which 21 possess a divided dorsal, and three a simple dorsal lobe. Of the 33 species, 30 are first made known to us by Professor Phillips; only three of them having been previously noticed by authors, namely, *Goniatites sphæricus* (Mart.), *Gon. Listeri* (Mart.), and *Gon. Henslowi* (Sow.)

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* *Ueber die im Rheinischen Uebergangsgebirge vorkommenden Goniatiten*, p. 22 (Beiträge, 1837). See also a translation of this Memoir in the Annals of Natural History for March and May, 1839.

† See Mr. De la Beche's Geological Report of Cornwall, Devon, and West Somerset, pp. 59, 60.

‡ Proceedings of Geol. Soc., May 1838; and Lond. and Edinb. Phil. Mag., April 1839.

§ It is remarkable that Prof. Sedgwick formerly considered the strata containing the fossils that have been indicated (and which include also *Trilobites* and *Orthoceratites*), as the lowest fossiliferous rocks in Devonshire and Cornwall[a]. And yet, according to a later view, originating in the suggestion of Mr. Lonsdale, Prof. Sedgwick and Mr. Murchison now include all the older stratified rocks of Devon and Cornwall under the head of *old red sandstone*; and to this view it appears Mr. Greenough has been led to conform, as shown by the colouring of those tracts in the new edition of his Geological Map of England and Wales. Viewing the subject, as I do, with the eyes both of a geologist and a miner, I must here, with all respect, repeat my protest against any such generalization, such appearing to me contrary to all analogy, and as only tending to confound subjects essentially distinct. The designation as *Devonian* might not be equally objectionable, as that does not necessarily bind us to more than what we actually encounter within the limits of the older stratified rocks of Devon and Cornwall, respecting which, however, we yet require much additional information.

[a] See Mr. Ansted in Cambridge Phil. Trans., vol. vi. p. 422, 1836; and Prof. Sedgwick in Proceedings of the Geol. Soc., vol. ii. p. 683, May 1838.

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* Von Buch, *Ueber Goniatiten*, in the Transactions of the Royal Academy of Sciences of Berlin, December 15, 1831.

† Illustrations of the Geology of Yorkshire, part ii. 1836. Mountain Limestone District.

I have no doubt, that Professor Phillips, who is so well qualified for the task, will give due attention to this subject in Devon and Cornwall; and it will be interesting to learn in what degree the Goniatites of the carboniferous limestone in North Devon, near Barnstaple (e. g., at Swimbridge, where they abound), differ from those found in the transition districts of South Devon and Cornwall. The same subject deserves attention in Ireland*.

To M. Von Buch we owe the first precise distinction between Nautili and Ammonites†. The range of the former extends, it is well known, from the oldest to the most recent of the fossiliferous strata, the genus being still in existence. The Ammonites, on the other hand, though equally ancient in origin, do not in their range pass beyond the limits of the cretaceous group. To the same distinguished naturalist we owe the distribution of the Ammonites into 14 families‡, the first and oldest of which, the *Goniatites*, are characteristic of the transition and carboniferous epochs, not extending beyond the coal formation; the second, the *Ceratites*, appear confined to the muschelkalk; while the remaining 12 families are distributed through the series of formations extending from the lias to the chalk inclusive. But we are indebted, primarily, namely, in 1828, to Professor Bronn of Heidelberg, for the important observation so conducive to the distinction of formations, that no Ammonites with denticulated lobes have been found in strata of the transition and carboniferous æras, such being confined to the formations of a later origin. This remark was publicly made known by Von Buch in 1829, nearly at the same time that M. Elie de Beaumont was preparing to announce a similar observation made by himself; and the remark was shortly after confirmed and generalized by Count Münster§.

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* Count Münster considers his *Goniatites ovatus*, found at Gottendorf and Schleitz in the Fichtelgebirge, to be the same as the *Ellipsolites* (*Nautilus*) *ovatus* of Sowerby, which occurs in the Cork band of limestone. And he inquires whether some other species described as Nautili by Mr. Sowerby might not prove to be Goniatites.

† *Annales des Sciences Naturelles*, tome vii. première série, 1829.

‡ *Annales des Sciences Naturelles*, tome viii. 1829; and Transactions of the Royal Academy of Sciences of Berlin, April 1, 1830, *Ueber die Ammoniten in den älteren Gebirgs-Schichten.*

§ Von Buch, *über Goniatiten*, December 15, 1831, in the Transactions of

M. Deshayes, however, has lately remarked that *true Ammonites* (with denticulated lobes, I presume) have been found in strata more ancient than the coal formation, namely, in the environs of Tournay *.

In reference to my successive memoirs on the east and south of Ireland, I will here add the general remark, that it has been my endeavour in both to embody in few words the results of practical experience and observations conducted at intervals in that country during a lapse of more than forty years; and in laying them before the public, it has been my object, in both cases, to exhibit through the medium of condensed abstracts a compendious view of the relations of the tracts described, thus relieving the reader from the labour and tedium of passing through the progressive steps of extended researches. That a minute examination of the ground which I have trodden may lead to further discoveries, particularly in respect of the distribution and number of the organic remains, I am far from disbelieving, and I shall be ready to hail their appearance with pleasure; but as in the development of the main relations of the mineral masses I have endeavoured to be exact, I may be excused if I do not anticipate any very great accession to our knowledge in the latter respect.

[To be continued.]

L. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron. By* Dr. C. SCHAFHAEUTL, *of Munich*.

[Continued from p. 50.]

I SHALL now describe another action of acid bodies on iron, which has much resemblance to that which is exerted by the action of sea water on it.

I poured over a parallelopipedic fragment of tilted but very tender and indifferent razor-steel, in a tea-cup, concentrated hydrochloric acid. After the action had for the most part ceased, I changed it for fresh acid, and all visible action of this fresh acid had ceased the next day. The cup was preserved in this state without being disturbed for nearly two months. After this lapse of time I found the fragment, apparently unchanged,

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the Royal Academy of Sciences of Berlin, published, with the preceding Memoir, in 1832.

* *Bulletin de la Société Géologique de France—Séance de* 19 *Fevrier*, 1838.

surrounded by a ring of black sediment, about a quarter of an inch distant from it; this ring, which was about three-quarters of an inch broad, was formed of rays tending towards the centre of the cup and having scarcely any connexion between them; the opposite end of the fragment, which lay in the magnetic meridian, was covered with fibrous bundles of the black sediment like the pole of a magnetic needle dipped into iron filings. I decanted the liquid in order to wash the remaining fragment and to free it from its black covering; but on touching it, I found it converted into a soft black plastic substance. Exposed to the air, it soon changed its black appearance to that of brown, and attracted very rapidly moisture from the atmosphere, until it became nearly liquid. Hydrochloric acid heated did not appear to act upon it; nitric acid first evolved some nitric oxide gas, converting it into a brown pulpy mass of a greater volume; nitro-muriatic acid at a boiling heat dissolved it almost completely; it melted before the blowpipe, after disengaging copious fumes, into a globule of silicate of iron, strongly acting upon the magnet. Twenty grains of it burnt with chromate of lead and chlorate of potassa, developed carbonic acid gas, azote, and water. The greatest part consisted of iron, but only a trace of silica was separated, too small to be weighed.

This leads me to the consideration of a paradoxical phænomenon consisting in the insensitiveness of iron to the action of nitric acid under certain circumstances; a phænomenon which has hitherto excited so much attention, and has been attributed to an electrical inactivity of the iron itself, or to a film of hydrogen or binoxide of hydrogen protecting the surface of the iron. We have seen that when a considerable mass of iron is attacked by acids a black skeleton always remains, which very quickly changes colour when exposed to the air. We have further seen, that cast iron, which is a mixture of carburet of iron and silicon, is even attacked by hydrochloric acid on the surface only; that is to say, that as soon as the surface is coated with a pellicle of that black or brown residuum which we shall soon more accurately describe, all action of the acid and all evolution of hydrogen apparently cease; but we learn from the latter experiment that in time an invisible action always takes place, by which the mass of iron is slowly decomposed, and the remainder assumes a different shape according to the concentration or the different chemical properties of the acid itself.

When the liquid in which the iron is immersed contains free acid, the decomposition of water stops as soon as the iron is covered with a pellicle, which prevents the close contact of a mass of iron with a mass of acid, and allows only a separated molecular contact between the iron and the acid.

When the liquid contains a great deal of *free acid*, after the evolution of hydrogen has ceased, and the air has free access to it, the residuum, after the following invisible action has ceased, will be more or *less plastic.* If the liquid contains very little free acid and the air is excluded, a basic protochloride of iron is formed by the liquid attacking only the carburets of iron, leaving the sulphurets of iron, the carburets of silicon, and the higher carburets of iron, as a skeleton behind; a similar effect takes place when the solutions contain only chlorides of magnesium and sodium. When the air has access, the metal becomes oxidized by the air contained in the liquid, and forms invariably basic salts with the chlorides in solution, which, when washed away (as is the case when iron is immersed in the sea), the skeleton of carburets of iron, silicon, &c. remain behind, which, from its highly spongy nature, condenses and compresses in its pores, when exposed to the atmosphere, atmospheric air in great quantity; part of the iron is oxidized, and when the iron is in very large masses, the temperature is generally increased to the boiling point.

The action of oxygen, by the presence of acid and water, on a solution of chlorides, presents one of the cases of chemical decomposition by which two or more chemical bodies enter into a new combination by the sole presence of another chemical body, which during the whole decomposition remains entirely unchanged. Berzelius explained this by a reference to a new force, which he called *catalytic force.* I shall, in another place—in my theory of "*the final action of chemical forces,*"—endeavour to demonstrate, that in fact *no chemical combinations of two bodies* takes place without the catalytic presence of a third, or in fact, that *all chemical combinations* and decompositions are produced by *catalytic force.*

All cast iron, when dissolved in acids, provided the fragments retain their form after the action has ceased, show, after being washed on a filter, the same property of condensing air in their pores, and of increasing the temperature by the oxidation of the iron left in a lower state of carburation. If the liquid contained very *little free acid*, very soon a basic salt is formed; and where the access of air is prevented during the progress of the solution, the remainder, when placed on a filter, oxidates extremely rapidly, and a hydrate of oxide of iron goes with the water through the filter, even after a week's incessant washing. Similar and often highly-interesting results are obtained by a peculiar sort of chemical action, where one body slowly separates from others, by combining with a third, without setting the remaining parts of the compound in perfect freedom, and enables them to replace the lost body by another chemical equivalent, or to allow their molecules to follow their own attractive tendency, and to form, after being set free, a new individual body for themselves.

The action of bodies in their utmost molecular division is quite otherwise than when their molecules had been sufficiently moveable to follow, after separation, their own mutually attractive forces and to neutralize them in one centre.

A body whose chemical force had been neutralized by another, now deprived of that neutralizing body, without having sufficient freedom of its molecules to follow their forces of mutual attraction set at once at liberty, is in the state of a body surrounded by an atmosphere of electricity and in a different relation and condition of tension to the surrounding bodies.

This relation is visible in all bodies placed under such circumstances in contact with others: the instance of the reduction of iron at a low temperature by a current of hydrogen is well known; the following fact seems less so. A lump of malleable, hammered, or rolled iron raised to a white heat, over which water is poured, exerts a very feeble decomposing power on the water. Let us now take a similar mass of iron out of the puddling furnace, just ready to be placed under the hammer, and pour a basin of water upon it. No hissing noise is perceptible, no generation of steam visible*, and the water is at once decomposed, the flames of hydrogen enveloping the white-hot ball and rising very often to a height of four or five feet. I need scarcely remark, that the iron taken out of the puddling furnace is in the same state as iron treated with acids; a skeleton of each grain of cast iron remains, with this difference only; that with iron treated with acid, electro-positive metals are partially removed from the compound; but by treating cast iron in a puddling furnace, the relative electro-negative bodies are taken instead, without destroying the mechanical texture. A few strokes of the hammer destroy this property of the white-hot iron.

When a constituent of a decomposed body is separated from another, in a solid state, by chemical action, the so-separated body is never left in the same state as that in which it existed when forming a corresponding part of another body; and each body set at once free by chemical action, with perfect liberty of its molecules, combines with other bodies or takes a new form of very different quality, under which all precipitates, separated from the solvent liquid, exist.

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* On the subject of the subitaneous formation of steam by red-hot or white puddling slag, oxide of iron, and malleable iron, I mentioned some curious instances in my paper "On the conversion of water into steam at the higher degrees of temperature," &c., in the Mechanics' Magazine, vol. xxx. pp. 138, 294, and 339.

In my theory of "*The final action of chemical forces*" I shall endeavour to show, that one *of the first principles* of the formation of the productions of animated life is in fact only an action of the before-mentioned chemical forces *under different circumstances*, which may be expressed in the short sentence: *extremely fine division* (molecular separation), during an uninterrupted motion.

When very gray cast iron is treated by hydrochloric acid, the residuum is a white substance, mixed with black scales of graphite, which Karsten considers as being mechanically mixed with the mass of the iron before solution, and contrasts it with the black precipitate which separates from white cast iron, which has a dull earthy appearance; but when this so-called dull and earthy powder of white cast iron is produced by a tolerably diluted acid, and viewed, while still in suspension in the liquid, under a certain angle of vision, all the small particles appear to consist *likewise of scales*, as we shall hereafter perceive, which reflect the light with the same power as the scales of graphite, their only difference from them consisting in a different chemical composition. They are therefore more easily decomposed, and apt from their softer nature to adhere rather closer to each other, in which state their scaly or foliaceous nature is not so easily to be discovered.

To ascertain the relative combinations of the elementary constituents of the different sorts of iron of commerce, the treatment by means of acids is indispensable, and I prefer the hydrochloric and nitric acids to all others.

The quantity and in part the quality of the different combinations obtained by treatment of the iron with hydrochloric acid, depends a great deal on the density of the bodies brought into chemical contact, or, in other words, on their specific gravity.

If irons of different specific gravity are dissolved in hydrochloric acid of the same specific gravity, the residuums will have a different appearance.

If filings of Swedish iron, for example, double bullet iron of the specific gravity 7·810, is dissolved in hydrochloric acid of the specific gravity of 1·169 in comparison with English iron of the specific gravity 7·60, the residuum of Swedish iron will appear in distinct heavy grayish scales, that of English iron in brown flocculent *rags*, partly remaining on the bottom of the phial and partly suspended in the liquid. But if the specific gravity of the acid is increased, the residuum of the Swedish iron will assume the same appearance as the English, and so in the reverse case; this circumstance explains why anchors made of English iron, although tougher than those made of Swedish iron, are almost twice as soon destroyed by the action of sea water.

The same rule holds good, when iron, with other chemical agents, is treated in the dry way. For instance, at the same degrees of heat as those in which malleable iron of the specific gravity 7·4 has combined with the greatest quantity of carbon necessary for it to become liquid,—at the same degree of temperature Swedish iron of the specific gravity 7·7 (called Hoop L), or 7·8 (called double bullet), just begins to crystallize, forming steel, and sinking down to a specific gravity of 7·5.

The quantity of the residuums of iron of the same description bears a very remarkable relation to the specific gravity of the acid, as is evinced by the following table, showing the different quantity of residuums of 35 grains of Welsh iron from the Maesteg iron-works, specific gravity 7·407.

Specific gravity of the acid.	Residuum.	Sulphuret of Lead.
1·160	6·770	1·73
1·140	9·244	1·71
1·103	13·711	1·703.

Mottled iron from the forges at Alais, departement du Gard, in the south of France:

Specific gravity of the acid.	Residuum.	Sulphuret of Lead.
1·16	7·51	6·06
Acid very much diluted	11·42	0·6475
Acids still further diluted	21·49	1·015

In the fourth example I poured water over the fragments of iron in a retort, the beak of which was connected with two bottles of a solution of nitrate of lead, and I dropped only into the retort so much hydrochloric acid as to cause a very moderate evolution of hydrogen. In the last sample I put only a few drops of acid into the retort, just sufficient to cause a very slight attack of the iron. The third column of all the samples contains the quantity of sulphuret of lead obtained. The evolution of gas continued for several days, and when it ceased, the addition of acid had no further visible effect*.

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* As I repeated the last experiment but one, I put some fragments of the cast iron in the upper part of the beak of a new retort in order to witness the effect of the evolving gas on iron fragments. The next day I found the fragments converted into a black crumbly and rather unctuous mass, soaked with a yellowish liquid, which fastened it to the glass. On nearer

By treating iron with *diluted* hydrochloric acid, the sulphur escapes *almost* entirely as sulphuretted hydrogen; the same is the case with antimony escaping as antimoniuretted hydrogen. On the contrary, scarcely any portion of the arsenic escapes with the hydrogen.

The case is different when iron is treated with nitric acid. When fragments of iron are treated with nitric acid of such a specific gravity that the iron is moderately attacked in a retort whose beak is connected with a solution of carbonate of barytes and acetate of copper or lead, and only a slight evolution of binoxide of nitrogen takes place, the nitric oxide is then first absorbed by the air in the uppermost part of the retort, and a partial vacuum is produced which makes the liquid rise in the beak of the retort several inches. A few hours afterwards pure azote is evolved, acting neither on the carbonate of barytes nor on the acetates or nitrates of metals. Shortly after, the evolution of gas again ceases, and after the action of the acid on the iron stops, a new partial vacuum is produced and the liquid is found to stand in the beak of the retort several inches above its level.

For example, I took 20 grains of dead gray cast iron from the forges of Creuzot, departement de Saone et Loire, treated it with hydrochloric acid specific gravity 11·5, and left residuum

inspection I found those lumps covered with stalks of a black vegetation, exactly like the mould on ink or sour starch paste, and differing only in colour; the stalks were from about a half to three-quarters of a line in height, and terminated with a little knob on the top. The next day the little knob on the top assumed an orange red, and on further exposure to the air, the stalks became of the same colour.

Viewed through the microscope, these little mosses appeared opake, of a varnished yellow colour like the petals of Everlasting, and having a long curved cylindric capsule on the end. The capsule was alternately expanded and contracted very similar to the duodenum of animals, and curved into a circle, so that the end almost met the point when the capsule was attached to the stalk; and when viewed under the microscope superficially, the knobs appeared to consist of a perfect ring fixed to the stalk. I found only one stalk, which shot forth two of these capsules from the same point, curved up just like the horns of a ram. No operculum could be discovered; the end of the capsule was found perfectly rounded like a globule with rather a narrow neck, and when pressed with a fine needle, the capsule burst, emitting extremely fine seeds, exactly like the genus *Phascum*. No traces of leaves could be detected, unless we reckon some entangled and interwoven black filaments on the surface of the decomposed iron. Not being sufficiently acquainted with the cryptogamic branch of botany, I am not able to decide on the generic character of these small vegetations.

On the retort being packed up in moss and afterwards washed with rain water, the seeds of the moss must have been derived from one of these sources; but the most extraordinary circumstance is their becoming fixed in these decomposed fragments of cast iron, serving as a very fertile mould, and growing rapidly in hydrochloric acid gas and sulphuretted hydrogen.

= 7·08 grains. The same quantity of the same iron treated in the above-mentioned manner with nitric acid of 1·10 specific gravity left residuum 3·24 grains; no trace of sulphuric acid could be discovered in the liquid. By boiling the remainder in a platinum crucible with strong nitric acid, the greatest part of the sulphur was separated, which adhered slightly as a yellow riband to the side of the crucible, and no traces of sulphuric acid were to be found in the solution. The sulphur thus separated from 1·55 grains of the residuum weighed 0·180 grains, and this burnt on a platinum foil left 0·100 sulphuret of iron. That which the nitric acid had dissolved during the boiling was precipitated with caustic ammonia and consisted of

Iron	0·443
Phosphorus . . .	0·155
And left residuum .	0·31
Humine	0·33

We see therefore that the sulphur had been in combination with the iron, approaching in this formula $F^3 S$, that is in a certain well-known state of chemical proportions.

Another remarkable circumstance is the difference of the products of solution according to the difference of the specific gravity of the acid, which throws a light on several chemical products, which arise from different degrees of concentration of chemical agents, and the varying products obtained by the various degrees of heat.

In hydrochloric acid we have a juxtaposition of atoms of chlorine and hydrogen intermixed with atoms of hydrogen and oxygen; and the interposition of a certain quantity of atoms of water betwixt a certain number of atoms of chlorine and hydrogen, alone determines how many atoms of the iron shall combine with chlorine to form protochloride of iron, and how many atoms of hydrogen shall form compounds with carbon and azote.

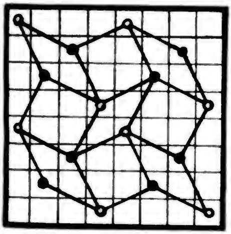
The description of the residuums left by treating iron with acid was always a matter of great speculation. Berzelius says the remainder of the solution of iron in sulphuric acid was black, in hydrochloric acid gray, and sometimes white.

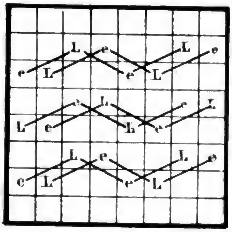
The colour of the residuum, as shown, depends partly on the specific gravity of the iron and partly on its chemical composition.

The residuum of white cast iron prepared with coke as fuel, in hydrochloric acid is always brown, yellowish brown or greenish brown; the remainder of very *gray coke iron* is always light gray, in different shades approaching to white.

[To be continued.]

L





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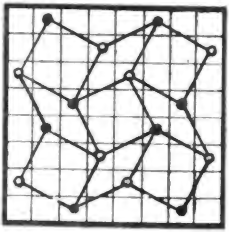

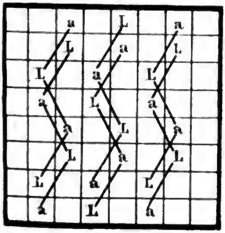

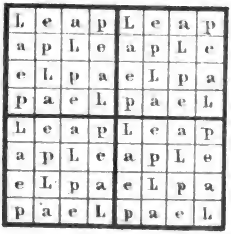

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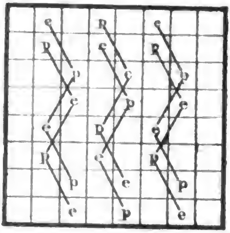

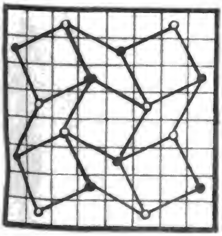

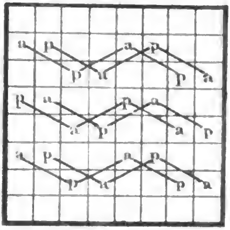

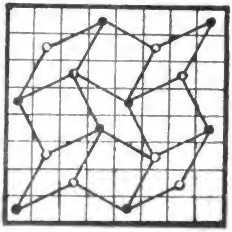

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Examples.

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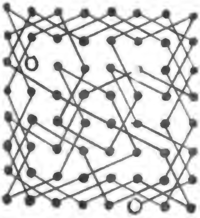

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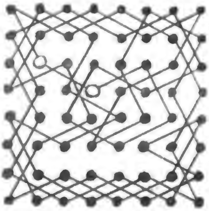

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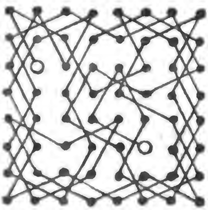

J. Basire, lith.

Dr. Roget on the Knight's moves at Chess.

LI. *Description of a Method of moving the Knight over every square of the Chess-board, without going twice over any one; commencing at any given square, and ending at any other given square of a different colour.* By P. M. ROGET, *M.D., Sec. R.S.*

[Illustrated by Plate I.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE problem of carrying the knight, in a course of his own moves, over every square of the chess-board, without going twice over any square, has engaged the attention and exercised the ingenuity of mathematicians during the last hundred years*. Even the great Euler condescended to put forth a portion of his giant strength in grappling with the difficulties it presents, and bringing it within the grasp of his powerful analysis†. Vandermonde attempted to construct a general algebraic formula for its solution‡. Others, confining their efforts to the attainment of mere practical results, have contented themselves with the search of particular methods of resolving the problem in limited cases, and under the simplest conditions only; such as that of being obliged to commence the journey of the knight from a given square; one of the corner squares having usually been selected for that purpose§. The next step was the contrivance of methods fulfilling a further condition, namely, that the square at which the tour of the knight terminates shall be so situated as to be one move from the square from which it was begun. It is evident that whenever this has been accomplished, we have obtained a recurring or circular course, which the knight might again traverse by continuous moves: so that such a course gives us the power of commencing with any given square whatsoever, and of traversing through the whole series of 64 squares, until the entire circuit is completed.

Various circuits of this kind have been devised, and described in different memoirs, which have, from time to time, been published‖; and the problem, under this form, has been deemed to possess sufficient interest to induce those who are curious in these matters to bestow pains in inventing expedients for impressing some particular circuit on the memory, so as to enable the possessor of this clue to guide the knight through the mazes of his devious route, without reference to chart or compass. In a memoir, which has appeared in Frazer's Magazine for the present month*, which I have just now seen, a method is recommended for attaining this object, which consists in designating each square in the board by a different syllable, composed of certain consonants and vowels, indicating the horizontal and vertical columns in which it stands. The whole series of these 64 arbitrary syllables, joined into 16 words, pointing out the sequence of the squares in the circuit, but void of any other meaning, is required to be learned by heart; by an effort similar, and not less distressing than that by which we strive to gain possession of the chronological epochs of the kings of England, when committing to memory the barbarous cacophonies of Grey's *Memoria Technica.*

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* See Ozanam, *Récréations Mathématiques et Physiques, nouvelle édition*, Paris,1750, tom. i. p. 260, where De Montmort, De Moivre, and De Mairan, are quoted as having treated this subject.

† *Mémoires de l'Académie de Berlin* for 1759, p. 310.

‡ In a paper entitled "*Remarques sur les Problèmes de Situation*," in the *Mémoires de l'Académie Royale des Sciences*, 1771, p. 566.

§ This is the point at which the problem is left by Ozanam.

‖ See Journal of Science and the Arts, iii. 72. March, 1817; and also Edin. Phil. Journal, iv. 397, ix. 236.

It does not seem to have occurred to any of those who have hitherto favoured the world with the results of their speculations, that the problem in question would be rendered more general, and consequently more curious, by imposing, in addition to the unlimited assignment of any square for the commencement of the moves, the further condition that they shall terminate at any other given square of an opposite colour †. A great many years ago, I contrived a method by which the problem, in this new and extended form, may be resolved with the greatest ease; and the attention of the public having now been again called to the subject by the last-mentioned paper, I have thought that the communication of my method might not be unacceptable; especially as it depends on a principle which not only furnishes the means of constructing an incalculable number both of recurrent and of non-recurrent circuits, but also admits of very general application to the problem of the knight's move. It is founded on the following considerations.

Conceive the chess-board to be divided into four quarters by a vertical and horizontal line, both passing through its centre, as shown in the central diagram in the group marked L E A P, Plate I. and let the squares in each quarter be considered as grouped together into four sets, designated severally by the letters L, E, A, P; thus,

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* Entitled "Chess without a Chess-board, by a Chess Player," p. 316.

† That the initial and the terminal squares must, of necessity, be of opposite colours, will be evident from the consideration that, as the total number of squares, namely 64, is an even number, and as the knight's moves are always alternately from white to black, and from black to white, the terminal square must be one of a different colour from that at which the moves commenced.

L	E	A	P
A	P	L	E
E	L	P	A
P	A	E	L

It is here to be observed, that each of these sets of squares, marked with the same letter, constitutes a recurrent circuit of four moves of the knight.

The squares in the other three quarters being similarly designated, as shown in the central diagram already referred to, it will be found that the several sets in each admit of being connected by knight's moves with the corresponding sets, similarly designated in the adjacent quarters. This is shown in the corner diagrams, L, E, A and P, where the connexions among the squares of each set are marked by oblique lines joining their centres*. The sets, thus connected, constitute four separate systems, of 16 squares each; and it will also be found that these 16 squares are so disposed that the knight may, in each system, perform the circuit of all its squares, beginning from any one given square, and ending at any other of a different colour. A few trials will soon satisfy the learner that, in every case, this may very easily be accomplished, and generally in a great variety of ways.

It will next be perceived that the knight can always pass from any of the squares, (excepting those situated at the corners of the board) of one system denoted by a consonant, to those of a system denoted by a vowel, and contrariwise; (as is shown by the diagonal lines in the four diagrams intermediate to the former); but not from vowel to vowel, or from consonant to consonant. From the corner squares, the move can only be made to squares belonging to the same system.

The solution of the proposed problem includes three cases:

1. If the given initial and terminal squares belong, the one to a system denoted by a consonant, and the other to a system denoted by a vowel, then, following the order of the letters when arranged in a circle, thus :

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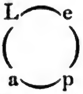
* The white squares have a circle, and the black a dot in their centres.



and proceeding either to the right or left, as the case may be, the circuit of each system must be gone over in succession, according to that order: beginning with that system to which the initial square belongs, and ending with that of the terminal square; taking care, however, for the reason above given, to avoid ending the intermediate circuits at a corner square. It will be advisable also to avoid ending these circuits at a square situated on the borders of the board, for they will not always admit of a transition being made from them to the next system into which we have to enter.

2. If both the given initial and terminal squares belong to the same system, omit, while going over that system, the terminal square, and also one in immediate connexion with it *; and fixing on some square in another system, which may be connected with it, proceed as before, taking care to end at this last-mentioned square; whence, when the rest of the entire circuits are completed, the two omitted squares may be attained, and the conditions of the problem satisfied.

3. If the initial and terminal squares belong, both of them, either to systems denoted by consonants, or to systems denoted by vowels, the same course with that just described must be pursued when the system to which the terminal square belongs is gone over, and with the same ultimate result.

Examples of each of these cases are given in the three lower diagrams, the path of the knight in his course over the board being traced by oblique lines joining the centres of the squares he traverses; the commencement and end of each course, which are supposed to be previously given, being marked by a small circle. I have made the second an example of a recurrent circuit, in order to show that this condition adds no new difficulty, and makes no difference in the mode of proceeding.

In these examples, the given initial square is the same in all of them, and belongs to the system L. In No. 1, the terminal square belongs to the system A. Here, we first go over the whole sixteen squares of system L; thence, passing over to system E, we traverse all the squares of that system. We next enter system P, covering in succession all the

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* The omission of this second square is not absolutely necessary, but will generally be found to facilitate the subsequent operations.

squares belonging to it; and we, lastly, come to system A, where we find no difficulty in ending the course at the given terminal square.

In No. 2, where the terminal square belongs to system L, as well as the initial, we must, in going over that system, omit that square, and also one connected with it. With this exception, we are to proceed as before, traversing in succession the systems L, E, P and A; and taking care to end at a square of the latter, connected by a knight's move with the other omitted square of system L.

In No. 3, where the terminal square belongs to system P, the same series of courses is to be pursued, excepting that the squares to be omitted will belong to that system.

Your obedient servant,
P. M. Roget.

39, Bernard Street, Russell Square,
March 20th, 1840.

LII. *Observations on the Precipitation of Copper by Voltaic Electricity.* By Edward Solly, Jun. *In a Letter to Richard Taylor, Esq. &c.*

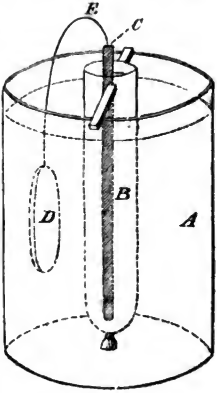
THE beautiful discoveries of Professor Jacobi and Mr. T. Spencer have, as it were, laid the foundation of an entirely new art, namely, that of copying works in metal, without heat, without pressure, and at a very small expense. As many of your readers may not have seen either Mr. Spencer's very interesting and ingenious pamphlet, or any detailed account of the process which he has so successfully employed in copying medals, copper-plates, &c., I will briefly sketch the principles of the process of voltaic precipitation, and describe the apparatus required for the purpose, introductory to a short account of some experiments on the subject which I have made.

When a piece of tin or other similar metal is immersed in a strong solution of sulphate of copper or blue vitriol, it soon becomes coated with metallic copper, which is said to be precipitated or reduced; the oxide of copper being decomposed by the more oxidizable metal having a stronger affinity for oxygen than the copper itself has. In this way the tin becomes oxidized and dissolved, and the copper is reduced and precipitated in the form of a thin film. If the whole surface of the tin were to become coated with copper, of course this action would cease, because the former being entirely cased in copper, would remain inert, and in fact represent a plate of that metal. From the mode in which this precipitation is caused, it follows that the metal precipitated must be everywhere in perfect contact with the surface on which it is thrown down; and thus we here have the first element of the process of copying, or obtaining a cast by precipitation, but extremely crude and imperfect. The film of copper so thrown down would be so thin and fragile that it would be impossible to remove it from the surface of the tin; or if it should acquire any thickness it would of course do so at the expense of a considerable quantity of tin, because for every portion of copper precipitated a corresponding quantity of tin must have been dissolved; the smoothness of the surface would become destroyed; and as this corrosion does not act equally all over the surface, if any design had been traced upon it, it would be much weakened, and in some parts wholly obliterated.

Both these difficulties are easily overcome in consequence of the facilities which electricity gives us, of, as it were, making chemical action portable; of generating it in one place, conveying it along metallic conductors, and making use of its power in another. Hence we are enabled to employ at pleasure one of the most powerful known deoxidizing agents, hydrogen. By means of electricity, we are able to give to any piece of metal, the power of evolving hydrogen, under the most favourable circumstances, from its surface, whilst immersed in a cupreous solution; and to continue that action, until the coat of deposited copper, precipitated by the action of the nascent hydrogen, has acquired any degree of thickness.

In order to effect this, a piece of some highly oxidizable metal, such as zinc, is connected by a wire soldered to its one end with the plate of lead, tin, or other metal, on the surface of which is engraved the design proposed to be copied. A vessel of any shape and material is divided into two portions by a partition or diaphragm of membrane, unglazed earthenware, or any other porous substance; the one division being filled with a strong solution of sulphate of copper, and the other with dilute sulphuric acid. The zinc is then placed in the dilute acid,and the mould or form to be copied, in the solution of copper. The zinc continues to dissolve in the acid, and generate the power, which being conveyed through the solutions to the surface of the mould, there causes the precipitation of copper, whilst the wire joining the zinc with the metallic mould, forms the connexion necessary to complete the galvanic circle.

The apparatus for this purpose is exceedingly simple, cheap, and easy of management; and when once arranged, and set in action, requires no further attendance until the operation is complete, when the mould is to be removed and the copy separated from it.



A very convenient form and arrangement is the following: *A* is a glass or earthen vessel containing a quantity of a saturated solution of sulphate of copper; *B* a piece of gut or tubular membrane formed into a bag by being tightly tied at the lower end and secured in a vertical position in the middle of the jar *A*, by means of a stick, which passing through two holes in it, rests upon the top of the jar. This bag is filled with dilute sulphuric acid, and contains the zinc rod *C*, which is likewise supported by the stick. *D* is the mould to be copied, and *E* the metallic wire connecting it with the zinc. Matters being thus arranged the precipitation goes on rapidly, and all that is requisite is to take care that the solution of sulphate of copper does not become too weak; when this happens, the copper comes down in a pulverulent and finely divided state, without any cohesion; falling off from the surface of the mould in the form of a bulky powder, and rapidly reoxidizing. This effect also happens when the surface on which copper is to be precipitated is very small, compared with the size of the zinc and the strength of the acid. In the first case it is easily prevented by always keeping excess of undissolved sulphate of copper in the solution to supply that which is decomposed.

When the deposited metal has acquired sufficient thickness, it is easily removed from the surface of the mould, by gently loosening its edge all round with any sharp instrument, after which it may be readily separated. Mr. Spencer has ingeniously availed himself of the different expansibility by heat of different metals, in removing the deposited metal from the mould. When copper is to be precipitated in a copper mould, he recommends rubbing into the surface of the mould a very small quantity of beeswax, the copper being previously warmed. In similar cases I have found that a small quantity of plumbago well rubbed over the surface completely prevented adhesion.

The form of apparatus above described, has I believe an advantage over that first proposed by Mr. Spencer, in the vertical position of the mould. When the mould is placed horizontally beneath the zinc rod, it is more liable to become fouled with dust and impurities of various kinds which can hardly be kept out of the sulphate of copper, and which becoming gradually covered over by reduced copper, cause in the face of the metal when finished, the appearance of black specks; this is avoided in the form described. Another convenience is that two, three, or even four moulds may be operated upon at once, and any one may be removed at pleasure without disturbing the others. I have been thus minute in describing the apparatus requisite for the process, because it is very commonly supposed, that as the process is called "Voltaic precipitation," a powerful voltaic battery, complicated and expensive apparatus, and a complete knowledge of electricity is requisite for its performance; whilst in fact the great beauty of the process consists in its extreme simplicity, requiring only a slight acquaintance with the most elementary laws of that science.

When the mould employed is perfectly clean and sharp, and the process has been properly conducted, the copy obtained is of pure and brightly metallic copper, usually of a pink colour. If a copy of a medal thus taken, and after having been removed from the mould have a small quantity of copper precipitated upon its face, it assumes a most beautiful dead silky lustre, which with very little if any injury to the sharpness of the work gives it a very beautiful play of light and shade.

The colour of the precipitated copper appears to be very much influenced by the nature and condition of the mould; and by paying attention to this circumstance, it may be obtained of a great variety of shades of colour. I have sometimes thought, that the colour of a voltaic cast of a medal is dependent on the nature of the metal, of which the original medal is composed; because I frequently observed that copper precipitated in a fusible metal mould made from a silver medal, had a remarkable whiteness, whilst those similarly made from copper medals were red, and from gold had a yellow colour. On endeavouring to ascertain whether these effects were really dependent on the nature of the original medal, I found that so many little causes seemed to influence the results, that it was almost impossible to draw any certain conclusions with regard to these curious peculiarities of colour. The surface of the deposited copper is exceedingly apt to tarnish from exposure to air, frequently becoming partially bright orange, and sometimes even of a brilliant red colour. When heated up to nearly a red heat it acquires a uniform iron grey colour which is perfectly permanent. The precipitated metal is rather brittle, though very elastic; but by heating it and allowing it to cool slowly it becomes tough and flexible.

In this process it is evident that a metallic surface is requisite for the commencement of any precipitation of copper; the arrangement in fact forms a single cell of a Daniell's battery, and is incomplete with the presence of the surface of the second metal in the sulphate of copper.

Mr. Spencer has shown, however, that moulds for the precipitation of copper may be made of any substance by gilding them or otherwise covering their surface with a thin film of metal which affords a conducting surface for the first portions of copper to be precipitated upon. My attention was early directed to this part of the process, because it seemed to open a wide field for new and beautiful applications. I was induced to pay particular attention to the deposition of copper upon non-metallic surfaces, and in consequence made numerous experiments to ascertain the circumstances most favourable to its precipitation under these conditions. My first experiments were made on surfaces of plaster of Paris, which I endeavoured to coat with copper, so as in fact to convert plaster casts into bronzes. I commenced by gilding the surface with different metals in the manner proposed by Mr. Spencer, but I found it exceedingly difficult to get a perfectly smooth and uniform surface; the process succeeded best with gold-leaf, but even that had its objections, and was besides very expensive. Subsequently I tried metals, such as bismuth and antimony in a state of very fine division, ground up with water and glutinous matters; these attempts were however not much better than the first trials.

In the course of these experiments I observed a curious fact, which I had not at all anticipated, and which very materially assisted me in attaining the objects which I had in view. When I had endeavoured to precipitate lead from a solution of one of its salts, in the same way that I had been doing with copper, I found that small grey crystals of lead soon formed upon the most prominent parts of the metallic mould I was employing, and which happened to be a leaden cast of a medal: these crystals rapidly increased in size, extending towards the membrane bag containing the zinc, which was about 3 inches distant from the mould. As soon as the crystals reached the surface of the membrane they bent about in various directions, crossing and recrossing each other until they had completely enveloped the membrane in a net-work of reduced lead. Again, when silver is precipitated from the fused nitrate, by electricity, the crystals formed at the one electrode extend across the fused electrolyte, until having reached the opposite electrode they complete the metallic circuit and prevent further decomposition. In the same way I expected that when copper was deposited against a badly-conducting surface, it would increase much more rapidly in the direction towards the zinc rod, and that it would have but very little tendency to increase sideways; but I found, on the contrary, that the deposited copper had a remarkable property of extending by its edges far more rapidly than it increased in thickness; seeming to creep along or cling to the surface of the plaster or other non-conducting substance, against which it was being precipitated: and even when the plaster surface was placed at an angle of 45° to the zinc rod, and the deposition commenced in the centre, where a piece of gold-leaf had been applied, the copper extended equally all round and quite as fast on the side, where it receded from the zinc as on that part where by increasing it approached it. Following out this circumstance, I soon found that by very slightly improving the conducting power of the surface of the plaster or other non-conducting substance, I was enabled to precipitate copper without any gold-leaf or other metallic surface for commencement. The degree of conducting power requisite for this purpose was very slight, all that was necessary being to wash the surface over once or twice with a solution of nitrate of silver or muriate of gold, drying and well blacking each successive coat by exposure to light, the surface having previously been well rubbed over with a small quantity of plumbago. When thus prepared and placed in the solution of sulphate of copper, it was sufficient to touch any part of it with the wire attached to the zinc to cause the precipitation; a small ring of copper soon formed on the blackened surface round the wire, which increasing in size, in time covered the whole surface which had been prepared. When the deposit of copper had reached the edge of the prepared surface it still continued to increase, but more slowly, extending around the edges, even on to the back of the plaster, and accommodating itself to all the inequalities of its surface almost as perfectly as if it were metallic. In this manner I have caused it to be precipitated along the surface of card, paper, and on a variety of the most delicate and easily destructible organic substances. Indeed I have frequently seen it, when arrested by an air-bubble, gradually surround and envelope it, and thus form a perfect cast; the process going on with sufficient rapidity, and yet without disturbing so frail a form.

Some of these experiments are interesting when viewed in relation to certain phænomena of fossilization, not merely in cases where organic matters are replaced by pyrites, but also in those where silica and other earthy substances are concerned.

By this process I was easily enabled to cover the surface of any article moulded in plaster, sulphur, wax, or any other substance; but it usually happened that by the time that the whole surface was covered some parts had begun to throw up little mammillated excrescences of copper which destroyed the smoothness and regular appearance of the surface. Although I thus failed in my original purpose, yet I saw sufficient reason to feel no doubt that plaster may be covered with a uniform coating of copper. The surface of the copper precipitated against the plaster is of course smooth, and therefore the process might be conveniently employed in any case where one or only a few copies are required of any metallic surface. Thus finger plates for doors, and all kinds of thin ornamental metal work, may be copied with great perfection.

A very beautiful effect is produced by coating the surface of facsimiles of medals or casts, made of lead or fusible metal, with a thin film of reduced copper; they then exhibit the beautiful silky dull appearance which I have before alluded to as being possessed by the precipitated metal. If these could be preserved from tarnishing by the application of any varnish or lacquer, exceedingly beautiful and cheap ornaments might be made in this manner, such as clock cases, &c.

I have likewise been engaged in a series of experiments on the precipitation of other metals by similar means, and shall probably, when sufficiently at leisure, prepare a short account of them.

38, Bedford Row, March 2, 1840.

LIII. *On the Galvanic Properties of the Metallic Elementary Bodies, with a description of a new Chemico-Mechanical Battery.* By ALFRED SMEE, *Esq.*[*]

LAST May a number of experiments were performed upon the galvanic properties of the non-metallic elementary bodies, and these were attended with the acquisition of some curious information[†], but till lately no opportunity has presented itself of extending the series of investigations then conducted: now, however, that I believe that I can lay before the public a valuable battery, no time is lost, that others may extend and improve the new principle about to be detailed.

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[*] Communicated by the Author.

[†] The results connected with this part of the subject will be given in a future number.

With regard to the metallic elementary bodies, their properties have been investigated so frequently, and to such an extent, that it may seem unnecessary to draw attention again to them; but two circumstances influencing their action have never been noticed. It is well known that the positive metal should be the most readily acted upon by the solution, and the negative the least, and the further these are apart, the more forcible will be the battery: thus, *cæteris paribus*, platinum and zinc are more powerful than iron and zinc; but if a circuit be made of a piece of smooth platinum and zinc it will sometimes happen that the effect is less than when a circuit is formed by a similar piece of iron. Now this appears at first sight paradoxical, though it can in many instances be easily explained; for if the platinum be carefully examined, it will be seen that the acid solution does not really wet the platinum, but runs off from the greater part of the surface, as metallic mercury does from glass. In this state, a piece of platinum having a surface of thirty-two square inches, formed into a battery with amalgamated zinc and connected with a magnet, supported three-quarters of a pound through five thicknesses of paper; when the same piece of platinum was heated or dipped in nitric acid and afterwards well washed, it supported a similar weight through twelve thicknesses of paper, thus being less powerful than iron in the first instance, and more so in the second. In the same way, silver supported under the like circumstances, the keeper of a magnet through three layers of paper: on being heated and again wetted, the attractive force was exerted through nine thicknesses of paper, but no additional power was gained by removing the surface of the silver by nitric acid. The metals in these cases appear to become coated with a film of air, which effectually prevents the contact of the fluid. This is also seen in the various forms of charcoal, which after ignition are very powerful, but lose much of their force if long exposed to the air; their energy however is restored upon their being again heated.

As in the experiments just detailed, and in those which I am about immediately to describe, the relative powers of the arrangements have to be considered, it will be proper to mention in what way the results were obtained. A soft iron horseshoe magnet was suspended, round which covered wire in communication with the poles of the battery was wound: the keeper, which weighed three quarters of a pound, was separated from the poles of the magnet by as many layers of thin blotting paper as could be used without its falling; thus with a battery of feeble force few layers of paper could be interposed; but with one of greater strength, forty or sixty thicknesses might be used. A similar form of apparatus might easily be devised, which would show by means of a delicate screw the exact distance at which a given weight would be supported by the attractive force of the induced magnet.

The influence of different conditions of surfaces is a subject which has escaped all experimenters. Now this is singular, for many must have noticed, that in a circuit, the greatest quantity of gas is given off at the corners, edges, and points. Following this hint, a piece of spongy platinum, consisting as it does of an infinity of points, was placed in contact with amalgamated zinc, when a most violent action ensued, so that but little doubt could be entertained of its forming a very powerful battery. The fragile nature of this material precludes it from being thus used, and therefore it was determined that another piece of platinum should be coated with the finely-divided metal. This experiment was attended with a similar good result, and the energy of the metal thus coated was found to be surprising. To test the value of this process, a piece of platinum, thus platinized, was placed in dilute acid in contact with amalgamated zinc, and the quantity of gas evolved in a given time was noticed.

		c.i.	
Platinized platinum	7 sq. inches gave off	5 per 1	minute
Platinum heated	ditto	1 per 1	minute
Platinum covered by air	ditto	1 per 6	minutes
Platinized coke	small piece	3 per 5	minutes
Plain coke	ditto	1 per 25	minutes.

In these experiments the contact was made in each cell alike; the same zinc being used, and the distance being the same between the metals. The energy of the metal thus prepared upon the soft iron magnet is very great. A piece of platinum exposing thirty-two square inches of surface, supports three-quarters of a pound through seventeen thicknesses of paper, whilst when smooth and wetted it supported it through eleven layers; and when no care was taken about its being wet, but when simply plunged into the liquid, only through five layers of the same paper.

The cause of this increase of power appears to be the facility given to the evolution of the gas from the number of points, and not from an increase of surface, as but little benefit attends its application in the nitric acid batteries, in which the hydrogen is not evolved, but absorbed by the fluid.

The next point which we have to consider, is whether other finely-divided metals have the same good effect; but no other of the many metals that I have tried can be used with similar good results, except palladium, which though it has not much effect in the sponge, is found when precipitated on platinum or silver to possess powers, about equal to the finely-divided platinum. Precipitated silver increases the power of the metals, but not nearly to the extent of platinum.

Having ascertained that a solution of platinum must be used for increasing the power of metals in their ordinary state, it becomes a matter of great importance to ascertain whether the platinum may be precipitated upon other metals with advantage; and for this purpose it was deposited upon earthenware, palladium, pure silver, copper plated with silver, nickel, German silver, tin, lead, brass, cast iron, sheet iron, steel, zinc, and charcoal. The platinized earthenware was not found to answer, apparently from the quantity of the metal not being sufficient to carry the electricity. Palladium, silver, and plated silver answered equally well with platinum to receive the precipitated metal, and if there was any difference, I think the silver was rather the best. Plated copper answers very well, but care should be taken to varnish every copper edge, or else that metal is apt to be slightly dissolved, and deposited again upon the platinized silver, which is injurious. Should copper, from any cause, get upon the silver, it may be dissolved by a little muriatic acid, and afterwards by a little strong ammonia. No other metal or alloy besides this answered for the reception of the platinum, except iron, and this was as active as silver for a time, but then a local battery was formed between the platinum and iron—the iron was dissolved and the battery destroyed. In some cases this does not take place so rapidly as in others. Carbon answers admirably for the reception of the platinum, and is improved in like manner.

We have now the elements for the manufacture of a powerful battery; for we have seen that increase of power is obtained by taking care that the negative metal is thoroughly wetted by the fluid, and that this is not only accomplished, but its power materially increased by the numerous points formed by the precipitation of finely-divided platinum. Whatever metal, alloy, or compound may be found hereafter to succeed for the reception of the platinum, or whatever metal may be found to answer instead of the finely-divided platinum, still the principle by which the advantage is gained will be the same. However, the battery which I now propose is to be made of either copper plated with silver, silver, palladium, or platinum. The silver can be rolled to any thinness, and therefore is not expensive. Each piece of metal is to be placed in water, to which a little dilute sulphuric acid and nitro-muriate of platinum is to be added. A simple current is then to be formed by zinc placed in a porous tube with dilute acid; when, after the lapse of a short time, the metal will be coated with a fine black powder of metallic platinum. The trouble of this operation is most trifling; only requiring a little time after the arrangement of the apparatus, which takes even less than the description. The cost I find to be about 6*d.* a plate of 4 inches each way, or 32 square inches of surface. This finely-divided platinum does not adhere firmly to very smooth metals, but when they are rough is very lasting, and sticks so closely that it cannot be rubbed off. On this account, when either silver is employed, or copper coated with silver, the surface is to be made rough by brushing it over with a little strong nitric acid, which gives it instantly a frosted appearance, and this, after being washed, is ready for the platinizing process.

With regard to the arrangement of the metal thus prepared great diversity exists; it may be arranged in the same way as an ordinary Wollaston's battery with advantage; a battery thus constructed possessing greater power than Professor Daniell's battery: four cells, containing 48 square inches in each cell, decomposed 7 cubic inches of mixed gas per five minutes, whilst four cells of Professor Daniell's, in which 65 square inches of copper were exposed in each cell, gave off only five cubic inches in the same time. However, in my battery thus arranged, the action dropped to 5 cubic inches in five minutes, but it resumed its power after the contact had been broken for a few seconds. This battery also possesses great heating powers, raising the temperature of a platinum or steel wire, 1 foot long and of a thickness similar to that used for ordinary birdcages, to a heat that could not be borne by the finger*. Its magnetic power is not less astonishing, three cells supporting the keeper of a magnet through forty-five, two cells through thirty-two, and one cell through twenty thicknesses of paper. An electro-magnetic engine was made to rotate with great velocity, the combustion of the mercury at the breaking of contact being exceedingly brilliant.

A battery of this construction should be in every laboratory, to be used in most cases where a battery is wanted, and the slight labour attending its operation is scarcely worth mentioning. I have used one for 48 hours consecutively without the slightest alteration either of the fluid, or in the arrangement of the metals, and the diminution attending its operation appeared to arise from deficiency of acid, for it was instantly restored by a little strong sulphuric acid in each cell. Where the battery is required to possess the same power for a long period, it might be advisable to separate the metals by a porous earthenware vessel, or what answers the purpose equally well, by a thick paper bag, the joinings of which must be effected by shell-lac dissolved in alcohol. By these means, the sulphate of zinc is retained on the zinc side of the battery. The use of porous tubes, however, appears from observation, as far as my battery is concerned, to be nearly superfluous, at any rate in most cases; for I find, that after a battery arranged as Wollaston's had been at work in the same fluid for forty-eight hours, it had no zinc deposited on the silver. It is worth remarking, that during the last 24 hours contact had not been broken for a single instant. Notwithstanding these experiments, however, it may be as well in an extensive battery to use porous plates.

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* A small pot battery of six cells fairly fused into globules 2 inches of iron wire, and the combustion of different metals was extremely brilliant, when the battery was in combination with a Bachoffner's apparatus.

The battery may be arranged like the pot batteries, but I should greatly prefer the troughs, such as used for Wollaston's batteries, from the convenience of packing, and from a battery of the same surface requiring so small a space. A battery may be constructed to form a most powerful calorimotor. It may also be arranged as a circular disc battery. Or it may be made as a Cruickshanks, each cell being divided or not by a flat porous diaphragm. Whatever arrangement is adopted, the closer the zinc is brought to the platinized metal, the greater will be the power.

The generating fluid which is to be employed is water, with one-eighth of sulphuric acid by measure; and the zinc ought always to be amalgamated in the first instance, as that process will be found very œconomical from its stopping all local action, and the amalgamation will be found not to require repeating, because there is no fear of copper being thrown down on the zinc, which occasionally happens in the sulphate of copper batteries.

The battery thus constructed is the cheapest and least troublesome in action that has ever been proposed, and from the smallness of its bulk will be found very valuable to electromagneticians. It is second in power only to the nitric acid batteries, the objections to which have been already noticed. For medical purposes, with a Bachoffner's apparatus, a battery composed of platinized silver two inches each way will be found sufficient.

To recapitulate the processes of the formation of a battery: first the platina, silver, or plated copper must be roughened, the two latter with nitric acid, and afterwards washed. The metal is next to be placed in an acid solution with a little nitro-muriate of platinum, which metal is to be thrown down by the formation of a simple galvanic circuit; and lastly, the platinized metal is to be formed with amalgamated zinc into a battery, either with a porous tube or paper bag, or without them, according to the fancy of the operator, or the purpose for which it is wanted.

The advantage from this form of battery arises, as I believe, from a mechanical help to the evolution of the hydrogen; and therefore it is proposed to call it the Chemico-mechanical battery. This battery may remain in the acid for a length of time, and neither the amalgamated zinc nor platinized silver will undergo the slightest change, and the whole will be as silent as death. Let only communication be made, the liquid in each cell becomes troubled;—it boils—it bubbles, and produces the effects which have been detailed. The quantity of electricity passing through either wires or liquids may be pretty accurately judged from the action taking place in the battery, for if the communication be made through a liquid of difficult decomposition, or through long small wires (70 or 80 feet), but little gas will be given off from the platinized metal, but when short thick wires are used the action is violent. A galvanometer might be constructed of one cell, similar to the oxygen cell of Professor Daniell, as this would show the exact amount of electricity passing.

The importance of constructing a battery that shall be small in compass, efficient in action, cheap in its operation, and devoid of troublesome manipulation, is important in the highest degree; and I consider that my chemico-mechanical battery will be found frequently a useful means of obtaining gases for the oxy-hydrogen light. Its value for blowing up vessels under water, and exploding powder in mines, is sufficiently obvious.

In conducting the extensive series of experiments, of which this is a summary, the grand features have been rather attended to than very minute results; and in conclusion, it would ill befit me if I did not here mention the valuable assistance I have received by the loan of apparatus, &c. from many individuals, but most especially from Professor Daniell, William Terry, Esq., and Mr. E. Palmer.

Bank of England, Feb. 29, 1840.

LIV. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* *By* M. Dumas.*

IN this memoir, I propose explaining and discussing different rules and their consequences which have so often been the subject of important communications to the Academy, for I should think it useless to call upon its kind attention, if the developments, into which I have been obliged to enter, had not given it an unusual length. But the Academy will pardon me, when it knows the importance and the variety of the questions which I have been forced to unite in it, and which are the following:

"1. In every combination, can the elements have their place supplied, equivalent for equivalent, by simple bodies or by compound bodies, which act their part?

"2. Are not these substitutions often effected, without the general nature of the compound being altered by it; the bodies thus produced belonging to the same *chemical type* as those from which they are derived?

"3. In other cases, can these substitutions furnish products entirely distinct in their actions (*réactions*) from those which gave them birth, and is it then right to consider them notwithstanding as belonging to the same *molecular type?*

"4. Can the nomenclature of organic substances be remodeled, from the present time, in such a manner that the name of each body shall express the chemical type, or even the molecular type to which it belongs?

"5. Do the phenomena of substitution oblige us to modify profoundly the value affixed until lately to the organic radicals?

"6. Is not the electric function (*rôle*) attributed to the elements of compounds by the electro-chemical theory, in complete contradiction to the phænomena of substitutions?"

I shall subject each of these questions in succession to an attentive examination, applying myself to what is general and elevated, without entering into technical details, which will take their place in special memoirs.

Law of Substitutions.

Some years ago, M. Gay-Lussac mentioned a very simple experiment in his lectures, which has become a point of departure for an immense succession of inquiries and discoveries. In treating wax by chlorine, said the illustrious professor, I saw this substance lose some of its hydrogen, and take exactly an equivalent volume of chlorine to that of the hydrogen set free. I had myself subjected oil of turpentine to similar trials, and was convinced, agreeably to M. Devielle's late re-examination of the subject, that it easily loses eight volumes of hydrogen and takes in their place eight volumes of chlorine, thus constituting the compound $C^{40} H^{24} Ch^{8}$, derived from the original oil of turpentine $C^{40} H^{32}$.

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* From the *Comptes Rendus de l'Académie des Sciences*, 1840, prem. semest. No. 5, Feb. 3.

At the same time, I studied the composition of some extraordinary products obtained from alcohol, viz. chloral, chloroform, bromoform, iodoform, of which I gave an exact analysis, and endeavoured to explain their formation.

This work was the occasion of the law of substitutions being developed for the first time. But as it was then believed that certain organic matters, and alcohol in particular, contained water ready formed, the law of substitutions, in the form in which I first presented it, attributed a function to this water, which gave rise to many objections. To return to the details of this point, would be without interest at the present time, for those chemists who admit the reality of substitutions, have in general given up the supposition of the existence of ready-formed water in the compounds in which these substitutions are observed.*

Although the function which I had attributed to the water may be reconciled to the general phænomena of chemistry, as it is now become useless, we must limit the law of substitutions to the following expression:—"When a hydrogenated organic substance is treated with chlorine, bromine, iodine, or oxygen, &c., these bodies generally remove hydrogen from it, and for an equivalent of hydrogen so removed, an equivalent of chlorine, bromine, iodine, or oxygen is substituted in the compound.

Is this phænomenon general? has it a peculiar character? This is what we are about to examine.

At the present time every one knows that in the reciprocal action of bodies certain relations of weight are observed, and that it is not enough to say that sulphur or oxygen combine with or act upon zinc or lead, but that a quantity of sulphur weighing 201, and of oxygen weighing 100, act upon or combine with a quantity of zinc weighing 403, and of lead weighing 1294. These quantities are the chemical equivalents of those bodies; all chemical action takes place between them and their multiples.

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* It may however be observed, that when I admitted that chlorine decomposed this water, seized the hydrogen, and left the oxygen in the compound, I made a very logical supposition. An analogous case presents itself when the benzoate of silver is decomposed by bromine, giving bromide of silver, the oxygen of the oxide uniting with the benzoic acid.

When I added that oxygen itself could decompose the water fixed in the compounds, I was guided by the theory of cementation, in which we admit that iron decomposes carburet of iron.

Now, to say that from an organic compound, an equivalent of hydrogen may be subtracted, and that its place may be taken by an equivalent of chlorine, is manifestly announcing a law in perfect harmony with the general law of the reciprocal action of bodies by equivalents. Every one comprehends, that if a crystallized body could produce another, likewise crystallized, by losing hydrogen and gaining chlorine, which could not be represented by equivalents, we must conclude from this that the theory of equivalents is false. The law of substitutions ought to be in accordance with the theory of equivalents, as moreover the general expression which has been given to it suggests.

But from thence to assert that the law of substitutions has no peculiar character, that it is only a particular case of the theory of equivalents, there is either an equivocation or an immense leap. That this leap was made when the law of substitutions was at first put forth, that nothing allowed the cause of it to be foreseen, in order to connect it with a theoretical principle, I concede without difficulty. This also did not fail to be the case, and amongst the objections of the German chemists to the law of substitutions, it always figures first. The philosophers who some years ago viewed it in this manner, were right without doubt, but they must have been very much surprised to see so many skilful men persist in finding in it a special character.

With regard to myself, if I believed in the future prevalence of the law of substitutions, in its importance, five years ago, when I was the only one who defended it, it is not to be supposed that I can change my opinion, when the most eminent English chemist, Mr. Graham, adopts it without reserve; when M. Liebig, after sharply criticising it, now receives it as admitted in science; when so many labours, undertaken often to combat it, have come within this very circle, to give it a complete consecration; when, lastly, far from seeing in the law of substitutions a simple experimental fact, we are now able to ascend to its cause.

Thus, to assert, as M. Pelouze has done, that the phænomenon of substitution, when it is observed, is only a particular case of the theory of equivalents, is to announce as a novelty two things perfectly known, viz.: first, that in the action of two bodies, substitution does not always take place; secondly, that when it is effected, it takes place by equivalents. This does not hinder the phænomenon of substitution from possessing a special character, from constituting a case of chemical action so particular, that it was absolutely necessary to distinguish it from every other, as I have done.

To be convinced that the theory of substitutions is not general, there is no need of new facts; it will suffice to read my memoir on chloracetic acid, which has been so often quoted for some time. We there see that besides the chloracetic acid produced by substitution by means of the action of chlorine on acetic acid, oxalic acid and carbonic acid are developed, the production of which by substitution we may be at a loss how to explain, at least for the present.

And better still, it suffices to glance at my memoir relative to indigo: we there see that the white indigo, under the influence of oxygen, loses an equivalent of hydrogen without gaining anything. In this case then there is no substitution; I convinced myself of it. At a later period MM. Liebig and Wœhler observed facts of the same nature in their admirable researches upon uric acid. Quite recently, Mr. Kane observed similar facts in the colouring matter of Heliotrope.

Thus, the phænomenon of substitutions is not general; still more, this is one of its most essential characters, as we shall presently see.

Not only it is not general, because a body may, under the influence of oxygen, lose hydrogen without gaining anything, but it is not general also for the contrary reason. Olefiant gas, for example, may lose 4 equivalents of hydrogen and take 6 of chlorine; every one knows this. Any one who had not analysed all the intermediate degrees of this action, as M. Regnault has done, would, in comparing the first and last term, have found the law of substitutions defective.

At present it may be explained and understood without difficulty, when we say that if white indigo loses hydrogen without gaining anything, it passes into a new molecular type; when we know that olefiant gas may produce a chloride of carbon of the same type as itself, and, by a fresh addition of chlorine, a new chloride of a different type. Thus, the law of substitutions prevails when the bodies preserve their initial type; it is no longer applicable in the contrary case; and, by this very means, it serves to distinguish the bodies which have preserved their molecular type from those which have lost it.

But there is no occasion to return to this explanation, which my wish to express myself clearly has induced me to give *en passant*, to justify the necessity of distinguishing the law of substitutions from other chemical actions (*réactions.*).

The law of substitutions expresses, that in an organic body, we may take away 1, 2, 3 equivalents of hydrogen, and supply their places by 1, 2, 3 equivalents of chlorine, bromine, iodine, or oxygen. It indicates that these substitutions will give birth to new bodies, the properties of which it is often possible to foresee. It makes known that these actions (*réactions*) are the easiest, the most frequent, the least changing (*altérantes*) that a body can undergo.

Before the law of substitutions was published, no one could have foreseen how a hydrogenated body would have acted under the influence of chlorine or oxygen. Now every one knows it, and a chemist performs in a few days, by means of this guide, operations which would have required years of labour before he had learned its use.

Ask the theory of equivalents what ought to take place when ether is subjected to the action of chlorine, and it will certainly reply that it knows nothing of the matter; or indeed, what comes to the same thing, it will show you a hundred possible cases between which you will have to choose.

For ether may lose in succession the five equivalents of hydrogen which it contains without gaining anything, which gives five new bodies; for it may, without losing anything, absorb 1, 2, 3, 4, 5, and many more equivalents of chlorine besides; which makes ten, twenty, thirty new bodies, if we desire it; for it may, in losing a single, or even two, or three equivalents of hydrogen, absorb equivalents of chlorine more or less in number; and in this third hypothesis, the number of compounds will become almost innumerable.

In fine, we should fall upon almost infinite varieties of combinations, if we add that the oxygen of ether may be eliminated, either free, in the form of water, or in the form of carbonic acid.

Thus the theory of equivalents announces the production of a prodigious quantity of compounds: provided the elements which the ether loses and those which it gains are represented by equivalents, it is sufficient.

It is otherwise with the law of substitutions. With regard to that, when ether loses hydrogen, it must receive chlorine. There are then only five compounds which are possible, of which the composition is perfectly foreseen.

$$\begin{array}{cccccc} C^4 & C^4 & C^4 & C^4 & C^4 & C^4 \\ H^5 & H^4\,Ch & H^3\,Ch^2 & H^2\,Ch^3 & H\,Ch^4 & Ch^5, \\ O & O & O & O & O & O \end{array}$$

Amongst them, three are already known, and there is not the smallest risk to run when we predict the probable discovery of the two others.

The law of substitutions sees then in these five compounds some of the *nearest*, the *most necessary* modifications of ether. The theory of equivalents sees there any modifications whatever more or less *possible*. The one says, these five bodies must be formed first very easily and very abundantly; the other says, they may be formed, and many others with them.

If the acetic acid is in question, the theory of equivalents would besides announce the possible formation of compounds so numerous, that nothing could guide the observer. The law of substitutions, more precise, foresees and predicts that in losing 1, 2, 3 equivalents of hydrogen, the acetic acid will take 1, 2, 3 equivalents of chlorine, and will thus produce three new compounds. One of them constitutes the so-called chloracetic acid.

In a multitude of possible actions which are nearly equally foreseen by the theory of equivalents, the law of substitutions discovers with certainty those which are about to be produced; it foresees them, predicts them, and up to the present time its help has truly been of invaluable efficacy.

How, without it, should we have been led to discover, one after the other, four or five mixed products, hardly differing from each other, in some actions recently studied? Otherwise, how would it have been possible to perceive, that the action which was to be produced had not been exhausted, if the formulæ, through the impossibility of making them agree with the law of substitutions, had not warned the observer?

Let me make a comparison drawn from a familiar order of ideas. Let us put ourselves in the place of a man overlooking a game at chess, without having the slightest knowledge of the game. He would soon remark, that the pieces must be used according to positive rules. In chemistry, the equivalents are our pieces, and the law of substitutions one of the rules which presides over their moves. And as in the oblique move of the pawns, one pawn must be substituted for another, so, in the phænomena of substitution one element must take the place of another. But this does not hinder the pawn from advancing without taking anything, as the law of substitutions does not hinder an element from acting on a body without displacing or taking the place of any other element which it may contain.

How can we believe that a knowledge of the rules which govern the game of our chess-board, is useless for the explanation of the moves which offer themselves, for the purpose of foreseeing those which are about to arise from the relations of the different pieces, of the various agents placed in contact?

These are the foresights, always justified by experience, which characterize the law of substitutions. If it be connected with the theory of equivalents, it then results that every chemical phænomenon is represented by equivalents, and that the facts of substitution are chemical phænomena; that every possible event in chemistry is translated into the language of equivalents, and that after all a *true* fact must be a *possible* fact. In the same manner that the possible comprehends the true, in like manner, and not otherwise, the theory of equivalents comprehends the law of substitutions.

So far, I have reasoned as if the law of substitutions only applied in reality to the substitution of hydrogen, which has furnished the first examples of it. But chemists know that in an organic substance not only can hydrogen undergo substitution, but also oxygen and azote, of which it would be easy to cite numerous examples.

Still more, we can cause carbon to undergo real substitutions, which sufficiently shows how artificial that classification of organic substances would be, which would rest solely on the permanency of the number of the equivalents of carbon in all the compounds of the same family.

In an organic compound, all the elements may then be displaced, and others substituted for them in succession. Those which disappear most easily, abstraction being caused by certain conditions of stability which we cannot yet foresee, are those of which the affinities are the most energetic. This is why hydrogen is one of the easiest to subtract and have another element substituted for it; this is why carbon is one of the most rebellious, for we know few bodies which can act upon carbon and not upon hydrogen.

In fine, I add that the law of substitutions allows us not only to foresee the disappearance of a certain number, or of all the elements of the organic compound, and new elements being substituted for them, but also the intervention, playing the part of the same elements, of certain compound bodies.

Thus, cyanogen, carbonic oxide, sulphuric acid, the binoxide of azote, nitrous gas, amidogen, and many other compound groups may intervene as the elements would do, take the place of hydrogen, and give rise to new bodies.

The law of substitutions is then an almost inexhaustible source of discoveries. It guides the hand of the chemist who trusts to it, it rectifies his errors by showing him the cause of them; and in a number of possible but uncertain actions, it points out some which are proximate, easy to produce, and of the highest interest.

This future, so rich in facts which may be realised, so full of accessible discoveries, which the law of substitutions reveals to the eyes of the chemist, justifies a remark of my dear friend M. Ampère, having so warm a heart and a mind so rich in delicate perceptions. When I was speaking to him of the law of substitutions, he also, at first, confounded it with the ordinary equivalent actions; but when I had developed the views, still very incomplete, which I was already endeavouring to attach to them, "Ah! my friend," said he, "how I pity you! you have found work for your whole life." A prediction which would have been realised, if so many minds of a higher order, taking up the law of substitutions, had not given it a flight which makes my part of the work much less necessary.

[To be continued.]

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 13, 1840. THE reading of a paper, entitled "Experimental Researches in Electricity, Sixteenth Series." On the source of power in the Voltaic pile. By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

The determination of the real source of electrical power in galvanic combinations has become, in the present state of our knowledge of electricity, a question of considerable importance, and one which must have great influence on the future progress of that science. The various opinions which have been entertained by philosophers on this subject may be classed generally under two heads; namely, those which assign as the origin of voltaic power the simple contact of dissimilar substances, and more especially of different metals; and secondly, those which ascribe this force to the exertion of chemical affinities. The first, or the theory of contact, was devised by Volta, the great discoverer of the Voltaic pile; and adopted, since it was promulgated by him, by a host of subsequent philosophers, among the most celebrated of whom may be ranked Pfaff, Marianini, Fechner, Zamboni, Matteucci, Karsten, Bruchardat, and also Davy; all of them bright stars in the exalted galaxy of science. The theory of chemical action was first advanced by Fabroni, Wollaston, and Parret; and has been since further developed by Oersted, Becquerel, De la Rive, Ritchie, Pouillet, Schœnbein, and others. The author of the present paper, having examined this question by the evidence afforded by the results of definite electro-chemical action, soon acquired the conviction of the truth of the latter of these theories, and has expressed this opinion in his paper, published in the Philosophical Transactions for 1834.

The author, after stating the fundamental doctrine laid down by

Volta, proceeds to give an account of various modifications in the theory introduced by subsequent philosophers; and also of different variations in the views of those who, in the main, have adopted the chemical theory. Being desirous of collecting further and more decisive evidences on this important subject, he engaged in the series of experimental researches which are detailed in the present memoir.

It is assumed, he observes, by the advocates of the contact theory, that although the metals exert powerful electromotive forces at their points of mutual contact, yet in every complete metallic circuit, whatever be the order or arrangement of the metals which compose it, these forces are so exactly balanced as to prevent the existence of any current; but that, on the other hand, fluid conductors, or electrolytes, either exert no electromotive force at their place of contact with the metals, or, if they do exert such a power, the forces called into play in the complete circuit are not subject to the same law of compensation as obtains with circuits wholly composed of metallic bodies. The author successfully combats this doctrine, by bringing forward a great number of instances, where certain fluids, which have no chemical action on the metals with which they were associated in the circuit, are in themselves such good conductors of electricity, as to render evident any current which could have arisen from the contact of the metals, either with each other or with the fluid; the evidence of their possessing this conducting power being their capability of transmitting a feeble thermo-electric current from a pair of plates of antimony and bismuth. The following he found to be fluids possessing this property in a high degree; namely, a solution of sulphuret of potassium, yellow anhydrous nitrous acid mixed with nearly an equal volume of water, very strong red nitric acid, and a mixture of one volume of strong acid with two volumes of water. By employing the solution of sulphuret of potassium as an electrolyte of good conducting power, but chemically inactive with reference to either iron or potassium; and associating it with these metals in a circuit, formed by two test-glasses containing the solution, into one of which was immersed a plate of platina and a plate of iron, and in the other two plates of platina; and the circuit being completed by wires of the same metals respectively, joining the iron-plate in the first glass with one of the platina-plates in the second, while the other two platina-plates were united by platina wires, interrupted at one part by a short iron wire which joined their ends;—it was found by the test of an interposed galvanometer, that, as no chemical action took place, so no electric current was produced; yet the apparatus thus arranged could transmit a very feeble thermo-electric current, excited by slightly raising the temperature of the wires at either of their points of contact. Hence, the inference may be drawn, that the contact of iron and platinum is of itself productive of no electromotive force. On the other hand, the author shows, that the interposition in the circuit of the smallest quantity of an electrolyte, which acts chemically on either of the metals, the arrangement remaining in all other respects the same, is immediately attended with the circulation of an electrical current far more powerful than the thermo-electric current above-mentioned. A great number of combinations of other metals were successively tried in various ways, and they uniformly gave the same results as that of iron and platina. Similar experiments were then made with various metallic compounds, and also with other chemical agents; and in all cases the same general fact was observed; namely, that when no chemical action took place, no electrical current was excited; thus furnishing, in the opinion of the author, unanswerable arguments against the truth of the theory of contact. The only way in which it is possible to explain these phenomena on that theory, would be by assuming, that the same law of compensation as to electro-motive power is observed by the sulphuret of potassium, and the other fluids of corresponding properties, as obtains in the case of the metals, although that law does not apply to the generality of chemical agents; and in like manner, different assumptions must be made in order to suit the result in each particular combination, and this without any definite relation to the chemical character of the substances themselves; assumptions, which no ingenuity could ever render consistent with one another. At the conclusion of the paper, the author describes some remarkable alternations in the phenomena which occur, when pieces of copper and silver, or two pieces of copper, or two of silver, form a circle with the yellow sulphuretted solution; and which lead to the same conclusion as the former experiments. If the metals be copper and silver, the copper is at first positive, and the silver remains untarnished; in a short time the action ceases, and the silver becomes positive, at the same time combining with sulphur, and becoming coated with sulphuret of silver; in the course of a few minutes, the copper again becomes positive; and thus the action changes from one side to the other in succession, and is accompanied by a corresponding alternation of the electric current.

March 5.—The reading of a paper entitled, "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic Processes;" by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed and concluded.

The object which the author has in view in this memoir is to place on record a number of insulated facts and observations respecting the relations both of white light, and of the differently refrangible rays, to various chemical agents which have offered themselves to his notice in the course of his photographic experiments, suggested by the announcement of M. Daguerre's discovery. After recapitulating the heads of his paper on this subject, which was read to the Society on the 14th of March, 1839, he remarks, that one of the most important branches of the inquiry, in point of practical utility, is into the best means of obtaining the exact reproduction of indefinitely multiplied facsimiles of an original photograph, by which alone the publication of originals may be accomplished; and for which purpose the use of paper, or other similar materials, appears to be essentially requisite. In order to avoid circumlocution, the author employs the terms *positive* and *negative* to express, respectively, pictures in which the lights and shades are the same as in nature, or as in the original model, and in which they are the opposite; that is, light representing shade; and shade, light. The terms *direct* and *reverse* are also used to express pictures in which objects appear, as regards right and left, the same as in the original, and the contrary. In respect to photographic publication, the employment of a camera picture avoids the difficulty of a double transfer, which has been found to be a great obstacle to success in the photographic copying of engravings or drawings.

The principal objects of inquiry to which the author has directed his attention in the present paper, are the following. First, the means of fixing photographs; the comparative merits of different chemical agents for effecting which, such as hyposulphite of soda, hydriodite of potash, ferrocyanate of potash, &c., he discusses at some length; and he notices some remarkable properties, in this respect, of a peculiar agent which he has discovered.

2. The means of taking photographic copies and transfers. The author lays great stress on the necessity, for this purpose, of preserving, during the operation, the closest contact of the photographic paper used with the original to be copied.

3. The preparation of photographic paper. Various experiments are detailed, made with the view of discovering modes of increasing the sensitiveness of the paper to the action of light; and particularly of those combinations of chemical substances which, applied either in succession or in combination, prepare it for that action. The operation of the oxide of lead in its saline combinations as a mordent is studied; and the influence which the particular kind of paper used has on the result, is also examined, and various practical rules are deduced from these experiments. The author describes a method of precipitating on glass a coating possessing photographic properties, and thereby of accomplishing a new and curious extension of the art of photography. He observes, that this method of coating glass with films of precipitated argentine, or other compounds, affords the only effectual means of studying their habitudes on exposure to light, and of estimating their degree of sensibility, and other particulars of their deportment under the influence of reagents. After stating the result of his trials with the iodide, chloride, and bromide of silver, he suggests that trials should be made with the fluoride, from which, if it be found to be decomposed by light, the corrosion of the glass, and consequently an etching, might possibly be obtained, by the liberation of fluorine.

As it is known that light reduces the salts of gold and of platinum, as well as those of silver, the author was induced to make many experiments on the chlorides of these metals, in reference to the objects of photography; the details of which experiments are given. A remarkable property of hydriodic salts, applied, under certain circumstances, to exalt the deoxidating action of light, and even to call into evidence that action, when it did not before exist, or else was masked, is then described.

4. The chemical analysis of the solar spectrum forms the subject of the next section of his paper. It has long been known that rays of different colours and refrangibilities exert very different degrees of energy in effecting chemical changes; and that those occupying the violet end of the spectrum possess the greatest deoxidating powers. But the author finds that these chemical energies are distributed throughout the whole of the spectrum; that they are not a mere function of the refrangibility, but stand in relation to physical qualities of another kind, both of the ray and of the analysing medium; and that this relation is by no means the same as the one which determines the absorptive action of the medium on the colorific rays. His experiments also show that there is a third set of relations concerned in this action, and most materially influencing both the amount and the character of the chemical action on each point of the spectrum; namely, those depending on the physical qualities of the substance on which the rays are received, and whose changes indicate and measure their action.

The author endeavoured to detect the existence of inactive spaces in the chemical spectrum, analogous to the dark lines in the luminous one; but without any marked success. The attempt, however, revealed several curious facts. The maximum of action on the most ordinary description of photographic paper, namely, that prepared with common salt, was found to be, not beyond the violet, but about the confines of the blue and green, near the situation of the ray F in Fraunhofer's scale: and the visible termination of the violet rays nearly bisected the photographic image impressed on the paper: in the visible violet rays there occurred a sort of minimum of action, about one-third of the distance from Fraunhofer's ray H, towards G: the whole of the red, up to about Fraunhofer's line C appears to be inactive; and lastly, the orange-red rays communicate to the paper a brick-red tint passing into green and dark blue. Hence are deduced, first, the absolute necessity of perfect achromaticity in the object-glass of a photographic camera; and secondly, the possibility of the future production of naturally coloured photographs.

5. The extension of the *visible* prismatic spectrum beyond the space ordinarily assigned to it, is stated as one of the results of these researches; the author having discovered that beyond the extreme violet rays there exist luminous rays affecting the eyes with a sensation, not of violet, or of any other of the recognised prismatic hues, but of a colour which may be called *lavender-grey*, and exerting a powerful deoxidating action.

6. Chemical properties of the red end of the spectrum. The rays occupying this part of the spectrum were found to exert an action of an opposite nature to that of the blue, violet, and lavender rays. When the red rays act on prepared paper in conjunction with the diffused light of the sky, the discolorating influence of the latter is suspended, and the paper remains white; but if the paper has been already discoloured by ordinary light, the red rays change its actual colour to a bright red.

7. The combined action of rays of different degrees of refrangibi-

lity is next investigated; and the author inquires more particularly into the effects of the combined action of a red ray with any other single ray in the spectrum; whether any, and what differences exist between the joint, and the successive action of rays of any two different and definite refrangibilities; and whether this action be capable, or not, of producing effects, which neither of them, acting alone, would be competent to produce. The result was that, although the previous action of the less refrangible rays does not appear to modify the subsequent effects produced by the more refrangible; yet the converse of this proposition does not obtain, and the simultaneous action of both produces photographic effects very different from those which either of them, acting separately, are capable of producing.

8. In the next section, the chemical action of the solar spectrum is traced much beyond the extreme red rays, and the red rays themselves are shown to exercise, under certain circumstances, a blackening or deoxidating power.

9. The author then enters into a speculation suggested by some indications which seem to have been afforded of an absorptive action in the sun's atmosphere; of a difference in the chemical agencies of those rays which issue from the central parts of his disc, and those which, emanating from its borders, have undergone the absorptive action of a much greater depth of his atmosphere; and consequently of the existence of an absorptive solar atmosphere extending beyond the luminous one.

10. An account is next given of the effect of the spectrum on certain vegetable colours, as determined by a series of experiments, which the author has commenced, but in which the unfavourable state of the weather has, as yet, prevented him from making much progress.

11. The whitening power of the several rays of the spectrum under the influence of hydriodic salts, on paper variously prepared and previously darkened by the action of solar light. The singular property belonging to the hydriodate of potash of rendering darkened photographic paper susceptible of being whitened by further exposure to light is here analysed, and shown to afford a series of new relations among the different parts of the spectrum, with respect to their chemical actions.

12. The Analysis of the Chemical Rays of the Spectrum by absorbent media, which forms the subject of the next section, opens a singularly wide field of inquiry; and the author describes a variety of remarkable phenomena which have presented themselves in the course of his experiments on this subject. They prove that the photographic properties of coloured media do not conform to their colorific character; the laws of their absorptive action as exerted on the chemical, being different and independent of those on the luminous rays: instances are given of the absence of any darkening effect in green and other rays of the more refrangible kind, which yet produce considerable illumination on the paper that receives them.

13. The exalting and depressing power exercised by certain media, under peculiar circumstances of solar light, on the intensity of its che-

mical action. This branch of the inquiry was suggested by the fact, noticed by the author in his former communication, that the darkening power of the solar rays was considerably increased by the interposition of a plate of glass in close contact with the photographic paper. The influence of various other media, superposed on prepared paper, was ascertained by experiment, and the results are recorded in a tabular form.

14. The paper concludes with the description of an *Actinograph*, or self-registering Photometer for meteorological purposes: its objects being to obtain a permanent and self-comparable register and measure, first, of the momentary amount of general illumination in the visible hemisphere, which constitutes day-light; and secondly, of the intensity, duration, and interruption of actual sunshine, or, when the sun is not visible, of that point in the clouded sky behind which the sun is situated.

In a postscript, dated March 3rd, 1840, the author states that he has discovered a process by which the calorific rays in the solar spectrum are made to affect a surface properly prepared for that purpose, so as to form what may be called a *thermograph* of the spectrum; in which the intensity of the thermic ray of any given refrangibility is indicated by the degree of whiteness produced on a black ground, by the action of the ray at the points where it is received at that surface, the most remarkable result of which is the insulation of *heat-spots* or thermic images of the sun quite apart from the great body of the thermic spectrum. Thus the whole extent over which prismatic dispersion scatters the sun's rays, including the calorific effect of the least, and the chemical agency of the most refrangible, is considerably more than twice as great as the Newtonian coloured spectrum.

In a second note, communicated March 12, 1840, the author describes his process for rendering visible the thermic spectrum, which consists in smoking one side of very thin white paper till it is completely blackened, exposing the white surface to the spectrum and washing it over with alcohol. The thermic rays, by drying the points on which they impinge more rapidly than the rest of the surface, trace out their extent and the law of their distribution by a whiteness so induced on the general blackness which the whole surface acquires by the absorption of the liquid into the pores of the paper. He also explains a method by which the impression thus made, and which is only transient, can be rendered permanent.

This method of observation is then applied to the further examination of various points connected with the distribution of the thermic rays, the transcalescence of particular media, the polarization of radiant heat (which is easily rendered sensible by this method), &c. The reality of more or less insulated spots of heat distributed at very nearly equal intervals along the axis of the spectrum (and of which the origin is *probably* to be sought in the flint glass prism used—but *possibly* in atmospheric absorption) is established. Of these spots, two of an oval form, are situated, the one nearly at, and the other some distance beyond the extreme red end of the spectrum, and are less distinctly insulated; two, perfectly round and well-insulated, at greater distances in the same direction; and one, very feeble and less satisfactorily made out, at no less a distance beyond the extreme red than 422 parts of a scale in which the whole extent of the Newtonian coloured spectrum occupies 539.

March 19.—"Researches in Electricity, Seventeenth Series: on the source of power in the Voltaic Pile." By Michael Faraday, Esq., D.C.L., F.R.S., &c.

In this series, the author continues his experimental investigation of the origin of electric force in the voltaic pile. Having found abundant reason, in the experiments already described, to believe that the electricity of the pile has its origin in the chemical force of the acting bodies, he proceeds to examine how the circumstances which can affect the affinity of substances for each other, influence their power of producing electric currents. First, with relation to *heat*:—circuits were made of a single metal and a single fluid, and these were examined with a view to ascertain whether, by applying heat at one of the junctions, only thermo-currents can be produced. Some peculiar effects of heat are noticed and explained; and several very necessary precautions in conducting these experiments are pointed out; and it is found, when these are guarded against, that heat has a decided and distinct effect over the chemical affinities of the parts of a circuit subjected to its power, and a corresponding influence on the electric current produced. This proceeds to such an extent, that, in some cases, either of two metals can be made positive or negative with respect to the other in the same fluid, solely by virtue of this power of heat.

The effect of *dilution* is then examined. For this purpose, only one metal and one fluid are used in a circuit; but the fluid is rendered more dilute at one point of contact than at the other. First, it was ascertained that such dilution produces little or no effect with metals which are not acted on by the electrolyte employed; and the precautions requisite as to other points are then stated. But when these are observed, still dilution is found to have a most powerful influence on the results; and, as the author believes, solely on account of its influence on the active chemical affinity. Thus copper in dilute nitric acid is positive with respect to copper in strong nitric acid; and the same is the case with lead, silver, and other metals. It is not that the piece in the weakest acid is always positive with respect to that in the stronger acid; for, in the first place, some very curious cases are given, in which a piece of metal in acid of a certain strength is *positive* with respect to a piece of the same metal in acid, either stronger or weaker; and, in the next place, other cases are stated in which the piece in the medium acid is *negative* with respect to the other piece in either stronger or weaker acid. The effect of dilution in nitric acid is such, that when certain different metals are compared together, one can, at pleasure be made positive or negative with respect to the other; thus, of the five metals, silver, copper, iron, lead, and tin, any one of them can be made either positive or negative with respect to any other; with the sole exception of silver positive with respect to copper. The inconsistency of these results with any theory of contact electromotive force is then strongly insisted on by the author.

The next division of the paper treats of the order of the metallic elements of voltaic circuits when different electrolytes are used. It is usual to say, that metals are positive or negative with respect to each other in a certain order; but Davy, and afterwards De la Rive, showed that, in certain cases, this order must be inverted. The author, by using ten metals and seven different exciting electrolytic solutions, shows that in no two solutions is the order the same; but that changes of the most extreme kind occur in exact conformity with the changes in chemical action, which the use of the different solutions occasions.

The next division of the paper considers the very numerous cases in which voltaic circuits, often such as are able to effect decomposition, are produced without any metallic contact, and by virtue of chemical action alone; contrasting them with the numerous cases given in the previous series, where contact *without* chemical action, whether it be the contact of metal with metal, or with chemically inactive electrolytes, can produce no voltaic current.

There then follows a consideration of the sufficiency of chemical action to account for all the phenomena of the pile. It is shown that chemical action does actually evolve electricity; that according as chemical action diminishes or ceases, so the electrical current diminishes or ceases also; that where the chemical action changes from side to side, the direction of the current likewise changes with it; that where no chemical action occurs, no current is produced, but that a current will occur the moment chemical action commences; and that when the chemical action which has, or could have produced a current is, as it were, reversed or undone, the current is reversed or undone likewise; that is, it occurs in the opposite direction, in exact correspondence with the direction taken by the transferred anions and cathions. The accordance of the chemical theory of excitation with these phenomena, is considered by the author as of the strictest kind.

The phenomena of thermo-electricity are considered by some philosophers as affording proofs of the efficacy of mere metallic contact in exciting an electric current. The author proceeds, therefore, to examine these phenomena in relation to such an action, and arrives at the conclusion, that they in fact disprove the existence of such a power. In thermo-electricity the metals have an order which is so different from that belonging to them in any electrolyte, that it appears impossible to consider their succession, in any case, as due to any mutual effect of the metals on each other common to both modes of excitation. Thus, in the thermo-circuit, the electric current is, at the hot place, from silver to antimony, and from bismuth to silver; but in a voltaic series, including dilute sulphuric or nitric acids, or strong nitric acid, or solution of potash, the electric current is from silver to both antimony and bismuth; whilst if the yellow sulphuret of potash be used, it is from both antimony and bismuth to silver; or if the hydro-sulphuret of potash be used, it is from bismuth to silver, and from silver to antimony; and, finally, if strong muriatic acid be used, it is precisely the reverse, or from antimony to silver, and from silver to bismuth. The inconsistency of these results with the contact theory is then insisted on and further developed.

The last section of this series is on the improbability of there existing any such force as the assumed contact force. The author contends that it is against all natural analogy and probability that two particles which, being placed in contact, have by their mutual action acquired opposite electrical states, should be able to discharge these states one to the other, and yet remain in the state they were in at the first, i. e., entirely unchanged in every point by what has previously taken place; or, that the force which has enabled two particles by their mutual action to attain a certain state, should not be sufficient to make them keep that state. To admit such effects would be, he thinks, to deny that action and reaction are equal. The contact theory, according to him, assumes that a force which is able to overcome powerful resistance, both chemical and mechanical, can arise out of nothing. That without any change in the acting matter, or the consumption of any other force, an electric current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped in its own course;—this, the author thinks, would be a creation of power, such as there is no example of in nature; and, as there is no difficulty in converting electrical into mechanical force through the agency of magnetism, would, *if true*, supply us at once with a perpetual motion. Such a conclusion he considers as a strong and sufficient proof that the theory of contact is founded in error.

FRIDAY-EVENING MEETINGS AT THE ROYAL INSTITUTION.

January 24, 1840.—Mr. Faraday on voltaic precipitations.

January 31.—Dr. Grant on the structure and growth of corals.

February 7.—Mr. Faraday on a particular relation (Dove's) of condensable gases and steam.

February 14.—Mr. Catlin's account of his residence and adventures among the native tribes of North America, with notices of their social condition, customs, mysteries, mode of warfare, tortures, &c.

February 21.—Mr. Nasmyth on the origin of alphabetic characters, and on the pneumatic mirror.

February 28.—Mr. Brayley on the application of science to the choice of building stones, with reference to the selection of stone for the New Houses of Parliament.

March 6.—The Rev. Mr. Hincks on the monstrosities of plants.

March 13.—Mr. Grove on voltaic reaction, or the phænomena usually called polarization.

Mr. Grove detailed the first experiments of Volta, Erman, Ritter, and Davy, the more recent ones of De la Rive, the explanation of these by Becquerel, and the confirmation of this latter philosopher's opinion by the experiments of Dr. Schœnbein, Mr. Matteucci, and Mr. Grove himself; all which, as well as the experiments of Mr. Grove on the inactivity of amalgamated zinc, which he proved to be due to the same order of causes, have been already given in full in various numbers of the Philosophical Magazine. All the effects which have generally been included under the term polarization were proved by Mr. Grove to be traceable to one principle, viz. the electrolytic transfer of elements having for each other a chemical affinity, and the reaction caused by this affinity when the decomposing and transferring power, i. e. the initial voltaic current, is arrested. What we are most anxious to call the attention of our readers to, are the astonishing effects exhibited by Mr. Grove at the conclusion of his lecture. Two batteries, little differing in construction from that described by him in the Lond. and Edinb. Phil. Mag., were charged some time previously to the lecture, and up to the period of its conclusion remained in perfect inactivity until the circuit was completed. One of these was arranged as a series of five plates, and contained altogether about four square feet of platina foil; with this the mixed gases were liberated from water at the surprising rate of one hundred and ten cubic inches per minute. A sheet of platinum, one inch wide by twelve long, was heated in the open air through its whole extent, and the usual class of effects produced in corresponding proportion. With the other arrangement, consisting of fifty plates of two inches by four, arranged in single series, a voluminous flame of one inch and a quarter long was exhibited by charcoal points, which showed beautifully the magnetic properties of the voltaic arc, as Dr. Faraday held a piece of iron near it, being attracted and repelled by different portions of the iron: bars of different metals were instantly run into globules and dissipated in oxide. It should be borne in mind that all these effects were produced by a battery which did not cover a space of sixteen inches square, and was only four inches high, and which had been charged for some hours.

Mr. Grove adverted to the letter of Prof. Jacobi to Dr. Faraday published in Lond. and Edinb. Phil. Mag., vol. xv. p. 161, and stated that Mr. Pattison, who navigated the Neva with Prof. Jacobi in October last, had observed that the batteries employed were on Mr. Grove's construction, which the Professor without hesitation admitted.

March 20.—Mr. Schomburgk on the aborigines of Guiana.

March 27.—Dr. Gregory on the statistics of disease and mortality in London.

LVI. *Intelligence and Miscellaneous Articles.*

THEORY OF SUBSTITUTIONS. POND GAS.

M. PERSOZ sent a letter to the Academy of Sciences, relating to the conversion of acetic acid into pond gas and to the theory of substitutions. The author appears to have written, lest some expressions made use of by M. Dumas, in the sitting of the 13th of January, should create a belief that he had entertained the same views as M. Dumas in arriving at the discovery of the production of pond gas from the acetates. He endeavours to show that if they have both arrived at the same conclusion, the means by which they have done so are peculiar to each and have nothing in common. On this subject M. Persoz recites several passages from a memoir which he sent in January 1838, under the title of ***On the necessity of distinguishing in chemical actions the phænomenon of displacement from those of alteration***; this memoir is still unpublished, and M. Dumas has declared that he was unacquainted with it; it contains the details of experiments, from which M. Persoz states it clearly results—

1st. That acetone contains two volumes of oxide of carbon.

2nd. That protocarburetted hydrogen gas is derived from acetone, and not from acetic acid, and consequently that the protocarburetted hydrogen which arises from the decomposition of the acetates, cannot be as M. Dumas supposes the immediate product of the decomposition of acetic acid by hydrate of potash; but, on the contrary, a consecutive secondary product resulting—

a. From the action which heat exerts on acetic acid, whether in a free state or combined with bases.

b. From the action which water exerts on acetone, one of the immediate products of the action of heat on acetic acid (MM. Liebig and Pelouze).

3rd. That this pond gas is formed by the disappearance of two volumes of oxide of carbon, and the assimilation of two volumes of hydrogen, derived from 1 eq. *decomposed water*.

From the preceding statements, M. Persoz adds, it is evident that he has not adopted the views of M. Dumas in explaining the formation of pond gas; and that the fact of its formation, so far from offering proof in favour of the theory of substitutions, justifies the statement which he has made respecting it, in the following passage of his "*Introduction to the Study of Molecular Chemistry*," p. 853.

"When M. Dumas maintains that chlorine is isomorphous with hydrogen, he lays it down as a principle, that bodies may be totally changed in their elementary condition, without varying in their molecular composition; we think this theory ought to be rejected as being contrary to experience: it is dangerous in its application, for to a certain extent it dispenses with the consideration, of the action which the first products, formed during a reaction, exert upon those which have not yet been altered."

In applying then the theory of substitutions to the formation of pond gas, obtained by the decomposition of acetic acid by means of an alkali, M. Dumas has not excluded the action which heat exerts upon acetic acid, and has thus neglected the compounds which result from it. He has, moreover, considered the formation of pond gas as the product of a simple action, whereas it is really the result of a complex one. Lastly, M. Dumas has completely neglected the action which water may exert, which in the formation of pond gas, acts according to M. Persoz a most important part.

M. Dumas has lately advanced, as a new argument in favour of the theory of substitutions, the fact of the identity which he has stated to exist between the final product of the action of chlorine on protocarburetted hydrogen, and the final product of the action of chlorine on chloroform: a final product which is represented by $C^2 Cl^8$, and obtained in the first case by the loss of eight volumes of hydrogen, replaced by eight volumes of chlorine, and in the second case by the loss of two volumes of hydrogen, replaced by Cl^2.

It appears to M. Persoz that in this case M. Dumas has confounded a phænomenon of alteration with a phænomenon of displacement. Will it be said, that because on burning four volumes of protocarburetted hydrogen with an excess of oxygen, there are obtained four volumes of carbonic acid containing four volumes of oxygen equivalent to eight volumes of hydrogen, there occurs in this fact an additional proof in favour of the theory of substitutions, and that eight volumes of hydrogen being taken from $C^2 H^8$ they ought to be replaced by four volumes of oxygen? Will carbonic acid be ever confounded with protocarburetted hydrogen in the same *chemical* type? certainly not; for all chemists agree, and M. Dumas especially, in admitting that in such a combustion the quantity of oxygen fixed with the carbon depends on the number of the atoms of the latter body; so that, in an organic compound, two atoms of carbon being combined with eight to twenty or any number of atoms of hydrogen, this compound being decomposed by excess of oxygen, there will be only four volumes of oxygen combined with the carbon. Will not M. Dumas admit, that in destroying, as he has done, with excess of chlorine, *chloroform* and *protocarburetted hydrogen*, compounds which both contain two atoms of carbon, he could in fact obtain only chloride of carbon, corresponding to carbonic acid, that is to say $2\,C\,Cl^4 = C^2\,Cl^5$, in the same way as by destroying protocarburetted hydrogen by excess of oxygen, there are obtained $2C\,O^2 = C^2\,O^4$, without resorting to his theory of substitutions?

M. Persoz adds, that it appears to him important to refer to the fact that the formation of pond gas served him as a means of discovering the mysterious agency of water in the reactions of organic bodies. By this decomposition of water, he explains the conversion of starch into sugar, that of sugar into alcohol, and that of certain immediate principles into essential oils; and he also conceives that he can reduce to the same order of phænomena (that of oxidizement) the action of nitric acid and hydrate of potash on sugar, which, as it is well known, is converted by both of these agents into oxalic acid.—*L'Institut*, No. 323.

ON ARSENIC CONTAINED NATURALLY IN THE HUMAN BODY.

M. Orfila has read a memoir on the above subject before the Royal Academy of Medicine; the experiments detailed were made with M. Couerbe, and their object was to solve the following questions:

1st, Does arsenic exist originally in the human body? 2ndly, Do the viscera contain any? 3rdly, Can its existence in the muscles be proved? 4thly, Is it possible to determine that the arsenic obtained from a corpse is not that which originally existed among the elements composing the tissues, but was introduced into the digestive organs, applied to the exterior, &c.?

I. Arsenic exists in human bones; if the bones of an adult be calcined, taking care not to raise the temperature too high, and to avoid contact with the fuel, these bones, when reduced to powder and treated with purified sulphuric acid, and then tried in Marsh's apparatus, will yield brown, brilliant and thick arsenical spots. This result was obtained both from the bones of corpses of adults who had been dead some days, or buried for some months.

When the calcination is effected at a white heat, no arsenic is obtained, nor is any procured from the bones of commerce reduced to a soft paste; but if they be subjected to heat and the processes indicated (nitric acid, potash and sulphuric acid), a certain quantity of arsenic is obtained.

From this first series of experiments, which amount to fourteen, I conclude, says M. Orfila, 1st, That the bones of the human adult, of the horse, ox, and sheep contain minute portions of arseniç, which it is possible to discover by treating the bones with potash purified by alcohol and pure sulphuric acid.

2ndly, This quantity of arsenic is not increased by long burial.

3rdly, Vitrification removes a portion of it, which is undoubtedly occasioned by the volatilization which it occasions.

4thly, Among the conditions favourable to the discovery of arsenic, must be especially reckoned that of not calcining the bones too strongly, and secondly to avoid carefully the contact of fuel.

5thly, When bones are treated with pure water and ebullition, no arsenic is discoverable.

6thly, If in operating in this mode, any arsenic be detected, it has certainly been in some mode introduced into the œconomy.

II. No arsenic is found in the viscera unless it has been absorbed. The organs of a dog which was hung, treated by the usual processes, did not yield any. The blood, brain, the liver, spleen, kidneys, intestines, stomach, &c. gave no traces of it. Carbonized with nitric acid, and afterwards tried in Marsh's apparatus, white opake spots only were obtained, and these were also produced without the presence of these organic matters.

The liver of an adult gave none; nor did the decoctions made with various organs yield any.

From these facts we may conclude, observes M. Orfila, but not positively, that the viscera do not originally contain arsenic; or to state the fact more accurately and not to prejudge the case, it may be asserted, that they do not yield any when treated with boiling water, sulphuretted hydrogen, or when carbonized by concentrated nitric acid, &c. It may so happen that the quantity is too small to be detected by sulphuretted hydrogen, or that it is lost by carbonization; but by acting on a large quantity of brain or other organs, it may be detected. At any rate, it is sufficient at present to have ascertained, that the viscera yield no arsenic by the reactions described, unless it has been introduced by poisoning.

III. It is not proved that muscular flesh contains arsenic: twelve pounds of it taken from the corpse of an adult, carbonized by nitric acid and tested by Marsh's apparatus, gave white opake spots; some were brilliant, with a blueish tint; others were yellow, and had an arsenical appearance; dissolved in boiling nitric acid, they gave no alliaceous smell when put on red-hot charcoal; in fact, they possessed none of the characteristics of arsenic. These spots were, however, very numerous; submitted for nearly twenty days to a current of sulphuretted hydrogen gas, they gave no indication of arsenic. It is possible that they were a mixture of arsenic and animal matter, and that the muscular flesh of two or three bodies might yield some by analysis; lastly, other processes may discover it in the same quantities as those employed, by occasioning less loss; therefore, adds M. Orfila, I will not conclude, positively, that arsenic does not exist in muscular flesh.

IV. It is possible to ascertain that the arsenic which may be discovered does not come from the organic substance itself, but that it has been combined with it by absorption. For if it be found in the bones, it will not be removed by long boiling in water, unless it had been introduced; and the same holds good with respect to the blood and the organs which have been examined.

Lastly, if the muscles yield spots, some of which resemble arsenic at first sight, the distinctive characters which have been stated must be remembered; and if the subject had taken arsenical remedies, this circumstance ought to be particularly attended to.—*Journal de Chim. Med.*, Dec. 1839.

METEOROLOGICAL OBSERVATIONS FOR FEB., 1840.

Chiswick.—Feb. 1. Cloudy: rain. 2. Very fine. 3. Rain. 4. Boisterous with rain. 5. Rain: clear. 6. Rain: cloudy. 7. Rain. 8. Heavy showers. 9. Fine. 10. Heavy showers: clear and very fine at night. 11. Fine. 12. Rain. 13. Very fine. 14. Foggy. 15. Frosty: rain. 16. Hazy and mild. 17. Dense fog. 18. Dry cold haze. 19. Clear and cold. 20. Cloudy, with some snow-flakes falling. 21. Bleak and cold. 22. Overcast. 23. Cold and dry. 24. Fine but cold. 25. Frosty haze. 26. Cold haze. 27. Cloudy, cold and dry. 28, 29. Fine but cold.

Boston.—Feb. 1. Cloudy: rain P.M. 2. Fine: rain early A.M. 3. Stormy: rain early A.M. 4. Stormy: rain early A.M.: rain P.M. 5. Cloudy: rain P.M. 6. Cloudy. 7. Cloudy: rain early A.M.: rain P.M. 8. Fine: rain and snow P.M. 9. Fine. 10. Rain. 11. Fine. 12—15. Fine: rain P.M. 16, 17. Cloudy. 18. Fine. 19. Cloudy: snow A.M. and P.M. 20. Cloudy: snow A.M. 21. Cloudy: snow melted. 22. Cloudy. 23—27. Fine. 28. Cloudy. 29. Fine.

Applegarth Manse, Dumfries-shire.—Feb. 1. Frequent showers. 2. Frequent showers: snow gone. 3. Frequent showers. 4, 5. Shower A.M.: fair rest of the day. 6. Rain very early: fine day. 7. Heavy rain A.M.: stormy P.M. 8. Occasional showers of rain and hail. 9, 10. Occasional showers of rain and hail with high wind P.M. 11. Fine day: a few drops of rain. 12. Storm of wind and rain P.M. 13. Fine day: no rain. 14. Fine day, but cloudy. 15. Wet morning: cleared up P.M. 16. Calm, cloudy, and mild. 17, 18. Fine A.M.: grew cloudy and sharp. 19. Cold easterly wind, but fair. 20. Cold easterly wind with slight frost and snow showers. 21. Cold easterly wind: frost: threatening snow. 22—24. Cold easterly wind: still frosty: sprinkling snow. 25. Cold easterly wind. 26. Beautiful sunny day, but still frosty. 27. Beautiful sunny day: frost very keen. 28. Cloudy all day: but still freezing. 29. Fine frosty day.

Sun shone out 25 days. Rain fell 13 days. Snow 2 days. Frost 10 days.

Wind north-easterly 6 days. Easterly 3½ days. South-easterly 7 days. Southerly 3½ days. South-westerly 8½ days. West 1 day.

Calm 14½ days. Moderate 8 days. Brisk 1½ day. Strong breeze 2 days. Boisterous 3 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
Feb.		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·338	29·363	29·270	29·07	29·29	29·14	43·7	44·6	40·4	47	48	41	38½	32	SE.	S.	SE.	NE.	·038	·20	...	...	41
2.	29·354	29·416	29·349	28·86	29·04	28·95	41·2	41·8	39·9	50	40	41	41	35	W.	SW.	W.	S.	·166	·09	·18	...	40
● 3.	29·174	29·191	28·988	28·80	28·89	28·86	43·8	44·4	41·2	49	44	42·5	40	39	S.	SW.	S.	ESSW.	·016	·18	·02	...	41
4.	28·648	28·802	28·597	28·40	28·73	28·79	44·8	45·6	40·2	47	40	41	40	33	S. var.	S.	SE.	NE.	·133	·30	·08	...	40
5.	29·104	29·656	29·116	28·60	29·10	29·44	42·3	45·7	41·5	46	35	42	40½	38	W.	NW.	calm	NNE.	·344	·02	·14	...	40
6.	29·754	29·857	29·759	29·25	29·40	29·52	39·8	45·7	38·4	50	34	39	40	33½	SW.	W.	calm	W.	·061	·02	·06	1·29	37
7.	29·596	29·616	29·449	29·18	29·07	28·74	46·9	48·0	39·0	53	40	43	44½	37	S.	SW.	SW.	SW.	...	·03	·08	...	41
8.	29·532	29·682	29·559	29·05	29·06	29·25	43·8	51·0	41·0	47	32	41	38	35	W.	SW.	W.	WSW.	·075	·02	·07	...	40
9.	29·724	29·759	29·722	29·27	29·32	29·10	38·0	38·7	35·5	47	43	34	45	32	S.	NW.	calm	S.	·066	...	·09	...	37
☽ 10.	29·580	29·796	29·588	29·10	29·05	29·34	47·4	48·4	37·2	51	37	38	46	40	S. var.	SW.	SW.	SW.	·013	·13	·04	...	42
11.	29·950	29·975	29·945	29·50	29·57	29·52	42·2	42·8	40·5	52	45	38	47	37	S.	SW.	W.	SW.	·091	...	·04	..	40
12.	29·724	29·781	29·715	29·26	29·29	29·18	47·4	48·3	41·9	50	35	46	47	42½	S.	SW.	SW.	S.	...	·02	...	..	42
13.	29·838	29·927	29·826	29·32	29·49	29·69	43·0	43·5	41·6	53	28	41	44	37½	S.	SW.	SW.	SW.	...	...	·04	...	41
14.	29·956	29·966	29·957	29·55	29·78	29·72	35·2	50·2	35·3	47	26	35·5	44	32	S.	SW.	calm	SW.	...	...	...	1·33	38
15.	29·848	29·880	29·733	29·43	29·45	29·41	37·7	39·5	33·8	46	40	39	46	35	S.	SW.	calm	SW.	...	...	...	...	35
16.	29·772	29·954	29·796	29·30	29·55	29·80	46·7	47·3	37·0	51	44	47	47½	44	SW.	SW.	calm	SW.	·150	·18	·11	...	40
○ 17.	30·004	30·162	30·008	29·48	29·99	30·10	48·2	50·8	46·0	48	32	45	46	40	SW.	SE.	E.	E by S.	...	...	...	...	41
18.	30·244	30·306	30·240	29·90	30·15	30·22	37·7	49·0	36·0	42	32	38	40½	32½	NE.	E.	E.	E by S.	·055	·06	...	...	35
19.	30·324	30·437	30·335	30·	30·31	30·38	35·2	41·2	35·2	36	30	36	36½	33	N.	E.	calm	E by S.	...	...	...	...	29
20.	30·450	30·515	30·475	30·11	30·45	30·45	33·5	36·6	32·8	33	30	33	36	30	N.	NE.	calm	E by S.	·016	...	·05	...	28
21.	30·452	30·480	30·417	30·16	30·44	30·38	32·3	33·4	32·0	34	28	32·5	34½	30½	NE.	E.	calm	SE.	...	...	·28	0·0	26
22.	30·324	30·362	30·286	30·10	30·30	30·28	32·7	33·8	31·0	34	24	33	36½	29	NE.	NE.	E.	E.	...	...	...	...	24
23.	30·224	30·287	30·265	30·	30·28	30·28	29·5	34·6	28·0	36	24	35	39	29	NE.	E.	E.	E.	...	...	...	...	25
24.	30·376	30·547	30·407	30·15	30·35	30·46	30·7	35·0	27·8	41	26	33·5	38½	30½	NE.	E	E.	E.	...	...	...	...	23
☾ 25.	30·596	30·661	30·609	30·27	30·50	30·50	35·3	37·4	30·7	44	25	35	34½	29	NE.	E.	E.	E by S.	...	...	...	...	30
26.	30·596	30·637	30·563	30·33	30·53	30·48	35·2	35·6	32·8	40	32	35	34	27½	NE.	E.	E.	E by S.	...	...	...	...	27
27.	30·468	30·549	30·438	30·15	30·44	30·36	34·7	35·3	33·6	40	32	33	30	22	ENE.	NE.	E.	NE.	...	...	...	...	27
28.	30·316	30·359	30·271	30·	30·30	30·31	34·6	35·2	33·6	45	32	34	32½	25	N.	NE.	N.	ENE.	...	...	...	0·0	31
29.	30·356	30·414	30·369	30·05	30·35	30·40	36·0	36·7	33·6	45	28	38	34	26	NE.	E.	E.	ENE.	...	...	...	..	32
Mean.	29·918	30·011	29·898	29·53	29·740	29·795	39·3	42·4	36·5	44·96	34·00	38 2	40·3	33·3					Sum. 1·224	1·25	1·28	2·62	Mean. 34·9

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LVII. *On the Boulder Formation, or drift and associated Freshwater Deposits composing the Mud-cliffs of Eastern Norfolk.* By Charles Lyell, *Esq., V.P.G.S., F.R.S., &c.*[*]

THE cliffs extending from Happisburgh or Hasborough light-house to near Weybourne, north-west of Cromer, in Norfolk, comprising a distance of about 20 miles, are designated in some maps as "the mud-cliffs." They are for the most part composed of deposits of two kinds, first, stratified and unstratified drift, called by some "diluvium;" secondly, freshwater strata. Both of these rest on chalk, which is usually concealed below the level of the sea. Occasionally between the chalk and drift or the chalk and freshwater beds, a thin layer is found of marine crag, agreeing in its fossils with that of Norwich, but occurring only in patches of small extent, except near Weybourne, where it is more continuous.

The drift, which sometimes attains a thickness of more than 300 feet, consists principally of clay, loam, and sand, in some places stratified, in others wholly devoid of stratification. Pebbles, and in some places large boulders of granite, porphyry, greenstone, lias, chalk, and other transported rocks are interspersed, especially in the unstratified portion. Pure and unmixed white chalk rubble, and even huge fragments of solid chalk, are also associated in some localities. No fossils have been detected in this drift which can positively be referred to the æra of its accumulation; but besides the organic remains derived from secondary strata, it contains almost everywhere broken fragments of shells which agree in species with those of the Norwich crag, from which there is good reason to believe them to have been washed out.

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[*] Communicated by the Author.

The freshwater strata associated with the boulder formation above mentioned occur for the most part in patches at the bottom of the drift, and immediately above the subjacent chalk and crag, where the latter is present, as may be seen at a variety of places between Happisburgh and Runton near Cromer. The two spots where it is most largely developed are at Mundesley and Runton. At the latter place it underlies the drift and rests immediately on chalk, with the occasional intervention of marine crag; while at Mundesley it occupies for a certain space the whole cliff, taking the place as it were of the drift, and appearing in part to be of contemporaneous origin, and in part of subsequent date and superimposed. Everywhere it contains the same species of shells, and as these are almost without exception identical with well-known British species, it is evident that the entire formation of the mud-cliffs, whether freshwater or drift, belongs to the latest part of the tertiary period, the only doubt being whether it should not rather be considered as post-tertiary or referable to a class of deposits which contain exclusively shells of recent species.

I mention at once this conclusion, because the recent origin of the drift adds a peculiar interest to the great derangement and change of position which it has undergone since its deposition. In no other parts of our island, or perhaps of Europe, are there evidences of local disturbance on so great a scale and of an equally modern date, for there are proofs of the movement both downward and upward of strata several hundred feet thick for an extent of many miles; together with most complicated bendings and foldings of the beds and the intercalation of huge masses of chalk, and what is no less perplexing and difficult of explanation, the superposition of contorted upon horizontal and undisturbed strata.

The line of coast in which the formations above alluded to are displayed was well described in Mr. R. C. Taylor's Geology of East Norfolk, published in 1827, and afterwards in Mr. Woodward's Outline of the Geology of Norfolk, 1833. Both these papers are the result of a careful survey of the coast, and contain original observations of great merit. In both are given coloured sections of the cliffs, from which a good general idea of their structure and composition may be derived. A memoir was also read to the Geological Society in January 1837, by the Rev. W. B. Clarke, in which among other remarks he insists on the necessity of separating the diluvium of Norfolk from the crag *.

My own observations on the coast of East Norfolk were made first in the year 1829, and afterwards in 1839; and as

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* Geological Transactions, 2nd series, vol. v. part ii. p. 363.

the sea has been continually encroaching on the cliffs, I found after an interval of ten years that a different section of the same beds was exhibited, and some difficulties cleared up which I had been unable to explain on my first visit. Nevertheless a high beach precluded me at both periods from obtaining a view of the lowest beds, which are sometimes exposed in winter at low water, and after storms. During my last visit in particular (1839) the prevalence of easterly gales prevented my seeing in some places no less than 12 feet in vertical height of the section which was visible in the summer of 1829.

The principal deposit which constitutes the mud cliffs of Eastern Norfolk is strictly analogous in character to that which has been called the "boulder formation" in Denmark and Sweden, and which, from the numerous erratics included in it, forms so remarkable a feature in the superficial geology of Scandinavia, and all the countries surrounding the Baltic, as well as northern Russia. It may be said to extend uninterruptedly from Sweden through the Danish islands, Holstein, and the countries of Hamburgh, Bremen, and Osnabruck, to the borders of Holland, and then to appear again with the same characters in Norfolk and Suffolk. Throughout this tract, however, the average number and dimensions of the included erratic blocks, especially those of granite, porphyry, gneiss, and other crystalline rocks, diminishes sensibly on proceeding from north to south.

As I am of opinion that the boulder formation in all these countries has been accumulated almost exclusively on ground permanently submerged beneath the waters, and that it does not consist of materials transported either by one or many transient rushes of water over land which had previously emerged, I shall dispense as far as possible with the term "diluvium," substituting that of "drift" for such portions of the deposit which cannot be proved to be fresh-water. Part of this drift consists of clay and loam wholly devoid of stratification, to which the name of "till" may be applied, a provincial term widely used in Scotland for similar masses of unstratified matter, which there also contain most commonly included boulders. The entire want of a stratified arrangement in the *till*, whether in Scandinavia, Scotland, or Norfolk, implies some peculiarity in its mode of origin; yet in all these countries some of the till has accumulated contemporaneously, and apparently in the same body of water, as much of the accompanying stratified gravel, sand, and clay. Moreover the stratified drifts are often identical in composition with the *till*, the distinction consisting merely in the mode of arrangement.

I have seen no kind of deposit now in progress precisely similar in character to the till, except one, namely, the terminal moraines of glaciers. These, as Charpentier has justly remarked, are entirely devoid of stratification, because the accumulation has taken place without the influence of any currents of water by which the materials would be sorted and arranged according to their relative weight and size. Year after year the ice, as it melts at the extremity of a glacier, adds fresh mud, together with fine and coarse sand, gravel, and huge blocks, to the moraine, all being carried to the same distance*, without the least reference to the volume or specific gravity of the component particles or masses.

There can be no doubt that similar accumulations must take place in those parts of every sea, where drift ice, into which mud, sand, and blocks have been frozen, melts in still water, and allows the denser matter to fall tranquilly to the bottom. The occasional intercalation of a layer of stratified matter in the till, or the superposition or juxtaposition of the same, may be explained by the existence or non-existence of currents, during the melting of the ice, whether successively in the same place or simultaneously in different places.

It is, I believe, a common error of those who are not unwilling to admit the agency of ice in reference to the larger fragments of transported rock, to forget that what carries heavier masses from place to place must unavoidably convey a much larger volume of lighter and finer materials.

Having offered these preliminary remarks, I shall proceed to describe in detail some of the appearances which present themselves to one who travels along the coast from Hasborough to Weybourne. The section of the mud cliffs begins at the more southern of the two lighthouses about a mile and a half south of Hasborough. The cliffs here, which are between sixteen and twenty feet in height, are composed generally of a mass of blue clay covered with yellow sand, the clay and sand being both stratified in some places with great regularity, but in others the clay or mud is quite unstratified. Included in this *till* I found pieces of unrounded white chalk, angular chalk flints, fragments of argillaceous limestone (lias ?), blocks of dark greenstone, and other rocks. There are also interspersed pieces of shells, apparently belonging to *Cyprina*, *Cardium*, *Mactra*, and *Tellina*, such as might have been derived from the denudation of the Norwich crag. At some points where stratified clay reposes on the till, the surface of the latter is very uneven, and was evidently so when the superior deposit was thrown down upon it. Examples of intercalation of till between laminated beds of clay and loam are not unfrequent. In 1839, I saw at a spot between the two lighthouses, till resting on stratified clay and covered by stratified gravel and white chalk rubble, which latter formed the top of the cliff.

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* *Ann. des Mines*, tom. viii.

Owing to the continual dilapidation of the cliffs the details of the sections seen by me in 1829 and 1839 were very different. During my last visit the beach at Hasborough was too high to allow me to see the fundamental bed of lignite which exists there, which in June 1829 was exposed at low water, the descending section being then as follows: 1st, sand and loam, 13 feet; 2ndly, unstratified mud or till varying from 8 to 16 feet; 3rdly, laminated sand and clay, one foot and a half, part of the clay being bituminous and inclosing compressed branches and leaves of trees. The clays, which were blackish, greenish or brown, contained occasional layers of small pebbles, rounded and angular, mostly of chalk flint. The entire height of the cliff was about 35 feet.

This locality has been mentioned by various authors as the principal site of the submarine forest of East Norfolk, which has been described as occurring about the level of low water; and Mr. R. C. Taylor observes of this deposit generally,—

> " That it consists of forest peat, containing fir cones and fragments of bones; in others of woody clay; and elsewhere of large stools of trees standing thickly together, the stems appearing to have been broken off about 18 inches from their base. They are evidently rooted in the clay or sandy bed in which they originally grew, and their stems, branches, and leaves lie around them, flattened by the pressure of from 30 to 300 feet of diluvial deposits. It is not possible to say how far inland this subterranean forest extends; but that it is not a mere external belt is obvious from the constant exposure and removal of new portions, at the base of the cliffs *."

A letter of the Rev. James Layton is thus cited by Mr. Fairholme:—

> " One remarkable feature in this compact blue clay is a stratum of wood, exhibiting the appearance of a wood overthrown or crushed *in situ*. At Paling the stumps of trees seem now to be really standing, the roots are strong, spread abroad, and intermingling with each other; were a torrent to sweep away the mould from the surface of a thick wood, leaving the roots bare in the ground, the appearances would be exactly the same. This phænomenon occurs again at Hasborough: the line of crushed wood, leaves, grass, &c., frequently forming a bed of peat, extends just above low-water mark. About this stratum are found numerous remains of mammalia, the horns and bones of at least four kinds of deer, the ox, the horse, hippopotamus, rhinoceros, and elephant. These fossil remains are found at Hasborough and its neighbourhood on the denuded clay shore: at Mundesley they are found in the cliff. The great mine, however, is in the sea, some miles from land, where there is an oyster bed on a stratum of gravel about six fathoms deep. How far this bed of fossils extends, I cannot pretend to say, but in 1826 some fishermen while dredging for soles

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* Geology of E. Norfolk, p. 21.

on 'the knowl,' a bank 20 miles off shore, brought up an entire tusk of an elephant nine feet six inches long. The elephants must have been abundant; I have at least 70 grinders, and the oyster dredgers reported that they had fished up immense quantities and thrown them into deep water, as they greatly obstructed their nets *."

Mr. Woodward had previously spoken of the same oyster bed, which was discovered off Hasborough in the year 1820, and says that during the first twelve months, many hundred specimens of the molar teeth of the elephant were dredged up by the fishermen, and that the remains of upwards of 500 animals must have been found there †.

I was not so fortunate either here or elsewhere on this coast as to see the stools of trees erect in this stratum, but so many independent eye-witnesses have lately described them to me with such minuteness as to leave in my mind no doubt of the fact. Besides the accounts of several fishermen, Mr. Simeon Simons of Cromer states, that at Cromer he saw ten or more trees in the space of half an acre exposed below the cliffs eastward of that town, the stumps being a few inches, or all less than a foot in vertical height, some of them no less than 9 or 10 feet in girth, the roots spreading from them on all sides throughout a space twenty feet in diameter. Many others were seen by him laid open on the beach opposite Sidestrand, about three miles further to the eastward, evidently belonging also to a submerged forest. All these roots were in a laminated blue clay, with associated blue sand, the whole, six or seven feet thick, resting on chalk. In one place a thin layer of Norwich crag intervenes between the chalk and the bed of blue clay with lignite. Shells had been found immediately below the roots, but I have been unable to obtain them.

I ascertained that at Woolcot Gap, between Hasborough and Bacton, the bed of lignite, containing the bones of elephants, pieces of wood, and the roots of trees *in situ*, had been exposed at the base of the cliff in the preceding winter of 1838–39. A mass of incumbent drift about 30 feet thick must have been removed by the waves and currents, in order to lay open this lignite on the spot alluded to, and the great extent of the submerged forest is proved not only by the numerous points between Paling and Runton, which are about 18 miles apart, reckoning by the sea-coast, and nearly as far in a direct line, but also by the proofs afforded of its extension inland in proportion as the overlying beds are swept away by denudation.

It follows then from the facts above stated that the chalk in this region had been overspread with layers of sand and clay, and converted into dry land, on which forest trees lived; these were afterwards submerged, broken off short near their roots, and buried with their branches and leaves. The subsidence implied by the submergence of the forest continued afterwards, so as to allow of the superposition of a considerable thickness of stratified and unstratified drift.

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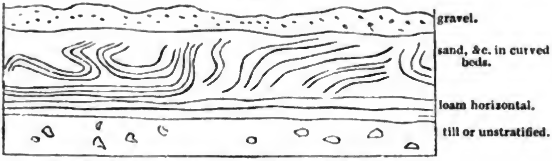
* Rev. James Layton, cited in Fairholme's Geology, p. 281.

† Geol. of Norfolk, pp. 7 and 23.

The general character of the cliffs between Hasborough and Bacton Gap, a distance of about three miles, may be thus described: first, at the bottom, the lignite or forest bed, a few feet thick; secondly, above this, blue argillaceous till, containing boulders of granite and quartz and small pieces of shells from the Norwich crag; thirdly, laminated blue clay resting on the till; fourthly, stratified yellow sand, the entire height of the cliffs being between 30 and 40 feet.

The cliffs between Bacton Gap and Mundesley, a distance of about three miles, are higher, but consist in like manner of drift containing a great variety of boulders, this being usually the lowest bed which is visible. Here we first meet with fine exemplifications of strata which have undergone great derangement since their original deposition, and which present that most perplexing phænomenon the superposition of bent and folded beds upon others which appear to have undergone no dislocation. Thus for instance the annexed section (fig. 1.) represents a cliff about 50 feet high, at the bottom of which is *till* containing boulders, having an even horizontal surface on which repose beds of laminated clay and sand about 5 feet thick, which in their turn are succeeded by vertical, bent, and contorted beds of sand and loam 20 feet thick, the whole being covered by flint gravel. Now the curves of the various coloured beds of loose sand, loam, and gravel are so complicated, that not only may we sometimes find portions of them which maintain their verticality to a height of 10 or 15 feet, but the replication is often such that a continuous seam of fine loose sand between two layers of gravel or loam might be pierced three times in one perpendicular boring. As it is clear that some of the underlying

Fig. 1.



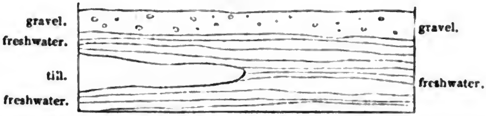
Cliff 50 *feet high between Bacton Gap and Mundesley.*

horizontal beds, and apparently the till also, of which the surface is so even, have not participated in these movements even in the smallest degree, we are compelled to suppose that some lateral force has been exerted against the upper masses of drift which has not been applied to the lower ones. Yielding beds having a thickness of at least 15 or 20 feet must in some cases have been subjected to this sideway pressure and moved bodily; and it is impossible to conceive that any original irregularity in the mode of deposition, nor any shrinking or settling of the materials, nor anything in short but mechanical violence, could have produced such complicated folds. Commonly we are in the habit of attributing such movements to a subterranean force acting from below, but it is difficult to imagine how such agency could have disturbed the overlying beds without affecting the subjacent. I shall defer to the sequel the consideration of the various hypotheses which may be suggested to account for such appearances, first describing other irregularities and apparent anomalies which present themselves in this same line of cliffs.

At one spot between Bacton and Mundesley, where the cliff is 50 feet high, I observed at a depth of 30 feet from the top, a small pit or furrow as it were cut into strata of blue clay, and filled with fragmentary white chalk and chalk flint, regular strata of sand and loam being superimposed. This indentation was four feet deep and six wide, and precisely resembles those irregularities which we see in superficial gravel; and they may all be explained if we suppose, that during the subsidence which is indicated by the buried forest, the drift of the mud cliffs was formed in very shallow water, so as to be exposed to the denudation of small streams or currents, by which narrow grooves and hollows were excavated, then filled with drift, and then, after the sinking of the whole, overspread with regular strata.

Freshwater Strata at Mundesley. — Both to the north and south of Mundesley, the cliffs, varying in general from 40 to 70 feet in height, consist in their lower part of blue clay or till, covered with stratified yellow sand and loam; but at the town of Mundesley itself the cliff lowers to a height of between 20 and 30 feet, and for several hundred yards is occupied by a freshwater deposit, covered with about 10 feet of flint gravel. The freshwater beds consist of brown, black, and grey sand and loam, mixed with vegetable matter, sometimes almost passing into a kind of peaty earth containing much pyrites. A few layers also of gravel occur composed of rounded flint pebbles. These beds are often irregular and rarely continuous for great distances. The bottom of the deposit is unseen, but is probably not much below the level of the sea, as Mr. Simons of Cromer tells me that he has seen chalk *in situ* at Mundesley at low water. In 1829 I observed a mass of the till prolonged in such a way into the freshwater formation at the southern junction of the latter with the drift as to imply the contemporaneous origin of the lower part at least of both formations. (See diagram, fig. 2.)

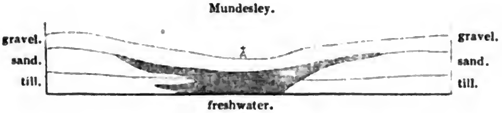
Fig. 2.



Interstratification of drift and freshwater at Mundesley.

Yet I inclined then to the conclusion that the Mundesley formation, which I traced for nearly 300 yards along the coast, might, as a whole, be considered as the deposit of a lake or hollow excavated in the drift. In 1839, when a new section had been laid open by the sea, it appeared to me rather that these strata of Mundesley were simply a large development of those observable at the base of the cliff at Hasborough and other places, and that their position relatively to the drift might be represented by the diagram, fig. 3.

Fig. 3.



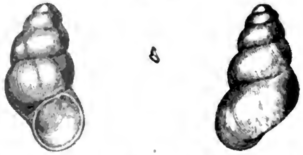
Position of the freshwater beds at Mundesley.

We may imagine that while the coast was sinking gradually, a small river may have entered here bringing down drift wood, freshwater shells, mud, and sand, and the flow of the stream may have partially counteracted those causes by the influence of which the boulder formation was accumulating in the spaces immediately contiguous. The following is a list of the shells which I obtained from the Mundesley beds, including some which Mr. Fitch of Norwich, and others which Mr. J. B. Wigham have kindly communicated to me: 1. *Paludina impura*; 2. *P. minuta*, Strickland, (see fig. 4); 3. *Valvata cristata*; 4. *V. piscinalis*; 5. *Limnea glutinosa*; 6. *L. peregra*; 7. *Planorbis albus*, var. (less flat, and aperture less oblique than the common form); 8. *P. vortex*; 9. *P. lævis*, Alder, Newcastle Trans.; 10. *Cyclas pusilla*; 11. *C. cornea*.

Specimens of *Unio* or *Anodon* occur, but too imperfect to be determined; one however resembles *A. cygneus*.

Of the eleven shells above enumerated, the only one which is unknown as a living species is the *Paludina minuta*, found by Mr. Strickland in a freshwater deposit at Crophorn, in Worcestershire, and also by Mr. Wood at Stutton on the Stour in Suffolk. Mr. George Sowerby, who has examined this species for me, finds that among recent species it agrees most nearly with the *Turbo thermalis*, Lin., as its volutions are exactly *four*: its apex is more obtuse and its volutions are more ventricose than in other recent species, and it is constantly smaller. (See fig. 4.)

Fig. 4.



Paludina minuta, from the freshwater beds at Mundesley; the middle figure is of the natural size.

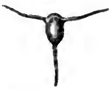
Insects.—The elytra of beetles are not uncommon in the clay of Mundesley, especially those of the genus Donacia, a tribe which frequents marshy grounds. The beautiful green and gold colours of these wing cases are almost as bright when first the clay is removed as in the living insect, but they soon lose a great part of their lustre on exposure to the light. Mr. Curtis, to whom I am indebted for an examination of these fossils, says that there appear to be two species of Donacia, (one of them *D. linearis*?) both probably identical with recent British insects; and among the other remains he found the thorax of an Elater, and the elytron of one of the Harpalidæ (*H. ophonus* or *H. argutor*). He also refers with confidence another elytron to *Copris lunaris*, a British beetle.

Fish.—I found many scales of fish, together with one large tooth, at Mundesley, and Mr. J. B. Wigham sent me similar remains, together with a smaller tooth, and some vertebræ and ribs of fish. These I submitted to the Rev. Leonard Jenyns and Mr. Yarrell, who referred them to the genera Perch, Carp, Pike, and Trout. The pike appears both by the teeth and scales to be the common *Esox lucius*. Of the salmo there were several small and one large scale, in which the concentric striæ of growth were extremely minute; the species was not determined. The scales of the perch were very numerous, but they did not agree in a satisfactory manner with those of the common British *Perca fluviatilis* which we were able to procure. The cilia of the free edges were proportionally smaller and blunter in the fossil, and the divisions in the fan at the basal extremity were longer and more numerous. But a more extensive comparison might perhaps have enabled us to identify these fossil scales with those of the living European perch. Most of the vertebræ and ribs may probably belong to this same fish.

Mammalia. —I was informed at Mundesley, that many years ago, when a zigzag road was cut down to the beach, the horns of the Irish elk were found in the cliffs, but I know not where they are preserved and by what naturalist they were seen, nor whether they were found in the freshwater deposit, as is most probable, or the overlying gravel.

Plants.—Among the vegetable fossils the most common and best preserved are the seed-vessels of an aquatic plant which Mr. R. Brown refers to *Ceratophyllum demersum*, English Botany, 947; (see fig. 5.) and I learn from Mr. J. B. Wigham, that his father considers the remains of the accompanying trees and shrubs to be those of the oak, alder, fir, and bramble; but more specimens will be required before a perfect reliance can be placed on these last determinations.

Fig. 5.



Seed vessel of *Ceratophyllum demersum;* Mundesley.

Between Mundesley and Trimmingham the cliffs are composed as usual of drift, the upper part being stratified and more sandy, the lower part consisting of till or blue clay with pieces of white chalk. Whether there intervenes everywhere between this drift and the fundamental chalk a bed of lignite like that of Hasborough or Mundesley, I was prevented from ascertaining by the height of the beach, but I found a substratum of this kind with numerous flattened leaves and branches at the base of a cliff 70 feet high about a mile northwest of Mundesley.

Protuberances of chalk near Trimmingham.—We have now followed the mud cliffs for a distance of about eight miles without finding any chalk *in situ* above the mean level of the sea, but near Trimmingham are three remarkable protuberances of chalk which rise up and form a part of lofty cliffs, the remainder of which consists entirely of drift. These detached masses or outliers of chalk were noticed in Mr. Greenough's map of England, and are described by Mr.

R. C. Taylor, who had opportunities of observing them at low tide as being continuous with the solid bed of chalk extending under the sea for nearly a mile from Trimmingham to Sidestrand, constituting everywhere under water a level platform. He also says that the chalk of this platform contains throughout parallel strata of flint, is harder than that of Cromer or Norwich, is characterized by several peculiar fossils, and occupies, he thinks, a higher place in the series than the chalk at Norwich *.

Now the platform here alluded to is evidently what the sea has left after sweeping away by gradual denudation all that once rose above low water, and it is therefore impossible for us now to conjecture to what height the chalk thus removed may once have risen. The most southern of the three protuberances before mentioned occurs near the Beacon hill, about half way between Mundesley and Trimmingham, and it is in contact with stratified drift the beds of which are highly inclined. The mass of chalk is about 20 feet in height, its extent along the beach about 100 feet, and its thickness from the beach inland a few yards only. It stands up like a narrow wall, which will ultimately be destroyed, and then the whole face of the cliff will consist of clay sand and gravel.

The surface of this wall of chalk, where in contact with the drift, dips inland at an angle of about 45°, and the beds of the newer deposit conform to this slope. As the chalk offers more resistance to the waves than the drift, a small promontory is produced at this point, which projects about 40 feet beyond the general coast line, and by aid of this promontory we are able to see the junction of the chalk and newer beds, both on the north and south side, so that the relative position of the two formations is very clearly ascertained (see fig. 6.)

When I visited this spot in 1829, I found the cliff nearly in the same state as it remained in 1839, and the description which I gave of it in the Principles of Geology would still be appropriate †.

But when last there I was able to examine the entire structure of this cliff more thoroughly, and I was more fully confirmed in my opinion that both the chalk and incumbent formation, for the thickness of several hundred feet, must have been subject to some common movement, whether sudden or gradual, by which the strata of both have been tilted.

The annexed view of the promontory (fig. 6.) was taken from a point on the sloping cliff a few hundred yards to the south, where the beds have already recovered their hori-

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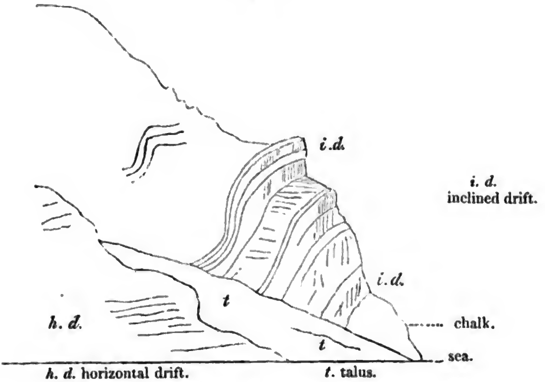
* Geol. Trans., vol. i. 2nd series, p. 376.

† Vol. iii. 1st edit. p. 179, or 5th edit. vol. iv. p. 85.

zontality, although they seem to correspond to the beds of

Fig. 6.



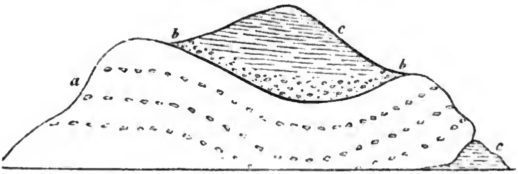
Southernmost protuberance of chalk, Trimmingham.

sand and clay which are so highly inclined near their point of contact with the chalk. In the diagram it will be seen that from the projecting point of chalk the cliff retires in a series of ledges and small precipices in which inclined beds of drift, *i, d*, are exposed for an aggregate thickness of several hundred feet. At the top of the cliff, which I conjecture to be about 400 feet above the sea, the beds of sand seemed to be horizontal, but these it should be observed are not immediately over the inclined beds. Respecting the tilted beds which are in contact with the chalk, it will be sufficient to say that they consist of gravel, sand, clay and loam like the stratified drift before described, that the clays are occasionally finely laminated, and that broken fragments of Norwich crag shells are dispersed through some of the strata; but there are no signs here of the freshwater or lignite beds.

The second or middle protuberance of chalk is near that last described: its front along the shore measured in 1839, 65 yards. Its height was between 15 and 20 feet.

The third and most considerable mass extends along the beach for a distance of 106 yards, (see fig. 7.) and its position deserves particular notice, for it forms like the southernmost mass a projecting promontory about thirty yards beyond the general line of cliff. On both sides of this promontory it is seen that the beds of gravel, clay and sand which abut against the wall of chalk are vertical, (see diagram, fig. 8.) yet the beds of the same formation have but a moderate inclination

Fig. 7.

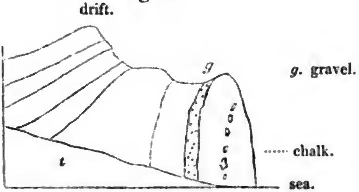


Northern protuberance of chalk, Trimmingham.

a. Chalk with flints.
b. Gravel of broken and half-rounded flints.
c. Laminated blue clay.

in the lofty cliff behind. A layer of chalk flints *in situ* shows that the stratification of the chalk itself is nearly vertical at least in one place, although the beds seen in a large cave facing the sea show a slight curvature only. Where the chalk joins the drift on the southern or Mundesley side of the promontory, I observed in 1839, at the junction, 1st, a portion of the chalk itself decomposed, then a vertical bed of gravel, (*g*, fig. 8) 30 feet high and several feet thick, then dark blue clay with white chalk pebbles, then sandy, and then other beds of ordinary drift. Some of these disturbed beds contain fragments

Fig. 8.



t. talus. *Side view of the same.*

of marine crag shells, as *Cyprina*, *Cardium*, *Tellina*, &c. I have stated in the Principles[*] that this mass of chalk at its northern edge, or towards Trimmingham, actually overlies some beds of blue clay or drift as at the right hand extremity of fig. 7. Now this remarkable superposition was still evident in June 1839, notwithstanding the unusual height of the sea beach, the clay, containing broken chalk flints, being traceable for seven feet under the chalk. It is known to have extended formerly much further in a seaward direction. It appeared to me impossible that any landslips or movements of the present cliffs could have given rise to this inverted position of the chalk and newer formation. Some persons employed in the Preventive Service assured me that the cliffs immediately above and behind this chalk are upwards of 400 feet high, but they appeared to me less elevated. They also said that in digging a well at Trimmingham at the top of the cliff they reached chalk at a depth of 120 feet from the surface. Without insisting on the precise accuracy of their measurements, I think it by no means improbable that the three protuberances of chalk may belong to a much larger mass, which still forms the nucleus of, the hill called Trimmingham Beacon, and I have no doubt, that as the sea encroaches, the chalk will eventually occupy more of the cliffs between Trimmingham and Cromer.

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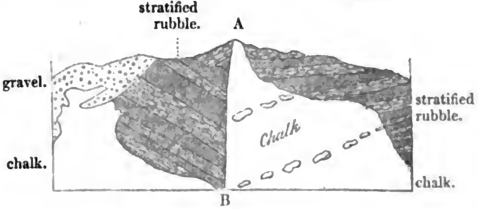
[*] Vol. iii. p. 180, 1st edit., and vol. iv. p. 86, 5th edit.

In like manner it may be observed that in other localities further to the north these masses of chalk are included in drift, or where strata of white chalk rubble enter largely into the composition of the cliffs we always find the chalk cropping out in the interior at a short distance from the shore.

In speculating on the time when, and the manner in which, the protuberances of chalk near Trimmingham have been brought into their present position, we may safely assume that the event happened after the deposition of the greater part of the drift, which has been subjected to precisely the same movements, and abuts in some places in vertical beds against the wall of displaced chalk. As the submerged forest before mentioned occurs both to the north and south of Trimmingham at about the level of low water, we must suppose that the Trimmingham cliffs have participated in the subsidence of 300 or 400 feet, and in the subsequent upheaval to an equal amount which the buried forest has undergone. If we imagine the drift to have accumulated gradually while the first or downward movement was going on, we must conclude that the disturbance of the beds did not take place till nearly the whole of this movement was completed; for had it occurred sooner, the upper beds in the Trimmingham cliffs would have been unconformable to the lower ones, whereas they are seen to be conformable throughout a thickness of at least two or three hundred feet of the beds above the chalk. I conceive therefore that the deranged position of the chalk and newer formation was more probably effected during or after the upheaval of the mass, and must in that case have been a very modern event. Although there is an obvious connexion between the amount of derangement of the newer strata and their proximity to the outliers of chalk, I saw nevertheless no signs of the masses of solid chalk having pierced the newer beds, as if forced through them; on the contrary, it appeared to me in every case, that the lowest bed of the drift, whether inclined at a high angle or vertical, conformed everywhere to the surface of the chalk, as if the same bed might have been originally in contact with it when horizontal. The chalk itself appears to have been in a flexible state, and therefore its beds of flint are variously bent.

Proceeding northwards from Trimmingham, we find the cliffs near Overstrand, about a mile S.E. of Cromer, entirely composed of clay and sand; but this drift does not continue far inland, and if the sea should advance for a few hundred yards, we might expect to see the whole cliff composed of chalk; for at the surface at Overstrand, a chalk pit is worked in which the very disturbed and shattered state of the chalk deserves notice.

Fig. 9.



Disturbed chalk in a pit at Overstrand, near Cromer.

In one part of the quarry we find what appears to be a fault, the line A B (fig. 9.) representing 18 feet in vertical height, where the solid chalk with flints, inclined at about an angle of 40°, comes abruptly in contact with alternating beds of white chalk rubble and gravel having an opposite dip, also at an angle of about 40°. After removing part of the chalk rubble I ascertained that the plane of the fault was continuous inwards at right angles to the line of section represented in the annexed diagram. The inclined chalk is covered by beds of stratified rubble resembling those before mentioned.

I stated that there were no signs of the submerged forest or freshwater deposit at the junction of the drift and chalk at Trimmingham, but this forest has been seen by Mr. Simons, about a mile and a half north-west of Trimmingham, at a place called Sidestrand, where the cliff, composed of drift, is 120 feet high. When I was there the base of the cliff was concealed by a high beach; but when this is removed, beds of laminated blue clay and sand, 6 or 7 feet thick, make their appearance, in which are some trunks of trees 3 feet in diameter, broken off to within a few inches of the roots, which spread for a distance of several feet on all sides. At one point near the bottom of this cliff a stratum of clay has been seen, in which freshwater shells of the genus *Unio*, apparently *U. ovalis*, abound.

At the town of Cromer itself, Mr. Simons has observed beneath the drift, several feet below low-water mark, a bed of lignite, in which were found the seeds of plants, and the wing-case of a beetle.

Norwich crag at Cromer.—At a still lower level than the freshwater beds last mentioned, and only exposed at very low water, is a thin bed of Norwich crag *in situ*, about one foot thick, resting immediately on the chalk. It was barely visible at low tide on the west side of the jetty when I visited Cromer, but with the assistance of Mr. Simons, I obtained many fragments in which pebbles, sand, and shells were aggregated together by a ferruginous cement. The most abundant shells were the *Purpura crispata*, Min. Con., *Tellina solidula*, and *Littorina littorea*, both the common form and the variety called *L. squalida*; I found also a *Fusus contrarius* and *F. striatus*, and *Cyprina islandica*, but I could detect no small or delicate shells, and the deposit had the appearance of having been formed in a shallow sea, and not in still water.

Although the deposit at Cromer varies slightly at each new spot where we examine it, it appears from repeated observations of Mr. Simons that the following section would give a fair representation of the whole: first, chalk, with horizontal surface; 2ndly, Norwich crag, with marine shells, from 1 to 2 feet thick; 3rdly, laminated blue clay, with pyrites, and the bones of mammalia, 8 feet. The upper part of this clay is at about high-water mark, and it forms the beach; 4thly, above high-water mark, layers of pure sand alternating with blue clay, with occasionally patches of gravel. In these beds the bones of mammalia occur and lignite abounds, thickness 10 feet. To these horizontal strata succeed the curved beds of drift, partly argillaceous and partly white and yellow sand, with imbedded masses of chalk and chalk rubble, the whole 60 feet thick.

Among the mammalian remains found on the beach and chiefly *in situ* in the blue clay, No. 3, Mr. Owen has recognized the following: 1. Teeth of *Elephas primigenius*; 2.

tooth of rhinoceros; 3. teeth of horse, the largest which Mr. Owen has ever seen fossil; its longest transverse diameter is 1 inch 4-10ths, which, however, does not exceed that of large living individuals; 4. bones of the ox; 5. horns and bones of a deer of the size of the red deer, and the base of a shed horn of the same; 6. a smaller species of deer; 7. lower jaw left ramus of the beaver, a species larger than the living one and apparently distinct. Among other characters the anterior molar of the lower jaw has a much greater proportional breadth.

The wood collected from the lignite bed, No. 4, is coniferous, and a cone which Mr. Simons procured from the same bed is certainly not the Scotch fir. Mr. R. Brown, who has examined it, has little doubt that it belongs to *Pinus abies*, or the spruce fir, a northern species not indigenous to Britain.

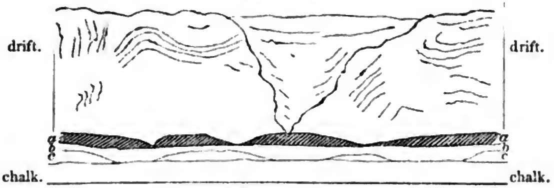
Cromer is the most south-eastern point on this coast at which I observed yellow ferruginous crag; but a blue sand containing the same marine shells has been traced for more than a mile further in that direction by Mr. Simons; and I have lately learnt from Mr. J. B. Wigham, that at Bacton Gap before mentioned, about 8½ miles distant in a straight line from Cromer, the hard ferruginous crag has been found immediately on the chalk. At that place, besides some of the usual shells, teeth of a small rodent (*arvicola*?) have been found, as at Norwich. About a mile *westward* of Cromer the crag re-appears, and again at Runton, as will be presently mentioned.

Freshwater strata of Runton between Cromer and Weybourne. —I shall mention here the only locality in which the freshwater deposit has been seen beyond Cromer, namely, at about 2½ miles N.E. of that town, on both sides of West Runton gap. Here it contains many shells as at Mundesley, and its position is unequivocally at the bottom of the drift, and immediately over the fundamental chalk, which is covered with patches of crag as at Cromer.

Thesection seen here on both sides of the gap consists, first, of drift, having its usual characters and irregularly curved stratification, and including small dispersed fragments of crag shells, its thickness being 60 feet and upwards. At the bottom of this the freshwater deposit occurs in patches of black earth from 3 to 5 feet thick, under which is a bed of reddish sand about 3 feet thick with freshwater shells in its upper part, and below this the crag in a discontinuous stratum less than a foot in thickness. The fundamental chalk contains large flints or paramoudræ. The lower part of the section beneath the black earth was covered up in June last, but exposed to view in March, and examined by Mr. Simons.

Fig. 10.

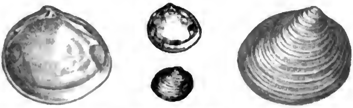
Runton Gap.



a. Black earth with shells } freshwater.
b. Reddish sand
c. Norwich crag in patches.

I shall now describe, first the freshwater beds and their fossils, and then the fossils of the subjacent layer of crag. The black earth is heavy and turns greenish when dried. It is sometimes divisible into layers, on the surface of which shells are seen in a compressed state; but this is not always the case, the shells being often uninjured and irregularly dispersed. Although the colour of this earth is doubtless due to vegetable matter, I have not found seeds in it, but occasionally small pieces of wood. The most shelly portions which I have seen were sent to me before my last visit to Norfolk, through the kindness of Robert Fitch, Esq., of Norwich. The red sand below resembles the crag in colour and contains the same shells, of which the following is a list, all of which have been examined by Mr. G. Sowerby: 1. *Paludina vivipara.* 2. *P. impura.* 3. *Valvata piscinalis.* 4. *Limnea palustris.* 5. *L. stagnalis.* 6. *Planorbis imbricatus.* 7. *P. albus.* 8. *P. marginatus.* 9. *Ancylus lacustris.* 10. *Cyclas cornea.* 11. *C. appendiculata.* 12. *C. amnica*, var.? Besides these is a small shell allied to *Turbo ulvæ*, but apparently different, of which I only procured one individual; also fragments of Anodon. Among these twelve species the only one which could not be identified with well-known British living species is the *Cyclas*, resembling *C. amnica.* It belongs to the sub-genus *Pisidium*, and is remarkable, says Mr. G. Sowerby, " for its great proportional altitude, in which respect it differs not only from the recent *P. amnicum*, but also from the fossil variety of *P. amnicum*, found at Grays in Essex. The concentric ridges on the outside of each valve are much more prominent than in the recent *P. amnicum*, particularly near the beaks, and in this circumstance they resemble the Grays fossil var. of *amnicum*. The shell appears to be rather thicker than the recent *P. amnicum*, and the teeth stronger: see fig. 11.

Fig. 11.



Cyclas (*Pisidium*) *amnica*, var.?

From the freshwater beds at Runton. The two middle figures are of the natural size.

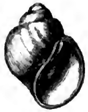
Neither here nor at Mundesley was I able to find *Cyrena trigonula*, which however we might have expected to discover in these beds, as it accompanies a similar assemblage of shells from various localities in Suffolk and Essex.

I found no remains of insects in the black earth, but the Hon. and Rev. R. Wilson, of Ashwell Thorpe, showed me in his collection, in 1838, the elytra of beetles of the genus *Donacia*, preserving their colours, which he had found several years before at Runton. I observed the scales of perch and of other fish resembling those of Mundesley in the black earth. Mr. Simons has also found fragments of the scapula and horns of a deer in the black earth.

In general it is most difficult to speak with certainty respecting the position of fossil bones of quadrupeds derived from the mud cliffs, because they have been picked up at the base of the cliff after portions of it had been washed away by the sea. It is the opinion, however, of collectors that they are chiefly derived from strata, in which the lignite and submerged trees occur. The remains are those of the elephant, rhinoceros, hippopotamus, horse, ox, pig, beaver, deer, &c. At Cromer and Weybourne some mammalian bones occur in the crag, but they are commonly more rolled and worn than those derived from the lignite deposits. Unfortunately no freshwater shells have yet been obtained from precisely the same bed as that in which the bones of the elephant and other extinct quadrupeds are met with, nor from the stratum in which the stools of buried trees are enveloped. The freshwater shells of Mundesley and Runton, although they may probably belong to the same formation, are not yet proved to be strictly coeval with the extinct quadrupeds. The present state, therefore, of our knowledge would not enable us to enter into minute details in regard to the order of superposition of the beds between the chalk and drift in the mud cliffs, but it would appear that the principal site of the bones of extinct mammalia as well as of the buried forest and lignite is between the marine crag and those beds from which freshwater shells have been procured.

Crag at Runton.—In the patches of marine crag below the freshwater at Runton, the following shells have been found and presented to me by Mr. Simons: 1. *Fusus striatus.* 2. *Scalaria grœnlandica.* 3. *Littorina littorea.* 4. *Natica helicoides*, Johnston, (see fig. 12.). 5. *Tellina obliqua.* 6. *T. solidula.* 7. *Cardium edule*, and a fragment of a *Helix.*

Fig. 12.



Natica helicoides, Johnston; from the crag at Runton, near Cromer.

The shell which I have called *N. helicoides* is identical with No. 58. in my list of Norwich crag shells published in the Mag. Nat. Hist., vol. iii. new series, 1839, p. 313. I have given it there as a new and extinct species, stating, that it resembled in shape *Paludina solida*, Say. I afterwards learnt from Mr. Edward Forbes that it had been found recent on our east coast in Berwick Bay, and published by Dr. Johnston in the Berwick Transactions, 1835, under the name of *N. helicoides.* That gentleman has since sent me the recent shell, which is quite identical with the fossil figured above. The species is remarkable for departing from the normal form of the genus *Natica.* It seems to have been much more abundant in the sea of the Norwich crag than in our own sea at present.

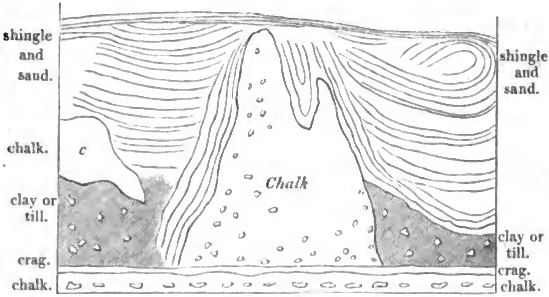
Cliffs between Cromer and Sherringham. — The drift near Cromer and to the north of it includes a much larger quantity of chalk rubble than to the southward, and huge fragments of chalk itself are sometimes intercalated in a manner which is very difficult of explanation. It is often no easy matter to decide whether the largest of the chalky masses associated with drift have been regenerated or not, in other words whether they have been brought piecemeal or in mass into their present position; but there are some clear and unequivocal exemplifications of both of these modes of transport. Some of the enormous fragments of chalk which are interstratified with drift have not only layers of undisturbed flints, but also sandpipes in the middle of them, or cylindrical cavities filled with sand and gravel, such as are found penetrating the chalk at various depths from the surface in the interior of Norfolk. These pipes seem to me to imply that such masses of chalk were once at or near the surface of emerged land, but a hasty observer seeing such patches of sand or pebbles in the middle of the chalk might suppose the whole mass to have been broken up and then redeposited, whereas in fact it has been brought bodily into its present position. The intercalated masses of unregenerated chalk are sometimes horizontal, sometimes vertical. Of the former I observed an example near West or Upper Runton, where a mass of chalk marl 15 feet thick, which I could not distinguish from undisturbed chalk, reposed on stratified blue clay 20 feet thick, and was again covered by stratified loam 30 feet thick.

The most remarkable example which I saw of a mass of chalk protruding in the midst of the drift adjoins to Old Hythe Gap about three quarters of a mile west of Sherringham: it is represented on a small scale by Mr. R. C. Taylor in his coast section, though nowhere described as far as I am aware. I found the shape of this mass considerably altered between the years 1829 and 1839, and by a comparison of its appearance at these two periods, I was able to form a more correct idea of its relative position to the chalk and drift than I could possibly have done during a single visit. In order to understand the peculiar position of this great outlier, the reader must be informed, that the fundamental chalk, which at Cromer does not rise above low water, begins, immediately west of Sherringham, to rise and form a ledge a few feet above high-water mark, being usually covered by a hard breccia of crag, commonly called *the pan*, nearly 1 foot thick. The waves at high tides and during storms wash over this ledge, and sweep away the more destructible clay, sand, and gravel of the overlying drift, which is thus made to recede four or five feet inward from the beach or seaward termination of the ledge of chalk. The chalk thus clearly exposed is seen by its horizontal layers of flint to be undisturbed.

The drift sometimes reposes in horizontal and sometimes in curved beds on the pan or ferruginous breccia of crag. At Old Hythe point above mentioned, the beds of drift suddenly become vertical for a height of nearly 70 feet, and flank an enormous pinnacle of chalk between 70 and 80 feet in height, (see fig. 13), which is enveloped in drift.

In this figure the fundamental chalk is seen at the bottom with its horizontal flints, and immediately upon the chalk the pan or layer of consolidated crag, continuous in this spot and varying in thickness from 6 to 12 inches. It contains large chalk flints and fragments of shells cemented by oxide of iron. The broken shells are abundant at some spots. Among them were observed *Cyprina islandica, Tellina solidula, Mya arenaria? Cardium* ——, *Littorina littorea, Fusus striatus, Balanus* ——. Next above the crag is the huge pinnacle or needle of chalk, distinctly separated from the fundamental chalk by "the pan." Chalk flints are scattered somewhat irregularly through the outlier of chalk, which is distinctly forked in its upper extremity. It will be seen that the pinnacle is flanked on both sides by drift: that on the east, or Sherringham side (left of the diagram), consists of alternate layers of loam, clay, and white chalk rubble several feet thick, which must have been deposited horizontally although now vertical. These are traceable from within a few yards of the pan to near the summit of the chalk, for a height of 60 feet or more. Between the two prongs of the fork, near the top of the cliff, are curved beds of drift. On the western or right side of the pinnacle the beds of drift are not the same as those on the left. They consist first, and nearest to the chalk, of strata of flinty gravel; secondly, layers of sand with round flint pebbles; thirdly, loose yellow sand, alternating with loam. These join on to curved beds of drift, which are represented near the top of the cliff on the right of the diagram—innumerable layers of sand and shingle, some of them bent round upon themselves and containing seams of carbonaceous matter, or in other places small white pieces of broken shells. Near the bottom of the section argillaceous till rests immediately on the crag, and on one side comes in contact with the chalk pinnacle near its base. Through this till small pieces of chalk and flint are interspersed. Another included fragment of chalk (*c*) occurs nearly half-way up the cliff, enveloped in drift to the westward of the pinnacle.

Fig. 13.



Included pinnacle of chalk at Old Hythe point, west of Sherringham.*

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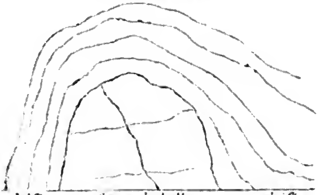
* This sketch is taken principally from my own drawing, but corrected from a view by Mr. Simons taken in March 1840, when the waves during a storm had reached about 8 feet above the level of the pan or crag, removing the talus which previously masked the junction.

The most singular and important circumstance connected with the great outlier of chalk at Old Hythe is the fact of its being perfectly disunited from the subjacent horizontal chalk. I could not myself positively determine this point either in 1829 or 1839, because there was a talus at the base of the vertical cliff resting on the projecting ledge of chalk and concealing the junction; but when the whole was cleared away by the waves in March 1840, after a storm, Mr. Simons visited the spot, and ascertained the continuity and infra-position of the crag which I had before inferred. My inference, previously announced to the Geological Society, was drawn from a comparison of the state of the cliff in 1839, with my sketches and memoranda made ten years before. At both periods I was able to trace the horizontal crag to within 5 feet of the base of the precipice, composed of vertical beds of drift enveloping the chalk; and as the sea had advanced greatly in the interval of ten years, the pan, had it not been continuous, must have been entirely removed before my last visit, in which case nothing could have been visible but chalk on the ledge immediately opposite the pinnacle.

From the summit of Old Hythe point the land slopes down to Old Hythe gap with a rapid descent. It also slopes, though at a less angle, directly inland, so that as the sea advances the cliff at this point will become less elevated. In 1829 the two masses of chalk appeared much more equal in size, and wrapped round as it were both on their sides and at the top with strata of shingle and drift.

Another included mass of pure chalk was also observable in 1839 between Cromer and Lower Runton near the bottom of the cliff. It was traversed by several rents, and alternating beds of laminated clay and sand were bent round it, as in the annexed diagram (fig. 14), which represents a perpendicular section 25 feet in height.

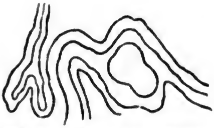
Fig. 14.



Section 25 feet high, west of Cromer.

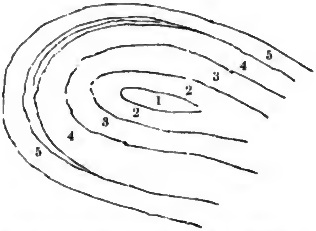
This mass, although on a smaller scale, may be compared to that of Old Hythe point (fig. 13.). It will sometimes happen, however, that the enveloping beds of drift appear to be folded completely round a nucleus of chalk or sand, or any other material found in the mud cliffs as in the annexed cut (fig. 15.) or in fig. 16, which represents a perpendicular cliff 20 feet high, in which the beds are: 1. blue clay; 2. white sand in thin layers; 3. yellow sand; 4. striped loam and clay; 5. laminated blue clay; and I saw curves not far from this place which extended for a vertical height of 50 feet, in which 30 distinct strata, without counting the subordinate laminæ, in all 24 feet thick, presented the same concentric arrangement. The beds consisted alternately of blue clay and white sand, the bed of sand exposed in the centre being blackened by bituminous matter.

Fig. 15.



Folding of the strata between East and West Runton.

Fig. 16.

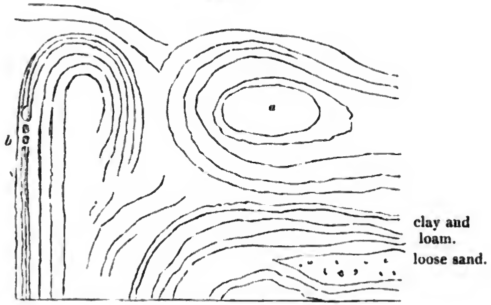


Section of concentric beds west of Cromer.

I have mentioned some of these cases of the apparent folding of the beds round a central nucleus in the Principles of Geology, especially one which occurs in the cliffs east of Sherringham, where a heap of partially rounded flints about five feet in diameter appears nearly enveloped by finely laminated strata of sand and loam, in the midst of which again is a nucleus of loam. After a more scrupulous examination of many of these cases, I have now ascertained that they are all, without exception, examples of the intersection of a series of strata which have been bent into a convex form, the apparent nucleus being in fact the innermost bed of the series, which has become partially visible by the entire removal of the protuberant part of the outer layers.

I observed a portion of a cliff 8 feet in vertical height between Beaston Hill and East Runton, in which a nucleus of very loose sand 18 inches in diameter (*a*) was surrounded by layers of clay and loam as represented in fig. 17. The vertical beds on the left side of the cut consisted of similar incoherent materials, some of the seams of sand being characterized by broken crag shells, and in one place some flint pebbles, *b*, occupying the space of several layers of loam.

Fig. 17.



Section 8 *feet high of vertical and curved drift in the cliff near Runton.*

Between the Runtons and Sherringham, and at a short distance from the latter place, are seen strata of vertical drift, on the one side of which are horizontal, and on the other curved and folded beds. The change in these cases from the horizontal set to the vertical is very abrupt.

Crag near Weybourne.—It is not until we arrive within less than two miles of Weybourne, that the Norwich crag appears in considerable force *in situ* above the level of the sea, in a cliff about 30 feet high, between Old Hythe Gap and Weybourne. At two different points I observed the chalk in contact with several feet of shelly sand and clay containing pebbles and the fossils of the Norwich crag without any intervening breccia or "pan." This crag was covered with clay and loam without shells.

About half a mile from Cliffend, Weybourne, the following section appeared, in a vertical cliff about 40 feet high, where I saw the greatest thickness of crag abounding in shells: 1st, horizontal chalk with flints, 8 feet; 2ndly, sand and flint pebbles with crag shells, 1 foot; 3rdly, fine sand with perfect crag shells, 10 feet; 4thly, sand and pebbles without shells, 3 feet; 5thly, unstratified clay or till with flints, 10 feet.

The following is a list of the shells obtained from this crag: *Fusus striatus*, *Littorina littorea*, *L. squalida* (var. of preceding?), *Purpura crispata*, perhaps var. of *P. lapillus*, *Cyprina islandica*, *Cardium edule*, *Cardium echinatum*? *Tellina obliqua*, *T. solidula*, *Nucula Cobboldiæ*, *Mya arenaria*? *Mactra*, *Astarte*.

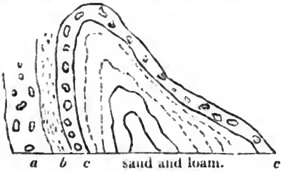
Among the above, the *Fusus striatus* and *Nucula Cobboldiæ* were very rare.

I have remarked, that westward of Sherringham, where the fundamental chalk rises a few feet above high-water mark, its surface, whether covered by the ferruginous breccia or not, is for the most part very level, a singular fact when the contortions of the overlying strata are considered. A slight exception occurs at one place near Cliffend, Weybourne, where the surface of the chalk undulates; so that in the distance of a few paces the chalk sometimes rises 12 feet above the level of the sea, then sinks to 1 foot, and then rises again to 8 feet above that level, being covered everywhere with a similarly undulating breccia made up of slightly rolled chalk flints and crag shells more or less broken.

Finally, near Weybourne, at the extreme end of the cliff, where it is 10 feet in height, the section given in the annexed diagram (fig. 18.) is seen. We here see the shelly crag subjected to the same violent movement so common elsewhere in the drift. The vertical gravel beds *a c* are separated by loose sand. Other loose sand occurs in the arch at *c c*. The crag shells in the gravel, consisting chiefly of *Cardium* and *Cyprina*, are in fragments, and the denudation of such beds may well have supplied those smaller and worn pieces of these shells which are so widely dispersed through the mud cliffs of Eastern Norfolk.

Fig. 18.

Arched beds of shelly crag at Cliffend, Weybourne; height of section 10 *feet.*



a. *c*. flint gravel with crag shells.
b. loose sand.

THEORETICAL CONSIDERATIONS.

Age of the deposits composing the mud cliffs.—It has been shown in the above account of the cliffs between Hasborough and Weybourne, that the chalk is everywhere the fundamental rock, lying southward of Cromer at about the level of low water, and rising on the north of that town to the height of a few yards above that level. Its surface between Cromer and Weybourne is covered with occasional patches of

Norwich crag, which is rarely more than one or two feet thick, except near Weybourne. Upon the crag, and where this is wanting immediately upon the chalk, rests here and there a lignite and freshwater formation, which varies in thickness from five to ten feet and upwards. It is seen at intervals throughout the whole line of cliff from Hasborough to Runton. In some places it resembles a bed of lignite, in others a black earth like that found in connexion with peat, while occasionally it consists of gravel, sand, clay, and marl, such as may be met with in any lacustrine deposit. In certain localities it contains the stools of trees, which remain in the position in which they originally grew, and which could only have been buried under the strata now incumbent on them by the submergence of what was once dry land. At Mundesley the freshwater formation is about 40 feet thick and occupies the whole cliff.

As both the crag and freshwater formations are extremely discontinuous in the mud cliffs, we sometimes find the one and sometimes the other in immediate contact with the chalk, while in many places both are wanting, and then the chalk is covered exclusively by drift, of which the great mass of the mud cliffs is composed. A cursory observer, indeed, might see nothing but drift from Hasborough to Cromer, except at Trimmingham, where the protuberances of chalk occur; and the section north of Cromer would seem to present little more than the same drift, with a slight exposure of chalk on the sea beach. The thin stratum of freshwater origin and the subjacent marine crag are most commonly hidden by the beach, or by the sea, except at low water.

Age of the crag.—As to the age of the crag, it agrees with that of Norwich in the species of marine shells which it contains, and the occasional presence of land shells and the rolled bones of mammalia. From the various localities above enumerated, I obtained the following eleven species of shells: *Purpura crispata*, *Fusus striatus* and *contrarius*, *Littorina littorea* and *squalida*, *Scalaria grœnlandica*, *Natica helicoides*, *Nucula Cobboldiæ*, *Cardium edule*, *Cyprina islandica*, *Tellina obliqua*, *T. solidula*, and *Mya arenaria?* All of these are known as recent except three, *Fusus striatus*, *Tellina obliqua*, and *Nucula Cobboldiæ*. It would be rash however to pretend to determine the per centage of recent species from so small a number, and the late discovery of *Natica helicoides*, one of the eleven, in a living state, should make us careful not to assume, when reasoning on these more modern deposits, that we have acquired a perfect acquaintance with the present Fauna of our seas.

Age of the freshwater deposit.—Next, as to the age of the freshwater beds, we know as yet too little of the species of mammalia, fish, insects, and plants, which are imbedded in them in considerable abundance, to entitle us to lay much stress on their evidence alone. But we have from Mundesley and Runton, at least nineteen species of shells in an excellent state of preservation, namely, *Paludina vivipara*, *P. impura*, *P. minuta*, *Valvata piscinalis*, *V. cristata*, *Limnea palustris*, *L. stagnalis*, *L. glutinosa*, *L. peregra*, *Planorbis vortex*, *P. imbricatus*, *P. albus*, *P. marginatus*, *P. lævis*, Alder, *Ancylus lacustris*, *Cyclas cornea*, *C. appendiculata*, *C. amnica*, var.? and *C. pusilla*.

Of these all but two are certainly identical with species now living in Great Britain. One of these two, *Cyclas*, fig. 11, p. 364, may possibly be a variety of our living *C. amnica*, while the other, *Paludina minuta*, fig. 4, p. 354, is unknown. I have not included in the list the shell allied to *Turbo ulvæ*, because it would be unsafe to decide on a species from a single individual; nor have I enumerated among the recent species *Anodon cygneus* and *Unio ovalis*, although there is little doubt that the freshwater mussels of Mundesley and Sidestrand belong to these species.

Upon the whole we may conclude that this freshwater deposit must agree very nearly in age with those of Stutton in Suffolk, Grays in Essex, Cropthorn in Worcestershire, and others, which contain nearly the same species, with fossil bones of extinct quadrupeds. It is still a question in all these cases, whether all the species are not living, although some few may not be British shells, or whether there is really a very slight per centage of lost species, to which opinion I incline. It will be seen that the freshwater stratum in the mud cliffs everywhere overlies the crag when in contact. Many, however, of the same species of fluviatile or lacustrine shells are found intermixed with the marine crag itself near Norwich, in which latter the same Cyclas figured above (p. 364) is met with.

Age and origin of the drift.—As to the age of the drift, it is proved by direct superposition to be newernot only than the Norwich crag, but also than the freshwater beds at Runton and Sidestrand. At the same time the section at Mundesley (fig. 2, p. 353) seems to prove, that in some places the deposition of the drift was going on contemporaneously with the accumulation of freshwater beds. To frame a satisfactory theory respecting the origin of the drift is difficult. The fluvio-marine contents of the Norwich crag imply the former existence of an estuary on the present site of parts of Norfolk and Suffolk, including the eastern coast of Norfolk. Into this estuary or bay one or many rivers entered, and in the strata then formed were imbedded the remains of animals and shells of the land, river, and sea. Certain parts of this area seem at length to have been changed from sea into low marshy land, either because the sea was filled up with sediment, or because its bottom was upheaved, or by the influence of both these causes. Two consequences followed: first, trees grew on some spaces gained from the sea; secondly, in other spots freshwater deposits were formed in ponds or lakes, and in the channels of sluggish rivers, or grounds occasionally overflowed by streams. Next succeeded a period of gradual subsidence, by which some of the lands supporting the forests were submerged, the trees broken down, and their roots and stumps buried under new strata. At the same time, the freshwater beds, whether resting on crag or immediately on chalk, became covered with drift, except in certain places, such as Mundesley, where for a small space the accumulation of drift seems to have been entirely prevented, perhaps by the continued flow of a small body of freshwater.

I have met with no fossils so imbedded in the drift as to entitle me to form any positive opinion whether it be of freshwater or marine origin. The regularly stratified arrangement of a large part of it, and the different materials of the alternating strata, clearly demonstrate that it was formed gradually, and not by any single or sudden flood. The boulders which it contains, some of large size, seem to imply, that while a great proportion of the mass may have been derived from neighbouring regions, part at least has come from a great distance. Mr. R. C. Taylor observes, that the shore to the west of Cromer exhibits a singular accumulation of travelled fragments of rocks, whence it would not be difficult to collect a tolerably illustrative series. They consist chiefly of rounded blocks of granite, basalt, porphyry, trap, micaceous schist, sandstones of various kinds, chert, breccia, besides limestone and claystone; also fragments derived from the chalk, plastic clay, London clay, green sand, Kelloway's rock, the oolites, lias, and marlstone; in fact almost every formation above the coal-measures. These, he says, are of all intermediate magnitudes up to four tons weight, large bouldered masses appearing in the sea at low water, lying mixed with flints upon the chalk. One block of granite is stated to be near six feet in diameter, and another mass, standing six or eight feet high, has for some years been known to the fishermen under the name of Black Meg. This collection extends about two miles, chiefly opposite to Beeston Hill*.

The author just cited truly remarks, that this singular assemblage of boulders must have been dislodged from the wasting cliffs, of which the softer and finer materials have been removed by currents, for similar boulders are occasionally observed in the midst of the clay or till of the cliffs.

In different parts of the interior of Norfolk, boulders weighing several tons have been found in blue clay or till†.

I stated in the first edition of my Principles of Geology that I was unable in 1829 to draw a line of demarcation between the crag and the drift or diluvium. The Rev. W. B. Clarke afterwards insisted on the distinctness of the two formations‡, in which opinion I now concur, although I am still unable, in many spots, as, for example, near Weybourne, and between Southwold and Yarmouth, to say where the crag ends and the stratified drift begins. But this difficulty arises from the absence of fossils in the crag as well as the drift, and from the fact that the strata in the latter are often as regular and continuous for considerable distances as those of the crag.

Professor Sedgwick informs me, that in the unstratified brown clay or till of certain parts of Cambridgeshire, large angular blocks of lower green sand and chalk, with fossils of the Oxford clay and lias, occur. The till alluded to attains at some points a thickness of 300 feet: it resembles that in the Norfolk mud cliffs, and has been traced over many of the adjoining counties. Its extent therefore in area and depth render its history of high importance in the geology of the east of England.

I mentioned in the beginning of this paper, that I recognized the strongest resemblance between the boulder formation which I have seen in Sweden, Denmark, Holstein, and other countries, and the drift of Norfolk; and as I believe coast-ice and icebergs to have been instrumental in transporting much of the large and small detritus in Scandinavia, so I presume that at the same period the effects of the same agency was extended to the British seas, although on a smaller scale. But while some of the Norfolk erratics may be of northern origin, other portions of the associated drift may have been brought from neighbouring regions, and perhaps in an opposite direction, just as we now observe that some granitic boulders are floated in ice from the distant shores of Labrador into the Gulf of St. Lawrence, while other large fragments of rock, together with much gravel and sand, are firmly frozen into ice and carried down every winter by various rivers into the same gulf. As the part of Canada where this drift is now forming corresponds in latitude to that of Norfolk, the adoption of this theory of ice-drift does not of necessity require us to assume the former existence of a colder climate than that now prevailing in North America.

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* Geology of East Norfolk, p. 24, 1827.

† C. B. Rose, Geology of West Norfolk, Lond. and Edin. Phil. Mag., January 1836, p. 195.

‡ Geol. Trans., 2nd Series, vol. v., part 2, p. 363.

Dr. Mitchell, in a paper on the Drift of Norfolk, Suffolk, &c., (Geol. Proc., vol. iii., p. 5) has suggested that the materials have been in great part derived from the destruction of strata which once occupied the site of the German Ocean. This conjecture is, I think, by no means improbable, and we are often too prone, when speculating on the original site of travelled boulders, to refer them exclusively to the places where similar substances happen now to be exposed above water, whereas they may often have come from a neighbouring region now submerged. The island of Heligoland for example, about forty miles off the mouth of the Elbe, has been wasting away for centuries, and in time will probably disappear. Its cliffs, from 100 to near 200 feet high, composed of marl and marlstone of the new red sandstone formation, might supply stony fragments and red mud, which if stranded by ice or other agency on the adjacent coasts of Holstein, Bremen, or Friesland, would differ entirely from the rocks occurring there *in situ*, or from any rocks met with nearer than parts of Hanover, situated 100 or 150 miles in an opposite or eastern direction. We ought always, therefore, to bear in mind, that fragments of chalk, green sand, oolite, and lias, imbedded in the drift of Norfolk and other counties, may not have come from the westward where those formations now crop out, but possibly from the N.N.E., like the erratic blocks, if some of these be really of Scandinavian origin.

The association of stratified drift with unstratified materials or till, a general character of this formation in Sweden and Scotland, as in Norfolk, has been already attributed to the possible cooperation of ice and currents of water (see p. 348).

DISTURBED POSITION OF THE STRATA.

The chalk and overlying formations seen in the cliffs between Hasborough and Weybourne may have been brought into their present contorted and dislocated position by three distinct kinds of mechanical movement; first, by ordinary upheaval and subsidence, to which geologists are accustomed to attribute the bendings, inclination, and dislocation of strata; secondly, by landslips or the sliding down of sea-cliffs, or the falling in of undermined banks of rivers or of submarine sand banks; thirdly, by the stranding of islands and bergs of ice. It is possible that all these three causes of disturbance may have co-operated to produce the complicated movements which we now behold in the cliffs under consideration.

By ordinary subterranean movement.—First, in regard to ordinary subterranean movements, a general subsidence must, I conceive, have taken place over a considerable area, in order to explain the submergence and burial of the trees of which the stools are found *in situ*; and this forest bed could not have been brought up again, together with the incumbent drift, to the level of low water, without a subsequent upheaval nearly equal in amount to the previous subsidence. But such a depression and re-elevation of a large tract may have taken place slowly and insensibly, and without any derangement of the stratification. A question would still remain, whether such protuberances of chalk as those at Trimmingham (p. 357), and the inclination or verticality of the associated drift, should be attributed to a local and violent movement from below, fracturing the chalk and thrusting up portions of it above the ordinary level of that formation. It is scarcely profitable to speculate on a subject which could only be set at rest if the section were prolonged downwards into the subjacent chalk. I have described in the Geol. Trans., vol. v., part 1, p. 243, masses of drift entangled in chalk at the top of the cliffs of Möen in Denmark; but in those lofty cliffs the section extends downwards for a depth of more than 400 feet into the underlying chalk with flints. The verticality of some of the layers of flint, the curvature of others, and numerous faults, bear testimony to such repeated convulsions, that I did not hesitate to refer the entanglement of the upper chalk and incumbent drift of Möen to subterranean movements. During those convulsions, fissures and chasms may have opened in the chalk, and masses of the superimposed boulder formation may have been engulfed.

There are many sections, such as that represented in fig. 14, p. 368, where the first hypothesis which suggests itself is the protrusion upwards of a boss of chalk, which has forced the yielding and incumbent beds to fold round it, so that the beds become perfectly vertical on the flanks of the protuberant chalk. But it frequently happens that these masses repose on chalk and crag so horizontal and undisturbed, that we are entirely precluded from the supposition of a movement from below upwards.

By landslips and slides.—The last remark leads naturally to the consideration of every combination of causes which can give rise to great disturbance in the overlying beds, while the stratification of those below remains even and unchanged. For striking examples of this phænomenon the reader is referred to figures 1 and 13, in which the superposition of vertical to horizontal drift, and of huge fragments and needles of chalk to horizontal chalk and crag, are clearly exhibited. In order to explain these sections, we may imagine that banks of mud and sand existed beneath the sea in which channels were occasionally excavated by currents. In banks of this kind off Great Yarmouth, a broad channel sixty-five feet deep was found in 1836, where there had been only a depth of four feet in 1822*. If the cliffs of loam or sand bounding this new channel give way, large masses may descend bodily and assume a vertical or curved position. They may easily escape subsequent denudation, because the direction of the currents are constantly shifting. Thus strata which have assumed a vertical position may be forced laterally against the opposite sides of the channels, where the beds have remained horizontal. Both the juxtaposition of vertical and horizontal beds, and the superposition of disturbed to undisturbed strata, may be caused in this manner. The constant descent of strips of land into river beds in the deltas of the Indus, Ganges, and Mississippi, on the subsiding of the annual inundations, are well known, and may give rise to analogous effects.

During the late landslip near Axmouth on the 24th of December 1839, a lateral movement took place, by which masses of chalk and green sand, which had been undermined, were forced more than forty feet in a seaward direction, and thrown into great confusion, while the subjacent lias was not disturbed†. The pressure moreover of the descending rocks urged the neighbouring strata extending beneath the shingle of the shore, by their state of unnatural condensation, to burst upwards in a line parallel to the coast, by which means an elevated ridge more than a mile in length, and rising more than forty feet, has been made to form an extended reef in front of the present range of cliffs. This ridge when it first rose was covered by a confused assemblage of broken strata and immense blocks of rock, invested with sea-weed and corallines, and scattered over with shells, star-fish, and other productions of the deep.

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* See Elements of Geol. p. 307.

† I have been indebted to the kindness of the Rev. W.D. Conybeare for a description and section of this landslip, which I have published in the 6th edition of the Princ. of Geol. vol. ii. p. 78.

We may imagine in like manner masses of chalk and overlying drift to have fallen from cliffs, and to have been forced sideways over a floor of horizontal chalk; but it appears to me impossible, even if we adopt this hypothesis, to explain how the Old Hythe pinnacle of chalk (see p. 367) became enveloped by drift, and this drift in great part vertical and resting on horizontal crag and chalk. It seems necessary first to suppose that a needle of chalk was thrown down on horizontal drift, and then that the whole was forced by lateral pressure into a vertical position, the fundamental rocks remaining unmoved.

It cannot be objected to explanations of this kind that ancient cliffs and adjoining needles of chalk are no longer visible, because they may have existed when the country was subsiding, and they may have been removed by denudation, when brought down within the action of the waves.

By pressure of drift ice.—There is still another cause, hitherto, I believe, overlooked, by which great foldings and contortions may be produced in the upper portions of banks of sand and gravel, while the lower remain undisturbed; I mean the stranding of icebergs and large masses of packed ice. In different parts of Scotland, Sweden, Norway, and probably everywhere in Europe where drift is found containing erratic blocks, between the latitudes 50° and 70° north, coiled and folded beds of loam, gravel, and sand are frequent, and I have often seen them in Scotland resting on and covered by strata which remain horizontal.

In the account given by Messrs. Dease and Simpson of their recent arctic discoveries, we learn that in lat. about 71° N. long. 156° W. they found "a long low spit named Point Barrow, composed of gravel and coarse sand, in some parts more than a quarter of a mile broad, which the pressure of the ice had forced up into numerous mounds, that viewed from a distance assumed the appearance of huge boulder rocks*." So many facts indeed have come to my knowledge of the manner in which masses of ice, even of moderate size, in the Baltic, and still more in the Gulf of St. Lawrence, push before them large heaps of boulders, that I can scarcely doubt that lateral pressure, exerted under favourable circumstances by drift ice on banks of stratified and incoherent sand, gravel, and mud, is an adequate cause for producing considerable flexure and dislocation. The banks on which icebergs run aground occasionally between Baffin's Bay and Newfoundland are many hundred feet under water, and the force with which they are struck will depend not so much on the velocity as the momentum of the large floating islands. The same berg is often carried away by a change of the wind and then driven back again upon the same bank, or in other cases it is made to rise and fall by the waves of the ocean, and may thus alternately strike the bottom with its whole weight, and then be lifted up again until it has deranged the superficial beds over a wide area. On these beds new and undisturbed strata may be afterwards thrown down. In other cases, when banks of mud and sand forming the top of a shoal have been made to assume various shapes by the lateral pressure of icebergs, the bed of the sea may subside, and then the disturbed beds may be overspread by horizontal strata, which may never afterwards be deranged by similar mechanical violence.

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* Journ. of Roy. Geograph. Soc., vol. viii. p. 221.

LVIII. *On the Method of performing the simple Experiment of Interferences with two Mirrors slightly inclined, so as to afford an* experimentum crucis *as to the nature of Light. By* R. POTTER, *Esq.*[*]

FRESNEL'S genius devised the experiment which is the most direct test of the reality of the interference of light, and which proves that property in the most unequivocal manner. This experiment is performed by causing the light diverging from a luminous point to be reflected by two plane mirrors, placed side by side, whose surfaces are *nearly* in the same plane, but which contain an angle a little less than 180°, and then examining the light by means of an eye-lens. Each mirror gives an image of the luminous point, and we have the reflected light proceeding as if it diverged from these two images and also from its having originally constituted only one pencil, the two reflected pencils are in the same state, so that they interfere where they cross each other's direction, producing in ordinary light coloured bands parallel to the line of intersection of the planes of the two mirrors, with dark intervals between them. These bands are seen in the air in the focus of the eye-lens when looking towards the images of the luminous point.

Without examining the experiment more minutely than just to ascertain that both the pencils are necessary to the production of the bands, it must be admitted that it is conclusive in establishing the theory of interferences.

The theory of interferences was brought forward by Dr. Young as a consequence of the undulatory or wave theory of light; and it must be confessed that the latter theory was greatly advanced in probability by the demonstration of the former. Certain circumstances, however, such as the colour and arrangement of the bands, are required to be examined before we can consider the primary theory to be confirmed in the same extent as its subordinate theory. For instance, according to the fundamental property of wave interferences the central band must be white; but if it should be found in the experiment that the central band is black, this discrepancy, whilst it would not weaken the demonstration of the theory of interference, would yet be a fatal objection to the theory of undulations.

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[*] Communicated by the Author.

Now if two equal series of circular or spherical waves which have the same direction, or nearly so, arrive at any points in the transmitting fluid, in such a manner that the like parts of the waves arrive at the same instant, then their conjoint effect will be to produce a resultant wave stronger than either of the component waves. If, however, the two series are in a state of complete discordance, so that one series would produce an effect equal and opposite to that which would be produced by the other series, they would counteract each other, that is, no resultant wave would be produced. Two series of waves, which are respectively of equal diameters, and similar, at every instant may be denominated simultaneous; and if we draw a line bisecting perpendicularly that joining their origins, the waves of each series will meet in the same state everywhere along this line, since every point in it is equally distant from the two origins. When these waves are very distant from the origins compared with the distance of the origins from each other, they will have very nearly the same directions, and we shall have resultant waves of greater strength than the component waves. This is a conclusion depending only on the fundamental properties of waves, and does not involve any review of the various hypotheses which are held, as to the nature of the vibrations of the particles of the fluid through which the waves are propagated.

The experiment with the two mirrors, before mentioned, produces the case we have just discussed; for the single pencil diverging from the original luminous point, is made into two pencils after reflection by the mirrors; and these two, if light consist of waves, are composed of waves both equal and simultaneous. We have here then an experiment on which to test the wave theory of light, and the experiment itself shows us which is the band corresponding to the line bisecting perpendicularly that which joins the luminous images of the point. For if the luminous point be formed of white light, the various colours having different intervals for their luminiferous surfaces or different lengths of waves in the undulatory theory of light, the bands will be of different breadths for different colours, but will have the one on the bisecting line above named bright for every colour, and which will be consequently white, when the luminous point is formed with white light; but on each side of it, the superposition of bands of different breadths corresponding to different colours, will cause the compound bands to be coloured, at first on their edges only; afterwards the colours will become more and more spread, and the bands at the same time more confused, as the distance from the central band becomes greater, until they are at length gradually lost in a light uniform to the eye. The bands on each side and near to the central one will have their inner edges violet and their outer edges red, so that the arrangement of colours will be symmetrical on each side of the central white band. The central band is thus pointed out in the experiment by the arrangement of the colours.

In my early trial of this experiment, I happened to have a clear sky and unclouded sun, which afforded a result causing me to hesitate before I accepted the undulatory theory as true. I have seen the experiment on days in which there were thin clouds in the atmosphere, and once when the sun was near the horizon, such that it would have led me to a different conclusion.

In my first experiments I was surprised to find the colours symmetrical on each side of a dark band, and not a bright one. Every precaution was used, such as keeping the bands clear of the diffracted fringes formed by the edges of the mirrors, making the distance between the luminous images so small that the bands were very large, and therefore that any prismatic effect produced by slight error of looking centrically through the eye-lens did not produce a sensible effect in the arrangement of the colours; also care was taken that the direct rays (as the term direct is used in optics in contradistinction to oblique) passing through the lens forming the luminous point were those which fell on the two mirrors. Still the central band was a dark one and not a bright one. I obtained the assistance of friends accustomed to accurate observation, to examine the appearances, and they came to the same conclusion. I was thus at a loss to conceive how the advocates of the undulatory theory could state that *the central band was always a white one*. Some time afterwards, however, I obtained a different result; for experimenting one fine evening when the sun was near the horizon, I saw the middle band clearly white, and the colours accurately symmetrical on each side of it, although the whole appeared misty and without that darkness in the intervals which I had found in the previous experiments. This continued whatever pains I took to keep every part of the apparatus in adjustment. The dullness and mistiness of the latter phænomenon led me to conclude that there could be no doubt but the former was the normal result. I however tried the experiment again with every care in the former circumstances, and found the same result as formerly.

In the Number of Phil. Mag. for April, 1833, p. 279, I stated, "The result of considerable experience with me is, that it may be seen both black and white, though with me it has much oftener been the former, especially when the bands have been well defined," &c. &c.

In the November Number for 1833, p. 342, I said, "I shall consider it extremely important to determine whether, when an achromatic lens of short focus is used to form the luminous point, the central band of direct interference given by two mirrors is black, as it has appeared to me, and to several friends to whom I have shown it, when adequately tried with a common lens," &c., &c.

I have in the papers from which the above extracts are taken, publicly stated my difficulties without reserve, and my ideas of the requirements for a decisive mode of experimenting.

A mode of experimenting has been adopted, (where or with whom the discovery originated I do not know,) which is stated to give the central band white. It is this: the image of the sun formed in the focus of a lens is made to fall on a small aperture in a thin plate of metal; the light passing through the aperture falls on the two mirrors, and the aperture thus illuminated is called the luminous origin. I am informed, that when the rays falling on the two mirrors are those which pass obliquely through the aperture, then the central band is most distinctly a white one; and it has been argued that this must be the normal way of trying the experiment, inasmuch as the rays of all colours will have accurately a common origin on the edge of the aperture.

It has been with me a subject of frequent study to find out a method of trying the experiment which must give a result not to be disputed; and I feel confident the apparatus described in this paper fulfils every desideratum. The points kept in view have been to employ only such parts as are essential in the simplest form of the experiment; and thus in dispensing with the mirror which is usually employed to throw the sun's light through the window-shutter into the darkened room, I have employed an equatorial method of mounting, which keeps all the apparatus in the adjustment it is first placed in, and at the same time enables us to follow readily the sun's daily motion, keeping at the same time the room quite dark.

To obviate all objections which attach to the luminous point being formed by a common lens, which has different foci for differently coloured rays, I have employed a spherical mirror, as better even than any achromatic lens; and again, as it was suggested to me that an objection might be raised if the rays crossed in a real focus, I have used a convex mirror, so that they diverge from a virtual focus without having crossed: also to enable me to use the luminous point the smaller, I have generally used the two mirrors slightly inclined, of polished speculum metal, which reflects many times the quantity of light which glass reflectors do in the position which my apparatus requires. With these precautions, I find, when the sun is perfectly unclouded, and near the meridian, *high above the horizon*, that the central band is black. When there are clouds before the sun's disc, however slight, the central band is more difficult to fix upon, and generally either white or doubtful. The discrepancies which have been to me a puzzle for so many years, I am now able to solve, and I announce the following new principle of interferences:—

When light in a state of interference is made to interfere again, the result is of an opposite character to what it would have been if the light had been in the first instance in the ordinary state.

This proposition, which is in itself reasonable, might have been anticipated, and solves all the anomalies. The light falling on the small hole in the thin plate of metal, formerly mentioned, as used to form the luminous point, is thrown into a state of interference by diffraction at the edge of the hole, and hence the central band is seen white. Again, the sun's light passing through thin clouds, or through the vapours of the atmosphere when near the horizon, is thrown more or less into a state of interference by diffraction at the edges of the particles of the vapours, and gives results which are either doubtful or with a white centre: for where part of the light is in its original state and part in a state of interference, the bright and dark centred bands will be superposed, the central band of the one set over the central band of the other, and thus produce indistinct phænomena in which the intensity of the one or the other species may prevail.

From the full investigation, I thus maintain that the normal result of this fundamental experiment in interferences, is, that the central band is black, and at variance with the consequences of all wave theories of light, and therefore the undulatory theory of light is not the physical theory.

This conclusion is also borne out by the phænomena of the rainbow, (see Lon. and Edin. Phil. Mag. for July 1838, vol. xiii. p. 9.) the actual primary and secondary bows being much more distant than they should be from all calculations according to the undulatory theory of light, the position calculated by that theory to be bright being actually found to be dark. In the artificial methods of producing the analogous effect with a minute stream of water, there is no doubt but that the effect calculated from the undulatory theory might be obtained, by using a *line of light* in a previous state of interference.

The apparatus is represented in figs. 1 and 2, in which the same letters refer to the same parts: *a*, *b*, *c*, *d* is a cylinder of wood about three inches in diameter, and seven inches in length from *a* to *b*, with pivots as in the figures. In fig. 2 this cylinder is seen placed in a rectangular aperture cut in a thick piece of board in which it exactly fits, and is retained in

Fig. 1.

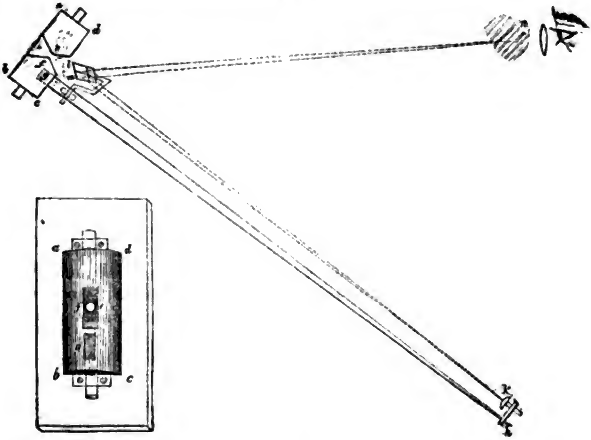

Fig. 2.

its place by steps screwed over the pivots. An aperture is cut straight through the centre of the cylinder, which in the axis at *e f* is circular, of a little more than half-inch diameter, but towards the surface it is extended longitudinally: through it the sun's light falls on the small convex mirror *k*. This mirror is attached, as in the figure, to the arm *g h*, made of light wood, about four feet long, which fits tightly in a lateral direction in a groove *g*, fig. 2, but can be moved in a plane passing through it and the axis of the cylinder, so as to place the mirror in the sun's light shining through the hole. The axis of the cylinder is fixed parallel to the earth's axis, when the apparatus is finally secured in the window-shutter of a room with a south aspect. The form of the aperture allows for the sun's declination at different times of the year, by which means, and the motion of the arm, the sun's light can be made to fall on the mirror *k* at all seasons; and to follow the sun's daily motion we have only to push the arm so as to turn the cylinder about its pivots. Every part is made to fit just so tight that the apparatus remains in any position in which it is placed. The two mirrors are placed as at *l m*, so that the sun's light just passes their edges when it falls on the mirror *k*; by this arrangement we make it certain that the light received by the two mirrors is that which is reflected nearly directly by the mirror *k*, and therefore with very little aberration. The two mirrors are attached to a piece of wood, which, having a hole in the line of contact of the mirrors, is moveable about a thick wire which fits tightly into this hole. This piece of thick wire is bent at right angles in three places, as seen in fig. 1, and forms a very convenient universal joint, which allows the mirrors to be turned about in any direction required: at its lower part it is pushed through a hole in a piece of wood fixed to the side of the arm *g h*. The dotted lines *n l m k*, are intended to show the course of two rays from the mirror to their interference at *n* in the focus of the eye-lens.

All the parts of the apparatus should be well blackened, and a piece of black velvet (as the most perfect black) placed behind and about the mirror. It is also desirable to have a tube of blackened paper placed on the arm near *k*, through which the sun's light may shine, but which will prevent the stray light from the mirror *k* injuring the darkness of the room.

Those who understand analytical geometry will find the readiest way of fixing the cylinder parallel to the earth's axis, to be that of calculating the lines of intersection of the board in fig. 2 with the plane of their window-shutter: great accuracy is not needed in fixing the cylinder, as its use is merely to afford the means of following the sun's daily motion and still to leave the room dark.

As the apparatus is here drawn, it will be advisable to have the mirrors *m l* of speculum metal, for the angle of incidence of the light is too small for glass mirrors to give very bright phænomena; although I have seen them of the same character as with the mirrors of speculum metal. The mirror *k* I have used of one-fourth of an inch focus.

All the mirrors should be of metal which is not porous, and of the highest polish, so that their surface is not visible when they are placed a short distance from the flame of a candle; for it is probable that numerous small pores in the metal, or the fine lines which constitute a second-rate polish, would produce diffraction sufficient to invalidate the experiment. As such mirrors are not everywhere to be had, I should, in such cases, recommend, as a substitute, that a good lens, of short focus, well centred, be placed in the aperture *e f*, and that a pair of glass mirrors blackened at their second surface, be placed on the arm at the other end *h*: the incidence would then be sufficiently oblique to afford an intense reflexion. Ordinary plate glass, or good pieces of window-glass, will show the bands very well; but those who wish to test the colour of the central band, should have their mirrors, as well as their lens, of good workmanship. The obtuse-angled prisms which may be obtained in the shops, are very convenient for observing the bands popularly, but are not to be depended upon as test apparatus.

Those who use the two mirrors for the first time will find some care necessary in fixing them with their contiguous edges in contact, for if the edge of one be raised any appreciable distance above that of the other, the bands are not produced. With glass mirrors, blackened at their second surface by sealing-wax melted over them, it is convenient to cement them together with the same material along their line of contact. If they are placed in position whilst the sealing-wax is soft, and then retained in their position until cool, they will remain ready adjusted to be used at any time.

I have been the more minute and popular in explaining the theoretical bearings, as well as the experimental details, from the consideration that this interesting and important experiment has very singularly been but little noticed in our more descriptive optical treatises; and also from the hope that I may by this essay induce others to try it, who have a sincere wish to form a correct opinion on the nature of light.

LIX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.* *By* Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*

[Continued from page 297.]

As a conclusion to this paper, and as connected with my subject, I am tempted to advert to the disposition so prevalent among geologists of placing in parallel British with foreign formations, without always maturely considering all their respective peculiar relations. As a case in point, none comes more readily to my hand than the late attempt of M. Dumont to assimilate the transition tracts adjacent to the Rhine, extending from the north of France through Belgium into Germany*, to the Cambrian and Silurian systems of Professor Sedgwick and Mr. Murchison†. From this author we had previously derived much valuable information concerning that region‡; but the parallel since drawn by him between the Belgian and British formations is the more remarkable, as it is confessed that the two countries do not correspond in the development of the mineral masses, nor yet in the distribution of organic remains. Nay, so far as the latter have hitherto been ascertained, there appears to be but a slight analogy between them, and which I now propose briefly to show, first introducing M. Dumont's table of equivalents, for the sake of reference.

	Belgium.		*England.*		
	Terrain Houiller	'Coal Tract'	Coal measures. Millstone grit.		Carboniferous system.
Terrain Anthraxifère, 'Anthraciferous tract.'	Upper calcareous system.	Limestone. Dolomite. Limestone.	Carboniferous limestone.		
			Old red sandstone.		
	Upper quartzo-schistous system.	Sandstone. Limestone subordinate. Schist. Limestone subordinate.	Upper Ludlow rock. Aymestry limestone. Lower Ludlow rock.	Ludlow formation.	Silurian system.
	Lower calcareous system.	Limestone. Dolomite. Limestone.	Wenlock limestone.	Wenlock formation.	
	Lower quartzo-schistous system.	Grey fossiliferous schist.	Wenlock shale.		
		Schist and red sandstone. Conglomerate. Sandstone, quartz-rock, schist.	Caradoc and Llandeilo formation.		
	Terrain Ardoisier, 'Slate Tract.'	Upper. Medial. Lower.			Cambrian system.

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* For a connected view of these tracts, it will be useful to refer to the "Geognostic Map of Germany and the adjoining States," published by Schropp and Co., Berlin, 1826.

† *Bulletin de l'Académie Royale des Sciences à Bruxelles*, Novembre 1838; see also a translation of this Memoir in the Lond. and Edinb. Philosophical Magazine for August 1839.

‡ A. H. Dumont, *Sur la Constitution Géologique de la Province de Liège*,

In the Ardennes, the clayslate tract (*terrain ardoisier*), referred to the Cambrian system, is divided into three stages: the lower destitute of fossils; the medial, in which roofing slate abounds, but in which few fossils have hitherto been found; and the upper, more quartzose than slaty, in which the vestiges of life begin to acquire greater extension, and in which the first indications of limestone appear, the latter being generally slaty, containing numerous crinoidal remains, and forming beds about a yard in thickness, but never occurring in mass like the limestone of the anthraciferous tract. In this upper stage M. Dumont notices *Orthoceratites*, *Crinoidea*, *Polyparia*, *Trilobites*, *Strophomena*, &c.; but according to M. d' Omalius d' Halloy, the most common fossils which it displays are *Spiriferæ* greatly expanded in the direction of their breadth, besides species referable to the genera *Calymene*, *Asaphus*, *Orthoceras*, *Hamites*, *Leptæna*, *Strophomena*, and remains of *Crinoidea* and *Polyparia**. M. d' Halloy admits that the fossils of this clayslate tract have not been determined with sufficient exactness. Yet as far as developed by himself and M. Dumont, can it be safely maintained that a precise analogy has been established between them and those found in the Cambrian system?

The clayslate tract, thus referred to the Cambrian system, passes in its upper stage by insensible gradations into the anthraciferous tract†, both on the north-western and south-eastern sides of the Ardennes; in the former direction in Belgium, and in the latter in the Eifel.

In the anthraciferous tract, the lower quartzo-schistous system, referred partly to the Llandeilo and Caradoc formation and partly to the Wenlock shale, is divided by M. Dumont into three stages: the lower, which contains no limestone nor fossils, or at least the latter, are extremely rare; the medial, in which the remains of organized bodies are also very seldom exhibited; and the upper, distinguished by the abundance and variety of its Shells and Polyparia from all other portions of the anthraciferous tract. In the upper stage, nodules or beds of fossiliferous limestone are often con-

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Bruxelles, 1832, a work respecting which I agree with M. Beyrich, that it does not appear to have drawn as much attention as it deserves. See the same author also on the Ardennes, Belgium, &c., in the *Bulletin de l'Académie Royale des Sciences à Bruxelles*, November 1836 and November 1837.

* *Elémens de Géologie*, troisième édition, 1839, pp. 476, 477.

† *Terrain anthraxifère*—this term, introduced in 1808, by M. d'Halloy, is admitted by the author not to be good, since anthracite is found in other groups, and it does not occur in all the systems which compose the tract designated as anthraciferous by himself and M. Dumont.—*Elémens de Géologie*, third edition, p. 438.

tained in the schistose rocks. But the only fossils enumerated hitherto are referred to the genera *Producta*, *Spirifera*, *Strophomena*, and the remains of *Crinoidea* and *Polyparia*. Where, then, it may be asked, are the peculiar organic remains of the Llandeilo and Caradoc formations and of the Wenlock shale, to which this system is referred?

In the lower calcareous system, referred to the Wenlock limestone, ten species of coral are enumerated, six of which occur in the Wenlock limestone, and eight in the Eifel limestone*. Two of them are also met with in the carboniferous limestone of Liège and Namur, namely, *Cyathophyllum cæspitosum*, *Cyathophyllum pentagonum*; and one in the lower part of the coal formation of Liège, namely, *Cyathophyllum quadrigeminum*. Of Terebratula we have two species common to the Wenlock limestone, namely *T. prisca* and *T. aspera*, besides two or three other species not found in the latter. If to these we add *Strophomena*, *Solarium*, *Nerita*, and *Crinoidea*, we have the whole of the organic remains enumerated as belonging to this calcareous system in his work on the province of Liège: but at a later period the author remarks†, that in this lower calcareous system are found also *Terebratula concentrica*, some *Spiriferæ* and *Euomphali*, besides *Productæ* and other shells which are commonly met with in the upper calcareous system (namely, the carboniferous limestone). Such being the case, it may be asked, wherein does the analogy consist between this lower calcareous system and the Wenlock limestone? Where are the numerous characteristic fossils of the latter? On the other hand, the occurrence in this transition limestone of fossils which are common also in the carboniferous limestone, deserves the attention of geologists, since it forms a parallel to similar phænomena in Devon and Cornwall, and the south of Ireland.

The upper quartzo-schistous system is referred by M. Dumont to the Ludlow formation. But where do we find the various fossils of that formation as enumerated by Mr. Murchison? The only corresponding species noticed are *Spirifera radiata*, *Terebratula aspera*, and *T. Wilsonii*. The rest of the fossils given appear more nearly allied to the upper calcareous system (the carboniferous) than to the lower calcareous (the transition). As bearing on this question, it may also be noticed that in the upper part of this system a bed of coal has been found.

Of the upper calcareous system there is no question among geologists as to it being referable to the carboniferous limestone. I may here remark also, that it contains among its upper beds one or two seams of coal. It exhibits many fossils that are common to the carboniferous limestone of the British isles; but in it are found also some species which occur in the Silurian system and other transition regions, namely, *Calymene macrophthalma*, *Orthoceras striatum*, *Terebratula lacunosa*, *Cyathophyllum turbinatum*, *Cyathophyllum cæspitosum*; besides one fossil, which is a native of the transition districts of Normandy, Brittany, and Anjou, namely, the *Calymene Tristani*. The only Goniatite noticed in this carboniferous limestone is *Gon. sphæricus*; but in the lower part of the succeeding coal formation there occur also *Gon. Listeri*, *Gon. Diadema*, and *Gon. atratus*[*].

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* These will appear in the tables given further on.

† See Lond. and Edinb. Phil. Mag. for August 1839.

Even this brief review may be sufficient to show that there is but little analogy between the three lower systems of the anthraciferous tract in Belgium and the formations of the Silurian system, to which they have been assimilated; that is judging by the organic remains hitherto elicited from the former as compared with the latter. What other fossils may be discovered by the extended researches of M. Dumont and others remains to be seen. At present the author appears to be fully justified in his remark, that "though the English divisions established by Mr. Murchison are very good for England, as being founded on the existence of fossils which appear to be different in each of them; yet these divisions must present palæontological differences more or less remarkable in other countries, and that this is in fact what takes place in Belgium: and he therefore proposes to draw the attention of the Royal Academy of Brussels to this subject at a future period[†]." With such an admission, would not the term *Belgian* have been more appropriate than that of *Silurian*, as applied to these formations? Premature approximations tend rather to retard than promote the advance of science. It has been well observed by a profound judge, that, "as we must be careful not to apply our domestic types without modification to other regions, so we must take care not to despair of modifying our scheme, so that it shall be more extensively applicable than it at first appeared to be[‡]." It will be well for geologists to bear this reflection in mind. Much, I apprehend, remains yet to be accomplished, before anything like a definite order can be established in the sequence of the transition formations.

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[*] See the works of MM. Dumont and Beyrich already quoted.

[†] *Bulletin de l'Académie Royale des Sciences à Bruxelles*, November 1838, and Lond. and Edinb. Phil. Mag., August 1839.

[‡] Proceedings of Geol. Soc., Address of the Rev. W. Whewell, as President, February 1839. [Lond. and Edinb. Phil. Mag. vol. xiv., p. 450.]

M. Beyrich considers the lower calcareous system of M. Dumont as identical with the Eifel limestone. And M. Dumont also places the Eifel limestone in parallel with the lower limestone of Belgium, finding below them both the same lower quartzo-schistous system reposing on the clayslate tract of the Ardennes, and tracing all these formations to the banks of the Rhine. In this view, I believe, he is correct.

The evidence as far as produced leads to the conclusion, that the north-western and south-eastern flanks of the clayslate tract of the Ardennes, together with the succeeding lower quartzo-schistous and lower calcareous systems on each side, in Belgium and the Eifel, are justly referred to the transition epoch. From the same evidence it would also appear that the upper quartzo-schistous system is more nearly allied to the carboniferous than to the transition series; and I conceive it may be composed in part of alternations of old red sandstone with beds of carboniferous limestone *, while the succeeding mass of the carboniferous limestone and the coal measures follow in their regular order.

That the whole of these series, extending from the north-western flank of the Ardennes to the Belgian coal formation inclusive, should have been formerly referred by Continental geologists to the transition æra in general is not very surprising, since they are commonly represented as forming an uninterrupted succession (yet with some exceptions), passing one into the other, and as having been subjected to the same disturbing forces; while they exhibit also some fossils that are common both to the lower and higher members of the series. But the *old red sandstone* of British geologists has been usually thought to be wanting in the succession, both here and in the corresponding tracts extending hence beyond the Rhine into Germany. M. Dumont originally conceived the *lower* quartzo-schistous system to represent the old red sandstone†, an idea afterwards abandoned; and which has been since also repudiated by M. Beyrich, who justly observes, that were we to adopt such a view, we should have scarcely anything in the whole Rhenish slate mountains but old red sandstone and carboniferous limestone, the latter of which would fall into two great divisions, of which the lower would be the Eifel limestone: but such an arrangement, he observes, would be quite inconsistent with nature, as proved by the evidence of organic remains*. That the old red sandstone, however, occurs in considerable force in various parts of Germany, I showed in the year 1821, when giving abstracts of the observations of M. Freiesleben on the Forest of Thuringia, on Mansfeld, Thuringia, and the Circle of the Saale, and of those of MM. Von Buch and Von Raumer on Lower Silesia, the county of Glatz, and part of Bohemia and Upper Lusatia. And at that time, this formation was also conceived to exist in partial distribution in the Netherlands, then forming the immediate support of the carboniferous limestone†. This view has been more recently revived by MM. Rozet and Constant Prevost, who contend that the old red sandstone of Britain is not wanting in Belgium, stating as an example, that it is well developed between Dinant and Namur; that is, between the lower Belgian limestone which is in force at Givet, and the carboniferous limestone which prevails at Namur; and hence that M. Dumont was in error when he excluded that formation altogether from the series ‡. This appears to accord with the prior observations of M. Von Dechen, to which I have referred above.

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* This view appears supported by the pertinent observations of M. Von Dechen, at p. 484 of his modified translation of M. de la Beche's Manual, in which he refers to the beds of red conglomerate which occur on the Meuse near Lustin and Profondeville, at Hayoux, south of Huy; on the Vesdre near Pepinster, and on the Vichtbach at Binsfelser Hammer.

† In his Memoir on the province of Liège, p. 67.

But perhaps no evidence is more conclusive of the want of similarity between the Belgian and the Silurian formations than that which is to be derived from a consideration of the fossils of the Eifel limestone (which, as already shown, is considered as identical with the lower limestone of Belgium), placed in parallel with those of the Wenlock limestone; the fossils of the Eifel having been determined to a much greater extent than those of the other formations, by the labours of Professors Goldfuss and Bronn, and MM. Von Dechen, Steininger, Dumont, and Beyrich; while the fossils of the Wenlock limestone have been well developed in Mr. Murchison's highly valuable work. To institute this comparison, I have drawn up the following tables, founded upon a comparison of the works of those authors, which may serve to place the subject in a clear light:—

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* *Beiträge*, p. 4, General observations on the fossiliferous strata of the Rhenish transition slate mountains.

† Annals of Philosophy, October 1821; *Ibid.*, the same subject continued in August 1822, May 1823, and July 1824.

‡ *Bulletin de la Société Géologique de France*, tome ix., Séance de 18 Decembre, 1837.

I. General Table of classes and orders in both the Wenlock and Eifel limestones, with the number of species distinct and common in each.

Wenlock limestone.	Total Species.	Distinct Species.	Species common to both limestones.	Distinct Species.	Total Species.	Eifel limestone.
Crustacea	15	14	1	9	10	
Annelida	1	1	—	3	3	
Mollusca.						
Heteropoda...	2	2	—	3	3	
Cephalopoda..	10	9	1	16	17	
Gasteropoda..	8	7	1	22	23	
Conchifera	27	18	9	69	78	
Crinoidea	14	12	2	19	21	
Polyparia.........	58	30	28	24	52	
Sedis incertæ....	2	2	—	2	2	
	137	95	42	167	209	

II. Table of genera and species common to the Wenlock and Eifel limestones.

CRUSTACEA, Trilobite	1	Calymene macrophthalma.
MOLLUSCA.		
—— Cephalopoda..................	1	Orthoceras annulatum.
—— Gasteropoda	1	Euomphalus carinatus.
CONCHIFERA	9	Terebratula Wilsonii (Terebratula lacunosa, *Dalm.*) Spirifera trapezoidalis; S. crispa, *Dalm.*, (S. octoplicata, *Sow.*); S. radiata. Atrypa reticularis (Terebratula prisca, *Schlot.*). A. aspera (Terebratula aspera, *Schlot.*, including T. explanata, *Schlot.*). A. galeata. Leptæna euglypha, Leptæna depressa.
CRINOIDEA	2	Cyathocrinites rugosus. Actinocrinites moniliformis.
POLYPARIA	28	Aulopora serpens, A. tubæformis. Glauconome disticha. Fenestella antiqua, *Lons.* (Gorgonia antiqua, *Goldf.*). Fenestella prisca, *Lons.* (Retepora prisca, *Goldf.*). Discopora? favosa, *Lons.* (Cellepora favosa, *Goldf.*). Discopora antiqua, *Lons.* (Cellepora antiqua, *Goldf.*). Ceriopora granulosa. ——— affinis.

POLYPARIA (*continued*).

Ceriopora punctata.
——— oculata.

The three last species are given on the authority of Goldfuss, as occurring in the Dudley limestone.

Stromatopora concentrica.
Favosites alveolaris, F. gothlandica, F. polymorpha, F. spongites, F. fibrosa.
Catenipora escharoides.
Porites pyriformis, *Lons.* (Astrea porosa, *Goldf.*).
Astrea ananas *de Blainville* (Cyathophyllum ananas, *Goldf.*).
Caryophyllia flexuosa.
Cyathophyllum dianthus, C. turbinatum, C. cæspitosum.
Cystiphyllum Siluriense, *Lons.* (Cyathophyllum vesiculosum, *Goldf.*).
Strombodes plicatum, *Ehr.* (Cyathophyllum plicatum, *Goldf.*).
Limaria clathrata, L. fruticosa.

42 Species.

III. Table of genera common to both limestones, but with distinct species in each.

In Wenlock limestone.	*In Eifel limestone.*
CRUSTACEA—11.	CRUSTACEA—9.
Calymene Blumenbachii, C.? Downingiæ, C. tuberculata, C. variolaris, C.? punctata.	*Calymene* lævigata, C. arachnoides, C. Schlotheimii, C. latifrons.
Asaphus caudatus, A. tuberculato-caudatus, A. flabellifer, A. Stokesii.	*Asaphus* Hausmanni, A. Bucephalus, A. armatus.
Paradoxides 2-mucronatus. ——— 4-mucronatus.	*Paradoxides* macrocephalus. ——— flabellifer.
MOLLUSCA.	MOLLUSCA.
Heteropoda 2. *Bellerophon* dilatatus, B. Wenlockiensis.	Heteropoda 3. *Bellerophon* undulatus, B. apertus, B. striatus.
Cephalopoda 6. *Orthoceras* Brightii, O. eccentricum, O. fimbriatum, O. canaliculatum, O. trochlearis. *Conularia* quadrisulcata.	Cephalopoda 4. *Orthoceras* giganteum, O. nodulosum, O. inflatum. *Conularia* teres.
Gasteropoda 5. *Euomphalus* sculptus, E. discors, E. rugosus, E. funatus. *Patella?* implicata.	Gasteropoda 8. *Euomphalus* nodosus, E. radiatus, E. striatus, E. articulatus, E. depressus, E. delphinuloides, E. trigonalis. *Patella* Neptuni.
CONCHIFERA 18. *Pentamerus* Knightii.	CONCHIFERA 46. *Pentamerus* Aylesfordii.
Terebratula borealis, T. nucula, T. crispata, T. imbricata, T. cuneata, T. bidentata, T. deflexa, T. Stricklandii.	*Terebratula* concentrica, T. heterotypa, T. triloba, T. lateralis, T. crumena, T. canaliculata, T. quinquelatera, T. dichotoma,

In Wenlock limestone.	*In Eifel limestone.*
Conchifera (*continued*).	Conchifera (*continued*).
	T. pentagona, T. Wahlenbergii, T. acuminata, T. diodonta, T. subglobosa, T. bifida, T. clavata, T. amygdala.
Orthis rustica, O. hybrida, O. filosa, O. canalis.	*Orthis* Pecten, O. testudinaria, O. radiata, O. fasciculata, O. nodosa.
Spirifera? sinuata.	*Spirifera* cuspidata, S. cyrtæna, S. pinguis, S. microptera, S. heteroclyta, S. macroptera, S. ceptoptera, S. minima, S. attenuata, S. curvata, S. striatula, S. glabra et obtusa, S. oblata, S. canalifera (Terebratula aperturata, *Schlot.*).
Atrypa didyma, A. obovata, A. tenuistriata, A. compressa.	*Atrypa* cassidea.
Leptæna 0.	*Leptæna* convoluta, L. furcata, L. capillata, L. pectinata, L. minuta, L. scabricula, L. rugosa, L. Scotica, L. lepis.
Crinoidea 8.	Crinoidea 6.
Cyathocrinites tuberculatus, C. goniodactylus, C. capillaris, C. pyriformis.	*Cyathocrinites* geometricus.
Actinocrinites simplex, A.? arthriticus, A.? expansus, A.? retiarius.	*Actinocrinites* triacontadactylus, A. cingulatus, A. muricatus, A. nodulosus, A. lævis.
Polyparia 13.	Polyparia 18.
Aulopora conglomerata, A. consimilis.	*Aulopora* spicata, A. sarmentacea.
Fenestella Milleri, F. reticulata.	*Fenestella* 0.
Discopora squamata.	*Discopora* 0.
Retepora infundibulum.	*Retepora* antiqua.
Gorgonia assimilis.	*Gorgonia* infundibuliformis.
Stromatopora nummulitisimilis.	*Stromatopora* polymorpha.
Favosites multipora.	*Favosites* infundibuliformis.
Porites tubulata, P. expatista, P. discoidea.	*Porites* 0.
Cyathophyllum 0.	*Cyathophyllum* radicans, C. hypocrateriforme, C. ceratites, C. flexuosum, C. vermiculare, C. secundum, C. lamellosum, C. placentiforme, C. quadrigeminum, C. hexagonum, C. helianthoides, C. pentagonum.
Cystiphyllum cylindricum.	*Cystiphyllum* 0.
Sedis incertæ 1.	Sedis incertæ 2.
Tentaculites ornatus.	*Tentaculites* scalaris, T. annulatus.
64 Species.	96 Species.

IV. Table of genera and species distinct in both limestones.

In Wenlock limestone.	*In Eifel limestone.*
Crustacea 3. *Homalonotus* delphinocephalus. *Acidaspis* Brightii. *Bumastus* Barriensis.	Crustacea 0.
Annelida 1. *Spirorbis* tenuis.	Annelida 3. *Serpula* ammonia, S. omphalodes, S. socialis.
Mollusca.	Mollusca.
Cephalopoda 3. *Nautilus* undosus (Clymenia? Münster). *Lituites* giganteus, L.? Biddulphii.	Cephalopoda 12. *Cyrtoceras* depressum, C. compressum, C. annulatum, C. lineatum. *Spirula* nodosa, S. costata, S. annulata, S. carinata, S. dorsata. *Goniatites* subnautilinus, G. multiseptatus, G. orbiculus.
Gasteropoda 2. *Nerita* spirata, N.? Haliotis.	Gasteropoda 14. *Turbo* armatus, T. nodosus, T. cælatus. *Trochus* exaltatus. *Pileopsis* prisca, P. compressa. *Sigaretus?* rugosus. *Rotella* helicinæformis. *Phasianella* ventricosa, P. buccinoides, P. fusiformis. *Turritella* bilineata, T. striata, T. obsoleta.
Conchifera 0.	Conchifera 23. *Gypidia* gryphoides. (Terebratula gryphus, *Schlot.*) *Strygocephalus* Burtini (Terebratula rostrata, *Schlot.*), S. striatus. *Calceola* sandalina. *Aptychus* lævigatus. *Pecten* Neptuni. *Pterinea* radiata, P. elegans*. *Modiola* Goldfussii. *Cardium* alæforme, C. elongatum. *Lucina* Proavia, L. lineata, L. rugosa. *Cyprina* minuta. *Sanguinolaria* concentrica, S. lamellosa, S. dorsata, S. truncata, S. phaseolina.

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* M. Beyrich remarks that *Pterineæ* are numerous, and in a manner characteristic of the greywacke and slate rocks which support the Eifel limestone. Besides the two species noticed above, Professor Goldfuss enumerates the following as occurring in the greywacke and slate countries adjacent to the Rhine, namely near Ems, *Pterinea ventricosa, P. costata, P. lineata, P. plana, P. trigona, P. lævis, P. elongata*; in Siegen (and the Harz) *P. lamellosa*; near Iserlohn, *P. reticulata*; and at Paffendorf (Coblenz) *P. carinata*. This last species occurs also, with *P. bicarinata*, at Lindlar, in the Berg territory. Of these fourteen species thus found in transition tracts, two only have been observed in the carboniferous limestone, viz., *Pterinea elegans* in the Ratingen limestone, and *P. carinata* near Lewistown adjoining Niagara in North America.

In Wenlock limestone.	*In Eifel limestone.*
CONCHIFERA (*continued*).	CONCHIFERA (*continued*).
	Pholadomya radiata.
	Isocardia Humboldtii, I. antiqua.
CRINOIDEA 4.	CRINOIDEA 13.
Marsupiocrinites cælatus.	*Eugeniocrinites* mespiliformis.
Hypanthocrinites decorus.	*Pentacrinites* priscus.
Dimerocrinites decadactylus, D. icosidactylus.	*Platycrinites* ventricosus.
	Melocrinites gibbosus.
	Rhodocrinites verus, R. giratus, R. quinquepartitus, R. canaliculatus, R. crenatus.
	Cupressocrinites crassus, C. gracilis, C. tesseratus.
	Eucalyptocrinites rosaceus.
POLYPARIA 17.	POLYPARIA 6.
Escharina? angularis.	*Anthophyllum* bicostatum.
Philodictya lanceolata.	*Achilleum* cariosum.
Hornera crassa.	*Manon* cribrosum.
Berenicea irregularis.	*Scyphia* cornu-copiæ.
Eschara? scalpellum.	*Coscinopora* placenta.
Blumenbachium globosum.	*Lithodendron* bicostatum.
Millepora repens.	
Syringopora reticulata, S. bifurcata, S. filiformis? S. cæspitosa?	
Monticularia conferta.	
Acervularia Baltica.	
Cladocora sulcata.	
Turbinolopsis bina.	
Verticillopora? abnormis.	
Cnemidium tenue.	
SEDIS INCERTÆ 1.	SEDIS INCERTÆ 0.
Cornulites serpularius.	
31 Species.	71 Species.

From the preceding tables it results that we have

In the Wenlock limestone.			*In the Eifel limestone.*		
Common species		42	Common species		42
Common genera but distinct species	64		Common genera but distinct species	96	
Distinct genera with distinct species	31		Distinct genera with distinct species	71	
Distinct species	—	95	Distinct species	—	167
Wenlock—total species		137	Eifel—total species		209

From the foregoing review, and the resulting numbers, it becomes clearly apparent, that M. Dumont was not justified in pronouncing the Wenlock and Eifel limestones as of the same formation. It is, however, also evident that they are akin in a certain degree, the species which are common to both limestones being to the total number in each, in the Wenlock limestone as 4 to 13 nearly, that is, forming about one third part; and in the Eifel limestone as 4 to 20, or one fifth part. Again, in the Wenlock limestone, the distinct species are to the common species as 9 to 4, or $2\frac{1}{4}$ to 1; and in the Eifel limestone as 16 to 4, or 4 to 1.

M. Beyrich has made the remark, that the greywacke and slate country which supports the Eifel and Belgian lower limestone, contains for the greater part the same fossils as are found in this limestone; and he adds, that among them occur a considerable number of species which are common to the carboniferous limestone, and in this remark he is joined by M. Dumont, as already observed: that this view is correct, the following table which I have drawn up will sufficiently prove.

V. Table of fossils common to the Eifel limestone and the carboniferous limestone—47 *species.*

	Localities in carboniferous limestone.
CRUSTACEA.	
Calymene macrophthalma, *Al. Brong.*	Richelle, Liège.
MOLLUSCA.	
Heteropoda—	
Bellerophon undulatus, *Goldf.*	Chimay, Schwelm.
B. apertus, *Sow*................	Richelle, Bristol, Fermanagh.
B. striatus, *Goldf*.......	Chimay, Ratingen.
Cephalopoda—	
Orthoceras annulatum, *Sow*..........	King's County.
O. giganteum, *Sow*.............	Closeburn.
CONCHIFERA.	
Terebratula pugnus and lateralis, *Sow.*	Ratingen, Derbyshire, Dublin.
T. crumena, *Sow*..............	Derbyshire.
T. lacunosa (T. Wilsonii, *Sow.*) *Dalm*......................	Liège.
T. Mantiæ, *Sow*................	Ireland.
T. acuminata, *Sow*.............	Ratingen, Yorkshire, Derbysh.
T. diodonta, *Dalm*.............	Ratingen.
T. amygdala, *Goldf*............	Visé.
T. sacculus, *Sow*..............	Derbyshire, Rutherglen.
Orthis testudinaria*, *Dalm*.	Cromford in Westphalia.
Spirifera cuspidata (S. elevata, *Dalm.*) *Sow*.	Ratingen, Bristol, Derbyshire.
S. octoplicata (S. crispa, *Dalm.*), *Sow*......................	Derbyshire, Castlemaine in Kerry.
S. oblata, *Sow*.	Richelle, Visé, Dublin.
S. glabra et obtusa (Terebratula lævigata, *Schlot.*), *Sow*........	Richelle, Ratingen, Derbyshire.
S. pinguis (S. lævicosta, *Goldf.*), *Sow*......................	Liège, Dublin.
S. attenuata, *Sow*..............	Liège, Dublin, Clonmell.
S. striatula, *Goldf*.............	Ratingen.
S. canalifera, *Goldf*............	Ratingen, Visé, Dublin.

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* See Professor Bronn in *Lethæa Geognostica*, p. 82. This is, I believe, the only instance on record of an Orthis having been found in the carboniferous limestone.

CONCHIFERA (*continued*).	*Localities in carboniferous limestone.*
Leptæna (Producta) depressa (Strophomena marsupita, *Defr.*), *Sow.*	Richelle, Fermanagh.
L. Scotica, *Sow.*	Richelle, Chokier, Castlemaine.
L. hemisphærica, *Sow.*	Richelle, Chokier, Ratingen, Buttevant.
L. longispina, *Sow.*	Richelle, Linlithgow.
L. sarcinulata, *Sow.*	Richelle, Visé.
L. scabricula, *Sow.*	Visé, Liège, Dublin.
L. rugosa (Strophomena, *Rafin.*), *Dalm.*	Chokier.
Pterinea elegans, *Goldf.*	Ratingen.
Cardium elongatum, *Sow.*	Ratingen, Derbyshire.
C. alæforme, *Sow.*	Ratingen, Queen's County.
Sanguinolaria concentrica, *Phillips*	Tour Mountain, County Cork.
CRINOIDEA.	
Actinocrinites 30-dactylus, *Mill.*	Mendip, Yorkshire.
A. lævis, *Mill.*	Ratingen.
Rhodocrinites verus, *Mill.*	Mendip, Bristol.
POLYPARIA.	
Gorgonia infundibuliformis, *Goldf.*	Arnsberg.
G. antiqua (Fenestella antiqua, *Lons.*), *Goldf.*	Arnsberg.
Cyathophyllum flexuosum, *Goldf.*	Limerick.
C. pentagonum, *Goldf.*	Namur.
C. cæspitosum, *Goldf.*	Chokier, Seilles.
C. turbinatum, *Goldf.*	Richelle.
C. quadrigeminum, *Goldf.*	Berneau.
Astrea ananas, *de Blainville* (Cyathophyllum ananas, *Goldf.*)	Namur.
Favosites polymorpha, *Goldf.*	Namur, Elberfeld.
F. fibrosa, *Goldf.*	Buffalo on Niagara River.

It thus appearing that there are 47 species in the Eifel limestone which are common to the carboniferous limestone, it follows that they constitute about *two ninths* of the whole number found in the Eifel, the ratio being as 47 to 209.

On the other hand, we have seen that the Eifel limestone contains 42 species which are common to the Wenlock limestone, or *one fifth* of its whole number, the ratio being as 42 to 209.

Hence, if we take these numbers as a guide, it may be said that the Eifel limestone is in a slight degree more nearly allied to the carboniferous limestone than it is to the Wenlock, the difference lying between the ratios of 2 to 9, and 2 to 10.

From the numbers which have been given it also results, that the Wenlock limestone contains 95 species which are distinct from those of the Eifel; and the Eifel limestone 167 species distinct from those of the Wenlock limestone; of which 47 species being also common to the carboniferous limestone, it follows that 120 species may be said to be distinctive of the Eifel, in contradistinction to the Wenlock and carboniferous limestones.

This view leads naturally to the inference, that in the order of superposition the Eifel limestone occupies a middle rank, namely, one higher in the series than the Wenlock, but lower than the carboniferous limestone. This result would appear to correspond with the notions of M. Beyrich, who, in speculating on the relative age which should be assigned to the Rhenish slate mountains among the transition formations, is disposed to consider them as of a later origin than those strata in the north of Europe, namely in Scandinavia and Russia, in which the Orthis tribe are so abundant. These latter may doubtless be placed, to a certain extent, in parallel with the Silurian.

If the views which I have taken be correct, in which the formations of the Eifel, and the lower formations in Belgium are, in the order of succession, considered as antecedent to the old red sandstone of Belgium (meaning such as in Britain has been usually designated by that name), it becomes doubly desirable that the distinguished geologists who have taken Devon and Cornwall in hand should complete their investigations. I have been led to anticipate that a considerable degree of analogy subsists between the two regions, and, if it be true, it may yet be proved that the older stratified rocks of those counties are not only of later origin than the Silurian formations (which would so far correspond with the views of Mr. Lonsdale, Professor Sedgwick, and Mr. Murchison), but of a date also anterior to that of the old red sandstone formation, taken in the common acceptation of that term. Whether the latter may yet be detected in the southern parts of Devon in unconformed position, may still be a fit subject for inquiry.

It is remarkable that among the fossils by which the Devonshire and Cornish formations are distinguished, Mr. Austen should state a number to exist as common to the carboniferous limestone, so nearly agreeing with the number of a corresponding character in the Eifel, namely, as 40 and 47. An account of the 40 species indicated by Mr. Austen, it is to be hoped, may be published, and it will be interesting to compare them with the list that I have given from the Eifel. What ratio that number may bear to the total number of species in the older stratified rocks of Devon and Cornwall remains yet to be seen, as well as what number of species may be common to the Silurian, what number may be similar to the Eifel fossils, or to those of the older stratified rocks and their included limestone bands in the south of Ireland, and what number may be more peculiarly distinctive of Devon and Cornwall. These are questions which can only be answered by extended researches; and until these be completed, it may be prudent to defer the assignment of the older stratified formations of Devon and Cornwall to a precise period in respect of relative age.

In the limestone bands of the south of Ireland I have shown that 50 species of fossils have been determined with precision, that is, including *Leptæna depressa* and *Leptæna lata*; but others also occur which have not been determined, e. g., *Avicula* and *Goniatites.* Of the fifty species, *six* are peculiar; *twenty-six* common to other transition tracts at home and abroad (of which *twelve* occur in the Eifel); while *forty-three* of the species are common to the carboniferous limestone also*; thus nearly agreeing with the numbers of a corresponding character found in Devon and Cornwall, and in the Eifel. But as in the south of Ireland, neither the bands of limestone, nor the strata with which they are directly associated, nor yet those situated more north which lie deeper in the series, have hitherto undergone that rigid examination which is requisite with respect to the organic remains which they may contain, we are not authorized as yet to pronounce upon the precise proportions in which those fossils may exist relatively to each other in the general series. Yet enough has already been elicited to prove that those limestone bands stand in uninterrupted connexion with strata in which fossils of a more decided transition character are by no means wanting; and no sufficient evidence has yet been produced to invalidate the conclusion that the whole constitute together one consecutive series, notwithstanding we may perceive some distinctions between the fossils of the schistose and greywacke strata and those of the included bands of limestone. Continued researches will doubtless throw further light on this subject.

A pertinent observation of Professor Sedgwick may here be aptly introduced: "There are two elements of classification applicable to stratified rocks of all ages, namely, physical structure and order of superposition; one giving the mineralogical unity of a group of rocks, the other their relative age. In addition to the two former, are classifications founded on the organic remains in the several groups.......As, however, the (so-called) laws respecting the distribution of organic types are mere general results founded on actual observation, it is obvious that they can never upset conclusions drawn from the clear and unambiguous evidence of sections. The two methods may be used independently, and conspire to the same end, but in their nature connot come into permanent collision*." In the present case it should be borne in mind that the consecutive series of the older stratified rocks of the south of Ireland is *unconformably* overlaid in the northern parts of Kerry, Cork, and Waterford, directly either by the *true* old red sandstone formation of British geologists, or by the carboniferous limestone, or the coal formation †.

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* See Memoir on the South of Ireland in Geological Transactions, vol. v., second series, and Lond. and Edin. Phil. Mag. for August 1839. When the former work was finished in 1835, I considered the *Isocardia oblonga* as peculiar to the Cork band of limestone; but Professor Phillips's Illustrations of Yorkshire, part ii., published in 1836, have shown that the *Isocardia oblonga* occurs also in carboniferous limestone in Yorkshire, and in the counties of Kildare and Dublin.

I might now extend the comparison by entering into the countries adjacent to the right bank of the Rhine, or into the Fichtelgebirge; but in neither of them is the information hitherto obtained of so extensive and detailed a character as to admit of precise conclusions. To what was previously known respecting the former tracts, M. Beyrich has made considerable additions in his *Beiträge*; and from these contributions it may be collected, that the greater part of the Nassau limestones near Dillenburg, &c., as well as those of Bensberg, Refrath, Paffrath, &c., adjoining the Rhine, together with the greywacke and slaty rocks in which they are intercalated, or on which they simply rest, exhibit in general the same organic remains as the Eifel, although they possess also species and even genera not hitherto found in the latter; e. g., at Paffrath the genera *Nerita*, *Megalodon*, *Cardita*, *Monodonta*, *Buccinum*; and in Nassau in the Wissenbach clayslate *Parmophorus*; and in the limestone on the Lahn near Villemar an *Ostrea*, a genus not previously noticed in any transition country. Of the Nassau limestones it is remarked that they differ chiefly from the Eifel limestone by being for the greater part interstratified with the greywacke and slate rocks, while the limestones of the Eifel are merely superimposed upon the latter in troughs. In Belgium, however, the same, namely, the lower limestone of that country, forms an interstratified portion of the general series. Considering then the fossils which have been noticed in these districts, we have here again an exemplification of affinities and differences in the organic types of their respective strata. Should M. Beyrich complete the work which he proposed to himself in the year 1837, of drawing up an exact critical catalogue of all the fossils which occur in the Rhenish slate mountains, such a work could not fail to prove of high interest to every geologist, and its appearance is much to be desired.

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* Proc. of Geol. Soc., vol. ii., p. 675, May 1838, [or Lond. and Edinb. Phil. Mag. vol. xiii., p. 299.

† See my Geological Map of the South of Ireland, in Geol. Trans. vol. v., second series.

From what is already known of the transition tracts of the Fitchtelgebirge and of Bohemia in the environs of Prague, it may be inferred that the same doctrine of affinities and differences applies there also. It is understood that the late Count Münster was engaged in a work from which great light might be expected to be thrown on the more ancient formations. As his valuable collections have passed into the possession of the University of Cambridge, we may well anticipate that his extensive labours will not be lost to science.

If the genus *Orthis* be considered, as it appears generally to be, indicative of the older transition formations, it must also be admitted that the genera *Terebratula*, *Spirifera*, and *Leptæna* are of equal antiquity.

In fine, even this brief exposition may serve to confirm the opinion which I have formerly expressed, namely, that in widely extended, or distantly separated, lands of transition or protozoic origin, the relations though akin may not be exactly alike; in other words, that resemblances may exist, but diversities prevail in the details of different tracts, both with respect to the composition and disposition of the mineral masses and the distribution of the remains of organized bodies. Whether the transition formations may ultimately be separated into definite consecutive groups, is a question which can only be satisfactorily determined by the results of extended comparative inquiries.

[To be continued.]

LX. *On the Course of the Electrical Discharge, and on the Effects of Lightning on certain Ships of the British Navy, &c. &c. By* Mr. Snow Harris, *Esq., F.R.S.*

[Continued from p. 128.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

24. In the instance I last quoted of damage to H.M.S. Rodney by lightning, it will be remembered that there was no regular metallic line through which the forces in action could become neutralized. The electrical agency had therefore to find for itself such a general course, as upon the whole opposed the least resistance to its progress; and it is

evident that in this case its path was determined on the general principles before laid down in sec. 17, p. 123.

25. I shall now proceed to state a few cases of damage to certain other ships of the navy, where metallic bodies happened to be so disposed about the rigging and hull, as to approximate in some measure to the conditions of experiment 2, sec. 17, and consequently to that perfect state of defence against the expansive force of the electrical discharge in which a ship would become placed, by perfecting the conducting power of the masts, and uniting them into one general continuous system with the metallic masses in the hull, and with the sea.

These cases are particularly interesting, and conclusive of the general question of the protection to be afforded by such a system.

No. 1.—In September 1833, H.M. ship Hyacinth had both the fore and main-top masts and top-gallant masts destroyed by lightning in the Indian Ocean. The electric fluid shivered these masts from the truck to the heel of the topmast, as indicated by the waving black line *a b* in the annexed diagram, fig. 1, which represents the effects on the main mast; at the point *b*, it became assisted by the chain topsail sheet leading to the deck at *c*, and so did no further damage to the mast; at *d* it received further assistance from the copper pipe of Hearle's patent pump, leading to a small well at *e*, and thence by a second pipe through the ship's side under water, and by this passed safely into the sea*.

26. Now it is evident here that a heavy discharge of lightning which shivered completely a sloop of war's main-top mast and top-gallant mast varying from 11 inches to a foot in diameter through a length of at least 80 feet, was conducted without damage or fusion by an iron chain and a short copper pipe. It is therefore important to state the dimensions of these metallic bodies. Now the iron chain consisted of links $2\frac{1}{4}$ inches long, made of iron rod $\frac{1}{2}$ inch in diameter. It reached from the lower yard to the deck, a distance of about 50 feet.

The pump consisted of copper pipe 4 pounds to the square foot; it was 3 inches in diameter, and about the $\frac{1}{11}$th of an inch thick, extending through a distance of about 10 feet.

The effects on the foremast were very similar, they are omitted therefore for the sake of brevity.

27. It is not a little remarkable, that five years after this, in 1838, this same ship was again struck by lightning, whilst at

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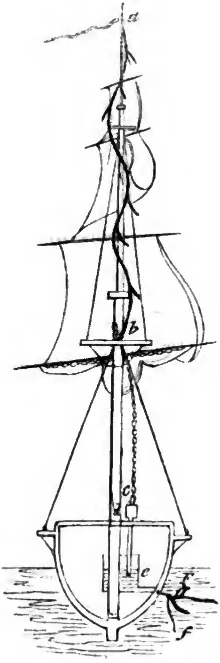
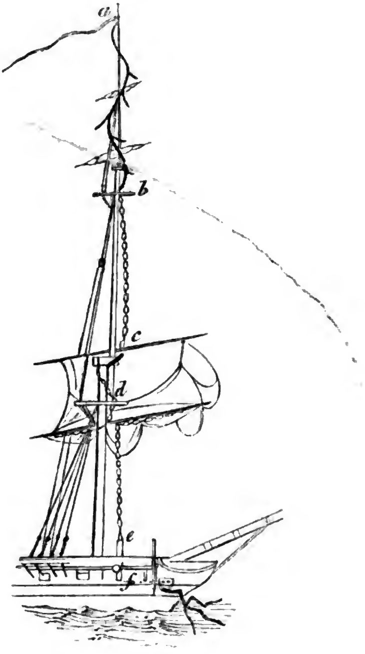
* These circumstances are minutely detailed by Capt. Blackwood, who commanded the ship at the time, and may be seen in his interesting letter on the subject, in the Nautical Magazine, vol. viii., p. 116.

Fig. 1.



Hyacinth.

Fig. 2.



Athol.

anchor in Penang Bay, and again lost her main-top mast and top-gallant mast in a similar way, the lower mast being preserved by her chain topsail sheets.

28. No. 2. In 1830, the Athol, of 28 guns, was struck by lightning on her foremast, in the Bight of Biaffra: at this time the topsails were lowered on the caps and the other sails furled, as showed in the annexed diagram, fig. 2. This ship had chains for hoisting the topsails which lay in the direction of the topmast as indicated by the dotted line *b c*. She had also a chain for topsail sheets, which led along the lower masts as indicated by the line *d e*. When the electrical explosion

fell on the truck it shivered the top-gallant mast in pieces so far as the commencement of the chain at *b*; here being assisted by the chain, it passed on *without* any damage to the topmast, which is extremely worthy of remark, because in the former case, where there was no chain, the top-mast was destroyed.

Having reached the point *c*, where the chain terminated, it passed *with* damage over the head of the mast, until again being assisted by the lower chain *d e*, it passed *without* damage to the deck; on reaching the deck at *e*, it passed by means of a bolt through a beam in the forecastle upon the chain cable, and thence into the sea *.

29. These effects are similar to the former, and show the protection afforded by the chains, and their power of conducting heavy discharges of lightning without any of the ill consequences insisted on by Mr. Sturgeon; since in both cases the chains were in the vicinity of large metallic masses, viz. the iron hoops, iron-bound blocks, &c. about the masts, and in both cases the lightning passed through the hull. Now as all the laws of nature are general, not partial, it is reasonable to infer, that if Mr. Sturgeon's view of a lateral explosion were true, it ought to apply in such palpable cases as these, more especially when he says he can produce a lateral explosion at 50 feet distance with a jar of only "a quart capacity."

30. No. 3. The effects of lightning on H.M.S. Snake, is another striking instance of the general laws we have been contending for. The phænomena are detailed with peculiar clearness by Capt. Milne in the March number of the Nautical Magazine. The electric fluid entered main truck, shivered royal mast, splintered top-gallant mast; then over *chain* main topsail tye *without* damage to within 8 feet of the deck so far as the topsail halliards.

Finding, as observed by Captain Milne, an obstruction here in the ropes, it again seized on the mast, and became divided at the saddle of main boom; one portion passed out of quarter-deck port to the sea, the other to lower deck and down the mast, and distributed itself over the hull, affecting persons below. The mast, on being examined at Halifax, was sprung about the partners two inches deep and 15 inches round, and was *perfectly burst asunder at the step*: hence the shock had extended to the heel, the electric matter, consequently, must have passed by the metallic bolts in the keelson to the sea.

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* An interesting and authentic account of this circumstance will be found in the Nautical Magazine, vol. viii., p. 114.

It is further stated, and it is *a most important fact*, that a seaman *aloft on the cross trees*, at the time, did not experience any sensation whatever.

31. No. 4. The Buzzard brigantine was struck by lightning on the Coast of Africa in February 1838, and lost her top-gallant and topmast, under precisely the same circumstances as those of the Hyacinth, the lower mast being preserved by the chain topsail sheet [*].

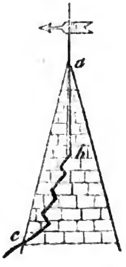
32. No. 5. The Fox revenue cutter was struck by lightning in March 1818. The mast was furrowed and otherwise damaged in every part *except where it was coppered*; as appears by a minute made at the time by the master mast-maker at the Plymouth dock-yard.

Now the copper usually placed about a cutter's mast is not the $\frac{1}{32}$nd part of an inch in thickness. In this case it remained perfect.

33. No. 6. The spire of a church at Kingsbridge in Devonshire was struck by lightning in June 1828, and fearfully damaged. This case is particularly worthy of notice.

The lightning fell on an iron spill, *a*, *b*, supporting the weather-cock, about 7 feet in length and 1 inch in diameter. On this it produced no visible effect, nor did *any damage arise to the stone-work about the rod*. It was not until the rod ceased at the point *b* that the masonry was rent [†].

Fig. 3.



34. No. 7. Extract from a letter from Lieut. Sullivan, of H.M.S. "Beagle," addressed to the Editor of the Annals of Electricity, &c., &c., relative to the protection afforded by a continuous conductor attached to the mast of H.M.S. Beagle.

"Having considered your communication in the Annals of Electricity on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. Beagle, I think it fair, both to Mr. Harris and the naval service, to describe the phænomena I witnessed on that occasion; first stating, that at the time of my joining the Beagle in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris, and certainly *no bias* in favour of the conductors with which the ship was fitted. I may therefore claim to be considered an impartial observer.

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[*] This case was given me by the commander Lieut. Fox. I was myself on board the vessel on her arrival. The particulars are noted in her log.

[†] MS. letter with a drawing, dated July 11, 1828, from the Rev. G. F. Wise, late Vicar of Kingsbridge.

"At the time alluded to, I was first Lieutenant of the Beagle, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H.M. ship Thetis at Rio Janeiro a few years before, when her *foremast was entirely destroyed by lightning*, my attention was always particularly directed to approaching electric storms, and especially on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal if not superior to the shock I felt in the Thetis; one of the clouds by which we were enveloped had evidently burst upon the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that mast; the vessel was shaken by the shock, and an unusual tremulous motion could be distinctly felt. As soon as I had recovered from the surprise of the moment, I ran down below to state what I saw, and to see if the conductors below had been affected; and just as I entered the gun-room, the purser, Mr. Rowlett, ran out of his cabin, (along the beam of which a main branch of the conductor passed) and said that he was sure the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill consequence was experienced; and I cannot refrain from expressing my conviction, that had it not been for the conductor the results would have been of very serious moment.

"This was not the only instance where we consider that the vessel had been saved from being damaged by lightning by Mr. Harris's conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship.

"Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris's conductors.

"I can, therefore, only repeat my conviction that the Beagle was struck by lightning in the usual way, and certainly without any *lateral* explosion or other ill effects similar to those you insist on in your Annals of Electricity."

35. Now these facts are totally subversive of all Mr. Sturgeon has advanced concerning his destructive lateral explosion

in the way of objection to the fixing conductors in ships' masts, and prove in the most conclusive manner the protecting power of such conductors: his statement, therefore, that "destructive lateral discharges will always take place when the vicinal bodies are capacious and near the primitive conductor or to any of its metallic appendages," is clearly fallacious.

36. It is allowed by writers on inductive science, that we wander from the true path of philosophical inquiry, and take up that of assumption and conjecture, directly we cease to verify our principles by an appeal to facts. In order to arrive at a general law of nature, it is requisite to examine carefully a great number of facts bearing directly on the question at issue, and show, that the principle we assume is common to them all; for if in any case the assumed principle is decidedly negatived, it is at least a powerful exception; and it *may* be sufficient to overturn our whole theory.

If such exceptions are numerous, any theory which cannot include them is decidedly untenable.

It has been well observed by Abercrombie [*] that in deducing a general principle, "when the deduction is made from a full examination of *all* the individual cases, and the general fact shown to apply to them all, this is truth; when it is deduced from a small number of observations and extended to others to which it *does not apply*, this is falsehood."

37. In applying these principles, we find Mr. Sturgeon's assumed lateral explosion decidedly negatived in all the cases just cited, since we do not find any such occur in the passage of heavy discharges of lightning along the masts, &c.; we do not find, as asserted by him, anything like electrical waves produced by the discharge through a conductor situated close to the magazine. Thus in the case of the Hyacinth, No. 1. the copper pump, *d e*, fig. 1, was a conductor near the after magazine. Yet the electric shock, in passing down this and through the ship's side, did not cause "intense sparks among the powder barrels, whose metallic linings and hoops reciprocally interchange them [†]."

38. Again, we do not find in the passage of a dense explosion of lightning that the sailors are necessarily subjected to lateral discharge, since in the case of the Snake, it may be observed that a seaman aloft on the cross-trees did not experience *any sensation* whatever, although the top-gallant mast was shivered, and a terrific shock darted from the heel of it to the chain topsail tye. Now if Mr. Sturgeon's views were

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[*] On the Intellectual Powers.

[†] Sturgeon's Memoir.

practically sound, this man ought to have been killed on the spot by a "*lateral discharge*," as he says happened to a seaman called Wilson in the case of the Rodney.

39. Mr. Sturgeon, therefore, if he still adheres to his theory, is at last reduced to the necessity of supposing, that his lateral discharge may sometimes occur, and sometimes not, which is manifestly in the teeth of his own hypothesis. This instance just quoted of the little effect experienced by persons in the vicinity of heavy electrical discharges is by no means a solitary one, as the following extract from a letter from Admiral Hawker, with which he favoured me relative to the damage done to the Mignomne, very fully shows:—

"The circumstances of the Mignomne being struck by lightning were these: she had been on shore, and was going to Port Royal, Jamaica, attended by the Désirée; we had a day I think the hottest I ever experienced in the W. Indies, without a cloud. After sunset we observed clouds rising up from every part of the horizon with thunder and lightning. I ordered the topsails to be lowered in case of squalls, and we ran down towards Port Royal: about midnight the heavens seemed to be one continued flame, and soon after the main top-mast was shattered into probably fifty pieces, scattering the splinters in all directions; the mainmast was split down to the keelson, and a sulphurous smell came up from the hold, which occasioned some to cry out that the ship was on fire. Two men were killed in the main-top, being burnt black, and having some splinters sticking in them, and a man who was sleeping on the lower deck with his head on a bag (for the ship having been on the rocks for three days there were no hammocks) near the armourer's bench was found dead, with one black speck in his side; *another man sleeping by him was not hurt*."

40. The number of instances in which dense explosions of lightning have passed very near to persons without causing any serious injury to them is remarkable.

Thus in the case of the Buzzard, No. 4, before mentioned; the explosion at the time of shivering the top-mast passed so near to a seaman called Robert Purk, that it actually tore the shirt from his arm: he very kindly showed me the shirt, and pointed out the place where he was standing. Lieut. Fox, who commanded this vessel, and who was good enough to send me an account of the damage, &c. sustained, says, in allusion to this circumstance, "The lightning took a strip out of the shirt about two inches wide from the shoulder to the wrist without hurting him."

No. 9.—In the instance of the Hawk cutter, lately struck by lightning on the west coast of Erris, and seriously damaged, it appears that the electric matter in passing down the main hatchway passed between a man and a boy. Neither were hurt; the latter experienced a shock only. It also passed close to another man lying across a hammock about the same spot, who jumped up and thought his neck handkerchief was on fire; the latter experienced a temporary effect only in his right arm.

41. All these cases evidently show, that no damage occurs from a shock of lightning *out of its direct path.* It may, however, divide in the absence of any good conducting course, and branch out into a variety of other courses (as already observed) and seize either wholly or partially upon bodies which happen to lie in certain points, as clearly shown in all these cases, and in the partial fusion of the leaf-gold given in experiment 2, p. 124, of my last communication.

We may also expect to find an *expansive* effect of greater or less force in the vicinity of a discharge of *free electricity* under the form of a dense spark, in *a bad conducting interval*; as observed by Dr. Priestley, "the air being suddenly displaced gives a concussion to all the bodies which happen to be near it."

42. It is clear therefore that in all cases where injury or death has occurred, as in those before given in the Mignomne, Rodney, &c., it has been the result of the passage of the electric agency, either wholly or partially, through the animal body, and not from the result of any *lateral explosion* of electricity, such as described by Mr. Sturgeon. If, as he says, such explosions in all cases of proximity to the primitive charge necessarily arise, such proximity to the passage of a dense shock of lightning would be in all cases fatal, which is evidently not the case.

43. I have now to consider briefly a few instances of the power of metallic bodies to transmit heavy discharges of lightning.

In the case above quoted of the Hyacinth, we observe, as already remarked, that a flash of lightning which shivered the top-mast and top-gallant mast passed over a small iron chain and copper tube without fusing either. A similar result ensued in the second instance of the Hyacinth being struck by lightning; also in the case of the Athol and Buzzard, and Snake, and in a great variety of others too numerous to detail here.

In the case of the Fox, No. 5, it is seen that the shock of lightning which damaged the mast, was conducted without fusion or damage by sheet copper of $\frac{1}{32}$nd of an inch in thickness placed in the wake of the gaff. This is conclusive of the fallacy of Mr. Sturgeon's assertion, that any conductor applied to the mast, would, under the operation of lightning, be "probably peeled from the wood."

In the case of the Kingsbridge spire, No. 6. The lightning which shivered the tower, fell on a cylindrical iron rod of an inch diameter without producing any effect on it.

In the case of the Rodney, the flash which set the top on fire and splintered the masts, was conducted by a short copper funnel for top-gallant rigging without fusion.

In the case of the Beagle, No. 7, a shock of lightning passed down the conductors without producing any effect on them.

No. 10. A house was struck at Tenterden; the lightning fell on an iron bar three-quarters of an inch square, but produced no effect on it.*

No. 11. A stroke of lightning fell on Mr. West's house at Philadelphia, having a conductor terminating in a brass rod ten inches long and *a quarter of an inch in diameter*; only a few inches of the point were melted, but no damage occurred to the building†.

No. 12. On the 19th of April 1827, one of the large New York packets, whilst in the Gulph Stream, was assailed by two most awful strokes of lightning twice in the same day. The first shock was productive of serious and destructive effects. The second shock fell on a pointed conductor subsequently hoisted to the main-mast head. This conductor consisted of an iron chain having links of a quarter of an inch thick and two feet in length and turned into hooks at each end, connected by rings of the same thickness, and one inch annular diameter. This conductor was *attached* to an iron rod placed at the mast head, half an inch thick and four feet long. The explosion fell in a *concentrated* form, and with an awful crash upon this rod. Although the small chain below was disjointed and some of the links fused, yet this pointed iron rod was only fused for a few inches. *The ship in the second case escaped danger.*

Now these are authenticated cases, and there are numerous others which I might adduce, to show how perfectly *capacious* and *continuous* conductors transmit shocks of lightning.

44. No good instance can be adduced in which conductors of great capacity have been even moderately heated by lightning. I do not admit Mr. Sturgeon's "on dit" respecting the conductor passing through the Nelson Monument in Edinburgh. It is really no evidence whatever on a scientific ques-

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* Philosophical Transactions. † Ibid.

tion. "*It is said* (observes Mr. Sturgeon) that the lightning rod passing through the Nelson Monument became so hot by lightning that it could not be touched by the hand by *the first person* who visited it afterwards. Allowing a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been made red-hot." This is of the same character with all Mr. Sturgeon's data; it is generally surmise, the *show* without the reality; it just amounts to nothing.

45. I am aware that it has been also *supposed* that the great conductors of St. Paul's church were heated by lightning, but it is only a *supposition.* The conductors were not examined before the lightning, which was said to have fallen on them, occurred, so that we cannot be certain that the observed appearances were not originally present after the forging of them; it is besides very unlikely, that a stroke of lightning should have fallen on this building, capable of rendering bars of iron, six inches wide and one inch and a half thick, red-hot, without destroying the thin copper covering the ball and cross on the dome of the building, and without the crash of the thunder having been heard over the whole city, no mention of which is made; when St. Bride's steeple was struck, the latter was peculiarly remarkable.

46. There is another instance on record, of the effects of lightning on an iron rod, in Port Royal, Jamaica, mentioned in the Transactions of the Royal Society, the evidence of which seems very incomplete. Two men are said to have perished by lightning near a church wall: that is not improbable: but, on subsequently looking inside the wall, a bar of iron an inch thick, and a foot in length, was found in many places wasted away to the size of a fine wire. Now it does not appear that this bar was examined *previously* to the occurrence of the lightning; hence we cannot infer that the wasting was produced by the electric fluid; more especially as similar appearances are not uncommon in bars of iron erected in churchyards in this country, and which have evidently resulted from oxidation and time.

47. Seeing then how much evidence we have from actual experience of the protective effect of regular conductors of the *worst* kind, and their power of transmitting dense explosions of lightning, we may reasonably infer that a conductor of copper equal to a rod of an inch diameter, and *extending the whole length of the mast*, would be proof against any discharge of lightning ever experienced, as, I think, is shown by the cases in which ships fitted with my conductors have been struck by shocks of lightning without damage.

48. Exceptions, however, have been taken by Mr. Sturgeon to the phænomena described by the officers who either commanded or were in the ships. Thus Captain Turner, in describing the shock of lightning which fell on the Dryad frigate on the coast of Africa, says, that "he saw the lightning on the conductor on the fore-mast, and saw it during another flash run down the mizen-mast; that all the men there heard a loud whizzing noise." Captain Fitzroy and Lieut. Sullivan also mention similar phænomena. Now the exceptions taken are these, viz. that no noise is ever produced by electricity entering a conductor, and that we cannot produce a "running light" upon a conductor carrying an electrical charge.

These exceptions, however, are rather captious objections to forms of expression, than to the facts themselves; it is easy to show from experience that luminous appearances are often attendant on discharges of both natural and artificial electricity.

Thus in the case of the Hawk (No. 9.) the account states that "the vessel was apparently enveloped in a flame of lightning;" whilst in the case of the Beagle, Lieut. Sullivan says, "on looking aloft the ship was apparently in a blaze of fire." In the case of the Snake (No. 3.) the electric fluid is said to have *descended* with an instantaneous explosion of a vivid purple colour.

When H.M. ship Norge was struck by lightning in Port Royal harbour, the electric fluid was observed (to use Admiral Rodd's expression) to "absolutely stream down a conductor attached to the mast of H.M. ship Warrior," close by.

Such phænomena are besides remarkably close to the results of experiments: thus a heavy shock of electricity, passed over a metallic wire in a partially exhausted receiver, will exhibit a transiently passing light on its surface.

49. The whizzing noise is quite in accordance with common electrical effects. It invariably occurs when a good conductor receives and disarms an explosion by a pointed extremity. Mr. Sturgeon, however, asserts that "no such noise is ever produced by the *fluid entering* a metallic conductor." This is mere sophistry; let any one attempt to discharge a highly charged battery by an acutely pointed conductor. A great part of the charge will immediately rush through or towards the point with a whizzing noise. Now the stratum of cloud may be either positively or negatively electrified, and whether the one or the other, it is clear that the rush of electricity from a charged surface toward a point, or from a point towards an

undercharged surface (according to Franklin's hypothesis) will be always attended by a whizzing noise.

50. The protection which continuous conductors would afford if well and efficiently applied to ships is, I think, apparent in all the preceding cases, and when we consider that the masts are themselves conductors of electricity, and that by their position alone they determine the course of the discharge into the body of the hull, it becomes the more requisite to affix to them good conductors, which quickly disperse and reduce the electrical action to a state of quiescence.

We have I think fair evidence of this in the trials hitherto made with the continuous fixed conductors applied to certain ships of the British navy.

51. These ships have been exposed more or less in all points of the world. Lightning has not fallen upon them *oftener* than other vessels not so fitted; and *when it has done so no damage* has arisen in any way, or has any destructive lateral effect, such as that contended for by Mr. Sturgeon, taken place. His comparison, therefore, of the effects of lightning on the Rodney with the "*probable* effects" (as he terms it) on my conductors, although he can find no instance of such *probable effects*, is therefore purely hypothetical. If Mr. Sturgeon has no good authenticated fact to oppose to the mass of evidence I have adduced, of what avail is any hypothetical or loose opinion he may find it *convenient* to advance?

52. Before concluding this communication, I cannot refrain from pointing out the apparent inconsistencies of his views on this point. Having described my conductors as dangerous and objectionable in every possible way, as calculated to induce oblique flashes of lightning to strike the ship to the destruction of the sailors' lives, the sails, rigging, &c. &c., he says, sect. 221, on discovering that he could not conveniently apply his own rods above the top-mast head, "as *however every chance of danger to the men and every species of damage* to the vessel ought *strictly to be avoided*, it still appears desirable to furnish the top-gallant rigging with conductors; and perhaps those which would give the least trouble to the men, would be strips of *copper let into grooves in the masts according to the plan proposed by Mr. Harris.*" Now, I think, it must be clear to any one, that if my system be so objectionable as he would have it believed, on the grounds above stated, it must be equally objectionable on the top-gallant masts; the lives of the sailors are just as much exposed there as at any other point, perhaps more so. Mr. Sturgeon himself admits that two men were killed there in

the case of the Rodney. But by his admission above-quoted, my method is not objectionable in the top-gallant mast, but is on the contrary calculated to avoid "every species of danger to the vessel and every chance of danger to the men;" if so, it must be equally efficient on the top-mast, lower mast, &c. This sort of traverse sailing, to use a nautical phrase, is not a little amusing, and is, I believe, quite unprecedented in any paper on science.

53. In order that no mistake may arise in respect of what I have advanced relating to lateral explosions, I may in conclusion simply state, that I do not deny the expansive force of a dense electrical explosion, and its destructive effect on *imperfect* and *non-conductors*. I do not deny its effect in causing expansion in the surrounding air, which I rather choose to call with Priestley, "the lateral FORCE of electrical explosions," than a *lateral explosion of electricity*. I do not deny this in the absence of any regular system of conductors, or that the discharge may divide in several directions, and in distributing itself over the hull, may cause dense sparks and other electrical appearances in various parts of the vessel, but which would not appear, if a perfect system of conduction, such as I have proposed, were resorted to.

I do however deny the probability of any lateral discharge of electric matter from conducting bodies transmitting an accumulation between oppositely charged surfaces, as assumed by several persons imperfectly acquainted with ordinary electrical action, and lately by Mr. Sturgeon; and, I maintain, that neither artificially, nor in the course of nature, can any instance of such lateral explosion be authenticated.

I am, Gentlemen, yours, &c.

Plymouth, March 14, 1840. W. SNOW HARRIS.

P.S. It has been insisted on by Mr. Sturgeon, that a shock of lightning, descending a continuous conductor on the mast, would magnetize every chronometer in the cabin, &c.—(Memoir, Sect. 207.)

This assumption is completely negatived by the cases above quoted. In fig. 1, an awful discharge descended an iron chain, and yet no magnetic effect was observable on the neighbouring compasses, or on the chronometer in the cabin. It is only in the *absence* of continuous conductors we find such magnetic effects, and even then their occurrence is comparatively rare. Really, Mr. Sturgeon makes so many random assertions, it is almost impossible to attend to them all.

LXI. *Remarks on the Compounds derived from the Stearopten of Oil of Peppermint. By* Robert Kane, *M.D., M.R.I.A.*[*]

In my paper on the constitution of the essential oils, I noticed that the composition of the oil of peppermint as determined by my analyses approximated to that announced by Blanchet and Sell as belonging to the solid crystalline substance which often forms in it; but at the same time I stated, that from the utter discordance of Blanchet's results among themselves, no confidence could be placed in them. The formula I adopted for oil of peppermint is $C_{21} H_{20} O_2$, and in order to establish a more direct comparison I will subjoin two of the analyses by which that formula was established.

	Experiment. I.	Experiment. II.	Theory.		
Carbon......	78·06	77·81	C_{21}	128·9	78·14
Hydrogen ..	12·32	12·11	H_{20}	20·0	12·12
Oxygen ...	9·62	10·08	O_2	16·0	9·74
	100·00	100·00		164·9	100·00

The two analyses of the stearopten made by Sell and Blanchet, gave results which I will also subjoin.

	Experiment. I.	Experiment. II.	Theory.		
Carbon......	79·63	77·27	C_{10}	61·40	77·28
Hydrogen ..	11·25	12·96	H_{10}	10·00	12·59
Oxygen ...	9·12	9·77	O	8·00	10·12
				79·40	100·00

The total discordance of these results, coupled with the fact, that on analysing the liquid oil of peppermint Blanchet and Sell had obtained numbers quite different from each other, and from the truth, shows that for purposes of further research the investigations of Blanchet and Sell cannot be taken as a standard.

Mr. Walter has recently published a memoir on the crystallized essence (stearopten) of peppermint, in which he lays down as the basis of his very interesting researches Blanchet's formula, and supposes that its truth is confirmed by his own analyses. With this I should not, however, have anything to do, had not Mr. Walter made the same observation with me, of how close my formula for the liquid oil approached to Blanchet's for the stearopten, and insinuated indeed pretty broadly, although without having analysed the liquid oil, which he might so readily have done, that the formula of Blanchet is the true one, and that mine, in as far as it differs from it, is likely to be incorrect. His words are: "Si l'essence de menthe poivrée liquide présente la même composition que l'essence de menthe concrète, ce qui est très probable d'après les observations de M. Robert Kane, qui est conduit cependant, quoique le rapport numérique qu'il a trouvé, soit le même, à adopter une formule différent de celle que je viens de présenter, formule du reste qui n'est déduite ni de la densité de la vapeur, ni d'aucune combinaison dans lequelle l'essence de menthe rentrerait," &c. Now I purpose to show in this notice that Mr. Walter has not done justice to his own analyses; for that by giving them their just interpretation, he would have found Blanchet's formula to be inexact, and that the stearopten possesses the precise constitution which I have assigned for the oil.

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[*] Communicated by the Author.

In an organic analysis the sources of error act in opposite directions on the carbon and on the hydrogen; there cannot be more carbon obtained than was present in the substance; and from the facility with which the hydrogen is burned, there is never less water obtained. In practice, a chemist very seldom succeeds in a complete combustion of the carbon; and almost in all cases, from the hygrometric nature of his materials, he gets more water than he ought. Hence in deducing a formula from a set of analyses, the highest number obtained for the carbon, and the lowest number obtained for the hydrogen, are those most likely to be true, and are those on which the formula should be constructed. There may be cases in which water may be lost, but it must arise either from an error in weighing or from bad management in the analysis. Setting out from these principles I will proceed to discuss Mr. Walter's numerical results.

In six analyses of the solid oil, the highest value he obtained for the carbon was 77·68, and the mean of five results was 77·36. The lowest result for hydrogen was 12·52 and the mean of four was 12·66. Mr. Walter's analyses and theory are therefore,—

	Analysis.	Analysis.		Theory.
Carbon......	77·68	77·36	C_{20}	77·27
Hydrogen ..	12·52	12·66	H_{20}	12·62
Oxygen	9·80	9·98	O	10·11
	100·00	100·00		100·00

It is also quite evident that the results of Mr. Walter's analyses cannot be considered as exceedingly correct, when we find that out of his six analyses, the carbonic acid was lost in one, and the water in two instances; probably from a too rapid and imperfect process of combustion. Mr. Walter's formula is therefore disproved by his own analyses, which give too much carbon and too little hydrogen; but they agree very well with the formula I proposed, if we allow his analyses to have been of the average degree of excellence.

The specific gravity of the vapour of the stearopten furnishes no test whereby to distinguish between the formulæ; $C_{20}\,H_{20}\,O_2$ gives 5·455, and $C_{21}\,H_{20}\,O_2$ gives 5·666. He obtained 5·62. It is true, the experimental density generally comes out a little higher than the calculated density; but on the other hand, with these oils there is almost universally a trace of decomposition by which the experimental density is thrown too low. Hence the density found agrees with one formula as well as the other.

Mr. Walter has given the name of menthene to a hydrocarbon, produced by the action of dry phosphoric acid on the stearopten. To this body he assigns the formula $C_{20}\,H_{18}$: his results are,

	I.	II.	III.	Theory.
Carbon	87·74	87·53	87·59	87·18
Hydrogen...	12·99	12·85	12·71	12·82
	100·73	100·38	100·30	100·00

In every case Mr. Walter obtained an excess of weight which must be an error in the hydrogen, and in every case the hydrogen increased by this error comes only to about equal the hydrogen required by his formula. In every case also he obtained too much carbon, and this with a substance whose perfect combustion must evidently to every organic chemist be most difficult. His formula cannot be true: but the formula $C_{21}\,H_{18}$ gives the following numbers:—

C_{21} =	128·9	87·76
H_{18} =	18·0	12·24
	146·9	100·00

coinciding perfectly with the analyses.

The density of the vapour of menthene Mr. Walter found to be 4·95. The formula $C_{20}\,H_{18}$ gives 4·835. The formula $C_{21}\,H_{18}$ gives 5·046. The difference here also is so very trifling that no stress can be laid upon it one way or the other.

The results obtained by the action of sulphuric acid, not being definite, do not require notice, further than to mention that the formula I announced for the sulphodadylate of lime, was $SO_3 . Ca O + C_{20} H_{16}$, and not the formula which Mr. Walter attributes to me.

The substance obtained by Mr. Walter in acting on the stearopten with perchloride of phosphorus is of great importance, as giving some evidence of how far this essential oil acts as an alcohol. The action appears to be very complicated, and hence it is to the analysis alone that we can look for explanation. Taking for the best analysis the highest carbon and the lowest hydrogen, the results of Mr. Walter are,

	Best analysis.	Mean of six.
Carbon	70·55	70·01
Hydrogen...	9·89	10·31

The chlorine, determined once, gave 20·87.

The most natural formula is to suppose that, as with alcohol or acetone, water is eliminated and decomposed, a sort of muriatic æther being produced. Hence the formula should be $C_{20} H_{19} Cl$ or $C_{21} H_{19} Cl$. The numbers are,

C_{20}	69·26	......	C_{21}	70·32
H_{19}	10·72	......	H_{19}	10·36
Cl	20·02	......	Cl	19·31
	100·00			100·00

Here with the C_{20} of Blanchet, there is again too much carbon and too little hydrogen, but with the C_{21} as on my view, the numbers are more nearly true. But Mr. Walter suggests that the hydrogen may be 18 atoms in place of 19, and then the formulæ give

C_{20}	69·6	C_{21}	70·71
H_{18}	10·3	H_{18}	9·87
Cl	20·1	Cl	19·42
	100·0		100·00

Thus we have still on Blanchet's basis too little carbon and too much hydrogen in the formula. On the basis of C_{21} the formula becomes much more likely to be true; at all events the third formula suggested by Mr. Walter $C_{20} H_{17} Cl$ is quite unnecessary. I consider this body to be chloride of menthene.

Although we should find similar examples in the products formed by the action of chlorine on the stearopten, yet I will not enter into any discussion relating to them, as from the analytical results, and other circumstances, it is evident that the final and definite products of that action have not yet been obtained. In Mr. Walter's formulæ there come into play half equivalents of chlorine and hydrogen, indicating that the substances analysed were not yet definitely characterized.

The peculiar acid body formed by treating the essence with nitric acid has its origin in decompositions so complex, that, until its atomic weight and composition shall have been accurately determined by the analysis of its salts, it is totally useless to discuss it in relation to the present question. But here as in the other instances Mr. Walter has got too much carbon, although not too little hydrogen, and the additional quantity of carbon given by my number makes his analytical results more consonant to the theory. I do not wish, however, to be considered as applying the formula $C_{21}\ H_{20}\ O_2$ to explain the origin of this body, which indeed I consider to belong to a totally different series.

In concluding, I must observe that I admire very much the general exactness of Mr. Walter's results, and the skill which he has displayed in this and other difficult investigations, by which he has been so highly distinguished. In fact, it is greatly to his credit that his analyses were so good, although he had been beguiled by the authority of Blanchet to adopt an insecure basis at his outset; and I have written these remarks not to diminish Mr. Walter's merit, which none can be more ready to express the highest sense of, but to show that all Mr. Walter's investigations have but confirmed my former results, and that they have fully proved, that the liquid and the solid oil of peppermint have the same constitution, and that although I had not confirmed my formula by the accessory methods generally employed, the confirmations have come unconsciously from Mr. Walter's hands, and that the formula $C_{21}\ H_{20}\ O_2$ is that which alone accounts for his interesting results.

LXII. *On the Galvanic Properties of the Elementary Bodies, and on the Amalgamation of Zinc. By* Alfred Smee, *Esq.*

[In continuation of a former paper, p. 315.]

THE first non-metallic element we have to examine is carbon.

When a diamond is placed in contact with amalgamated zinc in dilute sulphuric acid, no gas is given off, nor copper precipitated on it from a solution of that metal when touched by zinc. Gas coke, however, recently ignited, or plumbago, placed under similar circumstances, copiously evolves hydrogen from its surface. The same circumstance is noticed with the various forms of porous coke and boxwood charcoal, but in these cases no gas is given off for some little time. Observing this, it was a matter of great interest to know what became of the gas for the first few seconds, and it directly occurred that the first portions of gas were bound down in a nascent state with the charcoal: this was proved by placing it in a solution of sulphate of copper, when the charcoal and the coke became coated with a thin film of the metal. In the same way gold, silver, mercury, and lead were precipitated from their solutions, and iodine set free from iodic acid. Probably the other metals were also precipitated, but their colours render a thin film difficult to be distinguished. When charcoal or the porous coke is made to form the electrodes of a battery, the piece forming the kathode or platinode is found to have similar properties; but the anode or zincode, however, is found to possess nascent oxygen from its liberating chlorine from muriatic acid, though this is not quite so satisfactory as the experiment with the hydrogen. The gas coke and plumbago are found not to possess the property of retaining the gases. Occasionally charcoal will be found to precipitate gold and silver from their solutions, but in these cases copper, and those metals which have a greater affinity for oxygen, are not reduced.

View the importance of these experiments, as they demonstratively prove that which has hitherto been the prevailing theory, namely, that nascent hydrogen precipitates the metals, and that the precipitation may take place when the galvanic current is broken; for the coke will retain its hydrogen in some cases for forty-eight or more hours. Now in what state is the hydrogen when it has these properties? Is it in the form of minute bubbles adhering to the surface? This would appear to be a mystery. It is probably in a state analogous to solution; for if a piece of smooth platinum be placed in contact with zinc till minute bubbles are covering its whole surface, and then the zinc be removed and a solution of a metal be poured upon the platinum in such a way that the bubbles are not disturbed, no precipitation takes place; and even spongy platinum or spongy palladium fails under the same circumstances to precipitate the metal.

Much difficulty arises in naming the two poles of a battery; they are called the positive end and the negative end, the anode and the kathode, the platinode and zincode; now as each pole of a simple battery becomes reversed if the battery is doubled, it is better to name the two ends from the oxygen and hydrogen; since we have shown that the galvanic current owes its power of decomposing many substances entirely to these gases. The names which are proposed are the oxode, at which oxygen is evolved, and the hydrogode, where the hydrogen is given off.

The soft and spongy charcoals, as those of deal, possess the property of evolving gas very imperfectly.

Various kinds of coal, such as anthracite and cannel, were tried, but none were found to evolve hydrogen, nor to have copper precipitated when the circuit was made in a solution of that metal.

From the above experiments we see that batteries may be constructed of carbon in the place of a negative metal; the hard coke or plumbago answering best, and the porous coke and box-wood charcoal next*. These may be used as an ordinary battery with sulphuric acid, but of course a battery thus constructed possesses but little power. If, however, the hydrogen is removed upon Professor Daniell's principle, then will the power be increased, and a charcoal battery may be made of surprising energy. The hydrogen may be removed by metallic solutions which have a feeble affinity for oxygen, and therefore those of gold, silver, platinum, or copper would answer best; the latter being the only one in use from its cheapness. The highly oxygenated acids, such as nitric, &c., are more powerful than these, and are now considerably employed, but disadvantages attend their action; for if the current is required to be continued for a long time, a large quantity of acid must be used, and the fumes arising from the battery are injurious to the animal œconomy: in addition, the strong acid is liable to be spilt over the fingers or clothes; and lastly, it always transudes through the porous tubes and acts upon the zinc, even when amalgamated, to a considerable extent.

It is perhaps worthy of notice, that the powers of the nitric acid battery are not to be attributed to the fluids alone, for no current is formed when platinum is used in both cells. Strong sulphuric acid produced scarcely any action, but the addition of nitric acid rendered it powerful, for a time proportionate to the quantity of the latter acid used. I have tried other substances which have an affinity for hydrogen, such as chlorine, iodine, chloride of lime, peroxide of iron (or a mixture of muriatic acid and peroxide of manganese), so that nascent chlorine may be evolved during the action of the battery; but I find that even with the latter, the action, though powerful, is one quarter less than with strong nitric acid.

A coke battery of two cells, with eight ounces of nitric acid and dilute sulphuric acid, yields ten cubic inches of gas in five minutes. In this case about eight square inches and a half of carbon were exposed, and the communication was effected by means of thick platinum wires. The same quantity of gas was driven off from seven square inches of platinum. One piece of charcoal in a single cell gave one fifth of a cubic inch in twenty minutes.

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* The coke may be cut with a saw into any convenient shape, whilst plumbago, though softer, has the singular property of wearing down every tooth from the instrument in a very short time.

Experiments were performed on the properties of selenium, sulphur, phosphorus, bromine, iodine, and chlorine; but as nothing very worthy of notice was discovered, it will be unnecessary to dwell upon these substances.

The rationale of amalgamating zinc would appear to be exactly the converse of platinizing platinum or the other metals, for the one favours the adhesion of the hydrogen and prevents thereby local action, the other favours the escape of the hydrogen by its non-adhesion, and in that way increases the power. The reasons which I have to offer in support of these opinions are the following:—

When zinc dissolved in mercury is placed in dilute sulphuric acid no action takes place, because the gas cannot be readily evolved, but coats the whole surface; but that there is action really produced, is shown by adding nitrate of silver or sulphate of copper, when the nascent hydrogen is evidenced by the reduction of these metals; as soon as the whole of the metal is reduced it is again inactive, although the elements of a powerful current are there, namely, zinc, silver, acid; now touch the mercury by a piece of silver or the negative metal, the gas will be immediately evolved from it. This explanation appears to me to account for every phænomenon connected with the remarkable properties of amalgamated zinc, which is further confirmed by the attempt to make a galvanic battery with zinc and mercury, the junction being effected exterior to the acid solution, as here great adhesion will be seen to exist between the hydrogen and the mercury.

In conclusion, I have to regret that the continuity of the paper has been necessarily interrupted, but it is a matter of no great consequence, as it treats of many rather dissimilar properties.

Bank of England, April 10, 1840.

LXIII. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron. By* Dr. C. SCHAFHAEUTL, *of Munich.*

[Continued from p. 304.]

TO illustrate this and to arrive at a nearer insight into the chemical composition of iron, I shall select three specimens of cast iron, the one of English, the other two of French production.

(A) Iron from the Maesteg iron-works near Neath in South Wales. Colour white, brilliant and granulated; on the lowest part of the pig appearances of contraction; yielding in some degree to the stroke of the hammer and being very difficult to break into fragments. I am given to understand it is produced from clay iron-stone mixed with some Cornish ore, probably red oxide of iron, and by means of a hot blast. Specific gravity 7·407.

(B) Iron from Vienne in France, département de l'Isere; produced from the ore of La Voulte, (that is red oxide of iron,) mixed with about one-third part of pea-iron-ore. The coke is from Rive de Gier*.

The hot blast was also used here. Colour gray, of great dark graphite-like brilliancy; the scaly appearance very much developed; easy to be broken into small fragments, with a species of tough resistance, making a similar sound as pure tin when beaten, but still difficult to reduce to very fine powder; specific gravity 6·898.

(C) Iron of Creuzot, département de Saone et Loire; produced from bog iron ore, with a mixture of brown iron ore of varying qualities and some puddling slag. The cold blast was applied. The fracture dead grayish; earthy; hardness considerable; brittle, not yielding to the hammer; specific gravity 7·378.

Thirty-five grains of iron, (A) and (B), in small fragments were put into two retorts, and four ounces of hydrochloric acid of 1·16 specific gravity poured over them; the beak of the retort was connected with two Woulfe's bottles, filled with a neutral solution of acetate of lead, temperature 62° Fahr.

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* The coke from Rive de Gier contains, according to Berthier,

75.00	Carbon.
03·50	Volatile matter.
21·50	Ashes (alumina).
100·000	

containing likewise 0·300 pyrites. As soon as this fuel is changed for *purer* coke, gray iron cannot be produced there.

The acid had scarcely been poured over the iron (B), when the whole powdered iron rose, under a rather violent evolution of gas, to the top of the acid. A short time after the acid became of a yellow colour, and the caseoid or cheese-like whitish foam increased each moment on the top of the acid during the evolution of hydrogen, and filling almost the whole of the upper part of the retort.

In Woulfe's bottles distinct glittering scales of sulphuret of lead were rapidly deposited; the gas escaping out of the last bottle had, in an extremely slight degree, in smell the character of hydrogen developed by means of acids from iron, but no longer affected a solution of nitrate or acetate of lead. The liquid was found next day opake, of a light gray colour, still developing gas.

The evolution from iron (A) had ceased long before. The iron (A) or white Welsh iron formed likewise a caseoid or cheese-like substance on the top of the acid, but its colour was dark gray approaching to black, and the liquid below was also of an opake dark gray.

The sulphuret of lead formed in the Woulfe's bottle was not scaly, like that from the gray iron, but resembling a dark-brown greasy viscid mass, making the whole liquid turbid, and only settling two days afterwards. The mass in the retort was then dark gray, with a somewhat lighter sediment.

The residuum in the retort of the gray French iron (B) cast upon a filter, washed without interruption with boiling hot distilled water, was of a soap-like greasy form, and had after being dried a grayish-white flowery appearance. As often as I poured fresh water on the filter, the already collapsed mass began to swell like a sponge, and almost filled the whole filter. This residuum, dried at a temperature of 212° Fahr., weighed 5·53 grains, and had an extremely light appearance resembling silica, chemically separated from minerals.

On 2·1 grains of powder were poured in a test-tube 5 drachms of concentrated caustic ammonia, the test-tube shut with a perforated cork, which contained as usual an S-like bent glass tube; a violent evolution of gas in extremely minute bubbles took place, which ceased only twenty-four hours afterwards. The powder lay on the bottom of the test tube in lenticular aggregates of a gray colour; the evolved gas collected over water measured at 65° Fahr. and 29·35 height of barometer, 0·605 cubic inches, corrected for water 0·586 cubic inches. It was, except small traces of oxygen and azote from the remaining air in the test tube, *pure hydrogen.*

The contents of the test glass poured on a filter were carefully washed with ammonia, and the liquid afterwards evaporated to dryness in a platinum crucible. This dry remainder was perfectly white; only in the corner of the crucible some brown matter was collected, in all probability a species of humine. This dry residuum weighed 0·3 grains, was after ignition perfectly white, and had lost 0·08 grains. It dissolved in hydrochloric acid, left silica behind, which I was unable to weigh, and carbonate of ammonia threw down alumina mixed with some silica.

In consequence of this, 5·53 grains would have contained aluminum equal to 0·298793 grains. To account for 0·0188 hydrogen we ought to have 0·440893 aluminum. By actual analysis I found 0·352583; but it might very well have happened that, in separating alumina from phosphoric acid, a part remained combined with the acid.

The gray residuum which had been treated with ammonia, dried and weighed, was 1·82, and had therefore lost 0·28 grains, that is to say 0·02 grains less than the weight of alumina obtained. This extraordinary evolution of hydrogen takes place with all residuums of hot-and cold-blast gray iron, but with the former more than the latter; and perfectly white iron never evolves any gas whatever; yet white iron which is nearly approaching to gray iron gives always traces of hydrogen.

What causes this extraordinary decomposition of water? We know no other chemical body which, left as a residuum after being treated with acids, possesses the power of decomposing water by the presence of ammonia, except aluminum; and as the ammonia had really dissolved alumina, which only occurs when it comes in contact with the metallic base of alumina, we may safely conclude, that the extraordinary evolution of hydrogen was here produced by the presence of metallic aluminum.

Solution of caustic potash ley likewise decomposes the gray residuums, but only at a higher degree of temperature, and then a species of slight explosion takes place and the fluid is thrown with some violence out of the crucible.

Viewed through a microscope, the residuum of this gray cast iron appears to be composed of white gelatinous transparent nodules, which generally surround a centre, consisting of some dull black spots and of a scale, sometimes shining like graphite, but of a more silvery whiteness. The mixture of these scales with the white nodules gives the powder, to the naked eye, the appearance of having a grayish colour; these scales remain after being treated with ammonia, though not at all apparently diminished, having only lost somewhat of their brightness; the black spots sometimes disappear entirely.

1·72 grains of the remaining gray powder, separated from the filter and ignited, lost 0·203, and seemed in appearance to have undergone no alteration except having become a little more bulky and flocky.

Hydrochloric acid extracted oxide of iron, 0·036.

1·32 grains of this residuum deprived of oxide of iron was mixed with five times its weight of carbonate of soda and ignited: after cooling, the lid of the crucible was found covered with green drops of manganate of soda, and on the bottom was the fluxed mass crystallized, perfectly white on the borders, but having in the middle a dirty yellowish spot.

I separated in the common way

Silica	0·963
Iron and Manganese	0·076
Loss (of Carbon)	0·281
	1·320

The same result was obtained, when, instead of melting the remaining gray powder with alkali, it was boiled in solution of caustic potash and strongly concentrated ley; the silicon is taken up by the potash and iron, and manganese, mixed with some carbonaceous matter, is left in blackish-green bulky flocks on the filter, oxidizing very rapidly.

35 grains of the same metal analysed in the common way gave

Silicon	1·702530 =	4·86430
Aluminum	0·352583 =	1·00738
Manganese	0·262960 =	0·75130
Phosphorus	0·189000 =	0·54000
Sulphur	0·062110 =	0·17740
Carbon	1·183000 =	3·38000
Iron	31·152607 =	89·00740
Loss	00·095210 =	0·27222
	35·000000	100·00000

The white transparent nodules are undoubtedly silica; before the blow-pipe they melt with soda into a transparent globule, separating at the same time black scales of graphite; melted with microcosmic salt, a skeleton of silica is separated as usual. After subtracting silica, nothing remains but a small quantity of iron and manganese, and the loss as already shown is = 0·281, which amounts for 35 grains of iron to 1·17; the actual quantity of carbon was, as we see, 1·183, and therefore we may safely assume the loss to be carbon. The graphite scales, freed by means of solution of caustic soda from the gelatinous silica, and analysed like graphite, as stated in the beginning of this treatise, proved to be of the same composition as the graphite B.

I must here observe, that in fragments broken from the outside of the same pig-metal traces only of sulphur could be discovered; that the malleable iron produced from this according to my puddling process was extremely soft, and had the peculiar property of welding so easily, that tin-plates rolled from it adhered so firmly together by heating them a little too much, that it became impossible to separate them; but the *great quantity* of silicon contained in this iron was extremely destructive to the bottom and sides of the *puddling* furnace.

Let us now return to the residuum of iron (B.); white Welsh iron. This residuum was found to weigh 6·77 grains, and had a dark brown colour and a very strong unpleasant smell, peculiar to hydrogen obtained in this manner from cast iron. It was powerfully attracted by the magnet, and traces only of aluminum could be found.

3·13 grains of the dried residuum, heated cautiously in a weighed platinum crucible, began to glow around the periphery before the crucible became red-hot, and the ignition spread from thence very slowly towards the centre. The platinum crucible was now removed from the fire, covered with its lid and cooled near concentrated sulphuric acid under a bell glass. Its weight had increased 0·08 grains, and its colour changed from brown to black. On the crucible being again placed over the lamp and kept in a red heat for ten minutes, its black colour changed into a dirty light red, and its weight increased equal to . . . . 0·27

On a third repetition of this ignition . . . . 0·02
On a fourth . . . . 0·05
On a fifth. . . . . 0·00

0·42

The whole increase of 6·77 grains would therefore be 0·9084.

It was still as powerfully affected by the magnet as previously to ignition. A part of this powder, 3·55 grains, heated with hydrochloric acid of 1·16 sp. gr., in the same crucible, a grayish powder remained, which presented a distinct interspersion of dull black with white spots, and weighed 0·290; after ignition it had gained 0·028 and became perfectly white.

It consisted entirely of silica, for which we reckon 0·1528 silicon. As the black spots disappeared and it gained 0·028 grains in weight during ignition, we may conclude, that the black spots consisted of metallic silicon, which had become oxidized into silica during the process.

The solution had a greenish-yellow appearance, and after being concentrated by evaporation and diluted with distilled water was mixed with carbonate of magnesia. The precipitate dissolved in hydrochloric acid and again precipitated with succinate of ammonia, yielded 2·50 oxide of iron = 1·7335 metallic iron; no manganese could be discovered.

The filtered liquid now held in solution the protoxide of iron. Digested with nitric acid, precipitated with ammonia, ignited, dissolved again and freed from magnesia, it yielded oxide of iron = 1·100 = 0·8901 metallic iron. This method of separating the protoxide of iron from the peroxide is not sufficiently accurate, and I mention this part of the process only to show that the iron, even after ignition, remained in the state of protoxide mixed with peroxide. Having tried to ascertain the amount of protoxide and peroxide of iron in these solutions as accurately as possible, by converting the peroxide of iron by means of solution of sulphuretted hydrogen into protoxide, and ascertaining the quantity of separated sulphur &c. in one part of the solution, and mixing another part with liquid chloride of gold and sodium,—from the quantity of the reduced gold the quantity of protoxide of iron was very easily calculated, and I obtained for the greatest part proportions which approached to a rather strange formula, $3\dot{F}+2\ddot{\bar{F}}$. The carbon taken away by the escaping hydrogen is of far *less amount* than is generally asserted; and each atom of carbon volatilized in this way is at the *same time replaced by oxygen* or sometimes hydrogen.

The remaining part of the brown powder, = 3·39, was likewise put into a platinum crucible, heated like the first over a spirit lamp, till it began to glow on the periphery, and then quickly removed from the fire. The first smooth surface of the powder on the bottom of the crucible was now found to be intersected by small cracks, through which might be seen the interior of the powder in ignition for some time; its colour after cooling became darker, but it had neither gained nor lost in weight.

Hydrochloric acid of 1·16 sp. gr. did not visibly attack this powder; but heated in a sand bath, the action of the acid became at once very lively; a great quantity of hydrogen smelling slightly in the well-known way was disengaged, and a velvety black residuum of a rather oily appearance was left.

Its weight was found to be 0·75 grains, and it had therefore lost in the acid 2·64 grains.

During ignition it emitted very dense fumes—had changed its dark black colour into a dirty reddish-white; it had lost only 0·051 grains, notwithstanding the great apparent evolution of gases, and we must therefore conclude, that the black residuum contained silicon, taking in exchange for carbonaceous matter oxygen from the atmosphere.

By means of hydrochloric acid and succinate of ammonia iron was separated with a trace of manganese = 0·32 grains = 0·2219 grains of metallic iron, and silica = 0·332 grains = 0·1595 grains of silicon.

The oily residuum contained therefore, in 0·6989 grains,

Iron	0·2219	or peroxide of iron	0·3200
Silicon	0·1595	or silica	0·3320
	0·3814		0·6520

The hydrochloric solution, from which the white powder just mentioned remained, was colourless as water; after oxidation with nitric acid, oxide of iron was separated equal to 3·12, viz.: iron 2·163408, and oxygen 0·956592; oxide of iron separated from the silica = 0·32 grains. The oxide of iron contained in the liquid amounted therefore, as we see, to 3·12 gr.; on the contrary, what the iron lost by this treatment with acids amounted only to 2·64; the iron therefore could be neither in the state of protoxide nor peroxide in the remainder.

Now the first residuum before ignition weighed 3·13, after ignition 3·55; and contained therefore

Silica	0·265
Silicon	0·025
	0·290
Loss of protoxide of iron	3·260
	3·550 grains,

which is the weight of the residuum as before mentioned.

If we now consider the amount of iron and oxygen in the first separation of protoxide and peroxide of iron, we find

Peroxide:			
Iron	1·7335	Oxygen	0·766500
Protoxide:			
Iron	0·5869	Oxygen	0·173050
Total amount	2·3204	Total amount	0·939550

Silicon	0·127306
Do. mixed with the residuum	0·025000
	0·152306

Oxygen	0·137694
Total amount of oxygen before the 2nd ignition.	1·077244
Oxygen after 2nd ignition	0·028000

Therefore we have

Metallic iron	2·320400
Silicon	0·152306
	2·472706

Oxygen	1·077244
Increase during ignition	0·420000
Difference	0·657244

Powder before ignition weighed as already stated	3·130000
Amount of bases	2·472706
	0·657294

The difference is to be ascribed to the volatilized carbon, hydrogen and azote.

If we compare this difference with the difference between the real increase of the powder during ignition, and the calculated oxygen, we find the

Former difference	0·657294
Last difference	0·657244
	0·000050 grains.

This difference is so extremely small that we may consider the oxygen only calculated as the real quantity taken up, instead of 0·65729 carbon, hydrogen and azote. The increase could therefore only be 0·420000, as the remaining quantity of the absorbed oxygen =0·657244, was *counterbalanced* by the 0·657294 burnt carbon, hydrogen and azote.

The black residuum of the second part of the powder, which as already stated weighed 3·39 grains, was equal to 0·75; the loss therefore in 3·39 grains was = 2·64, consisting of iron and oxide of iron.

If we calculate from the hypothetically-assumed quantity of carbon, hydrogen and azote in the first part of the powder, the same relative quantity for the second part of the powder, we ought then to have for 3·39 grains of residuum 0·71188 grains of carbon, hydrogen and azote.

If we therefore assume the black residuum to contain only silicon, and iron in its oxidized state, we obtain as already mentioned,

Silicon	0·1595
Oxide of iron	0·3200
	0·4795

This difference between the original weight of the remainder = 0·7500, which amounts to 0·2705, we may consider as carbon.

Now we calculated the amount of carbon to be 0·71188, but as we find only 0·2705 grains of carbon, the difference = 0·7118 − 0·2705 = 0·44138 carbon, must have been counterbalanced by an equal quantity of oxygen to produce *oxidum ferroso-ferricum* in the following way:—

Oxygen	0·44138
Iron	1·12268
Oxidum ferroso-ferricum . .	1·26406
Remaining iron	1·040728.
The	2·604788

will be therefore the calculated loss; the actual loss on the contrary was found as before mentioned = 2·64; the difference between experiment and calculation amounts only to 0·035212.

[To be continued.]

LXIV.—*On the Heat of Vapours and on Astronomical Refractions. By* John William Lubbock, *Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*[*]

PREFACE.

THE connexion between the temperature and the pressure (or elasticity) of elastic vapours is a desideratum in Physics. A knowledge of it is indispensable to an exact theory of the Steam Engine, to an exact theory of Astronomical Refractions, and to an accurate solution of other important problems. The want of it has hitherto been supplied by unsatisfactory approximations; but these questions cannot be completely investigated without a more careful attention to the premises than has hitherto been possible, owing to a want of the proper key to these researches, which consists in a knowledge of the mathematical law which connects the temperature and the pressure in elastic fluids, and which is required in addition to the law of Mariotte and Gay Lussac to complete their theory.

If V represent the absolute heat or *caloric*, i the *latent* heat, c the *sensible* heat or that which affects the *thermometer*,

$$V = i + c.$$

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[*] Reprinted, by the obliging permission of the author, from the original memoir lately published. London, 1840. 8vo.

If θ be the *temperature* as indicated by a thermometer, there can be little doubt that V is capable of being expressed in a series proceeding according to positive powers of θ, so that

$$V = a + b\theta + c\theta^2 + \&c.$$

a, b, c, &c., have a certain signification in Taylor's theorem, but without being able to determine their values, *à priori*, or to obtain any relations between them, they may be treated as constants. If the *latent* heat be constant, which is probable, and if the effect indicated by the thermometer is proportional to the sensible heat,

$$c = b\theta, \quad V = a + b\theta.$$

It must, however, be left to experiment to decide how many terms are to be taken into account for any given substance, within any given range of the thermometric scale, and in order to satisfy the results of observation within any given quantity. The other suppositions upon which my theory is founded are those of Laplace, viz. that the quantity called γ by M. Poisson is constant for the same substance at different temperatures, and that the equation

$$V = A + B\frac{p^{\frac{1}{\gamma}}}{\varrho}$$

is the solution of a certain differential equation. See *Méc. Cél.*, vol. v. p. 108. Poisson, *Méc.*, vol. ii. p. 640.

The theorems which are given by M. Poisson in the second volume of the *Mécanique*, and which are also to be found in the works of Pouillet and Navier, rest upon the condition that the absolute heat is constant, while the sensible heat varies. This is the most restricted hypothesis which can be made upon the nature of heat, and it does not satisfy the observations. In this Treatise I have gone a step further, by supposing the absolute heat to vary with the sensible heat, or to be represented by an expression of the form $a + b\theta$, (or what is the same, $V = C + D(1 + \alpha\theta)$. See p. 2.) θ being the temperature reckoned from some fixed point, a and b constants. This includes implicitly the other hypothesis, which if true, in determining a and b by means of observations, the constant b should come out zero. This in the case of steam is certainly not the case, nor is it so in any case which I have examined.

The experiments of Dulong and Arago upon steam at high temperatures, those of Southern and Dalton, and those of Dr. Ure, furnish data by which the supposition I have adopted and the formulæ which flow from it can be scrutinized; and if the expressions which result from it fail to represent those observations, we have at least arrived at this conclusion, that the condition of the invariability of the quantity called γ by M. Poisson does not obtain in nature, or that the absolute heat cannot be represented by so simple a function of the temperature or sensible heat. Recourse must then be had to more complicated expressions. If, on the contrary, my formula represents the observations of the temperature of vapours with accuracy, its origin in a simple theoretical notion of the quantity of absolute heat, and its simplicity, are great additional recommendations in its favour. The formula which I have obtained does, I believe, represent the observations better than any hitherto devised; at low temperatures and pressures it deviates a little, but a very slight error in the observed pressures may account for this discrepancy. Dalton says that it is next to impossible to free any liquid entirely from air; of course if any air enter, it unites its force to that of the vapour. Moreover, when the pressures are small, the variation of temperature becomes great for a small variation of pressure; so that the agreement of theory with observation may be considered as complete, even if the absolute amount of the error of the calculated temperature is then more considerable.

My formula has also been compared with the observations of Dr. Ure, on the vapour of alcohol, æther, petroleum, and oil of turpentine, recorded in the Philosophical Transactions for 1818*.

I think that the comparisons contained in this treatise afford sufficient evidence that my formula is established, and that the deviations of the calculated results from those of observation are within the limits of the errors of the latter; but this point I leave to be decided by those more conversant with the nature of the experiments. It would not militate against my views if it were found necessary to take in an additional term and to make

$$V = C + D(1 + \alpha\theta) + E(1 + \alpha\theta)^2 + \&c.$$

but the expressions for the temperature and density in terms of the pressure would not be quite so simple, although more pliable.

As the same principles must be applicable to the constitution of the atmosphere, I have examined the observations made by M. Gay Lussac in his aëronautic ascent from Paris, and which are published by M. Biot in the *Conn. des Temps.* My calculated temperatures may be considered as identical with the *températures regularisées* of M. Biot, which are given by that distinguished philosopher as representing the condition of the atmosphere divested of the irregularities and errors incidental to observations made under circumstances so difficult and so disadvantageous. But the altitude to which man can ascend is so limited, that observations of the temperature made in aëronautic ascents will never fur-

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[* Dr. Ure's paper containing these observations was reprinted in Phil. Mag., First, Series, vol. liii. p. 38, *et seq.*—Edit.]

nish so complete a test of the accuracy of any formula professing to give the relation between the pressure and the temperature in elastic fluids, as observations of the temperature of the vapour of water and other substances, which can be carried through a greater range of the thermometric scale, and above all through the low pressures where the character of the curve is more decided.

M. Biot has dwelt with reason upon the importance of introducing into the theory of Astronomical Refractions a greater conformity with the conditions of the problem than has hitherto been attempted; and he has also noticed the imperfection in principle of the present mode of calculating heights by observations of the barometer, a method which must of course be abandoned (at least in any accurate exposition of this theory) whenever the discovery of the true connexion between the temperature and the pressure of the higher regions of the atmosphere renders it possible to adopt a more rigorous mode of eliminating the density from the differential equation which connects d p and d z. The correct expression which connects the difference of altitude with the pressures at the upper and lower stations ought to be the foundation of the theory of Refractions. Considering on the one hand the notions upon which my formula is ultimately founded, its identity with the results offered by the observations of steam and other vapours, and moreover the agreement afforded by the direct comparison with the observations of M. Gay Lussac, there can be no doubt that it represents the density of the atmosphere at different altitudes with greater fidelity than any hypothesis which has up to the present time been made the basis of the theory of Astronomical Refractions.

I think that my table of mean refractions represents the observed quantities within the limits of their probable errors, and I have obtained this result without any arbitrary alterations of the constants.

In the higher regions of the atmosphere the cold is intense*, depriving the air of its elasticity and converting it into a liquid or solid substance. My formula of course is only applicable as long as the air continues in the state of an elastic vapour; and if at any altitude it ceases to maintain that condition, the density must be represented by a discontinuous function. But the density of this frozen air must be extremely small, and it probably has little effect upon the amount of Refraction.

I am indebted to Mr. Russell for his kind assistance in the numerical calculations which accompany this treatise.

29, *Eaton Place,* March 2, 1840.

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* See Poisson, *Théorie de la Chaleur*, p. 460.

CONTENTS.

General Expressions.—On the Pressure of Steam.—On the Steam Engine.—On the Vapours of Æther, Alcohol, Petroleum, and Oil of Turpentine.—On the Conditions of the Atmosphere and on the Calculation of Heights by the Barometer.—On Astronomical Refractions.

GENERAL EXPRESSIONS.

Let V be the quantity of absolute heat, considered as a function of the sensible heat or temperature θ,

$$\frac{\mathrm{d} V}{\mathrm{d} \theta} = \frac{\mathrm{d} V \mathrm{d} \varrho}{\mathrm{d} \varrho \, \mathrm{d} \theta} + \frac{\mathrm{d} V}{\mathrm{d} p} \frac{\mathrm{d} p}{\mathrm{d} \theta}$$

$$p = k \varrho \, (1 + \alpha \theta).$$

ϱ being the density, p the pressure, k and α constants,

$$\frac{\mathrm{d} \varrho}{\mathrm{d} \theta} = - \frac{\alpha \varrho}{1 + \alpha \theta}$$

$$\frac{\mathrm{d} p}{\mathrm{d} \theta} = \frac{\alpha p}{1 + \alpha \theta}$$

if

$$- \frac{\mathrm{d} V}{\mathrm{d} \varrho} \frac{\alpha \varrho}{(1 + \alpha \theta)} = \gamma \frac{\mathrm{d} V}{\mathrm{d} p} \frac{\alpha p}{(1 + \alpha \theta)}$$

$$\varrho \frac{\mathrm{d} V}{\mathrm{d} \varrho} + \gamma p \frac{\mathrm{d} V}{\mathrm{d} p} = 0.$$

If γ be considered as a constant quantity the integral of this partial differential equation is

$$\frac{p^{\frac{1}{\gamma}}}{\varrho} = \text{funct}^{\text{n}}.\ V^{*}.$$

The simplest form which can be assigned to this function of V is such that

$$V = A + B \frac{p^{\frac{1}{\gamma}}}{\varrho}$$

A and B being constants.

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[*] So far the reasoning is identical with that contained in the *Mécanique* of M. Poisson; but M. Poisson proceeds further upon the limited supposition of V being constant.

Laplace arrived at this equation (*Méc. Cél.* vol. v. p. 128.).

See Poisson, *Annales de Chimie et de Physique*, tom. xxiii. p. 342*; *Mécanique*, vol. ii. p. 648; Navier, *Leçons données à l'Ecole des Ponts et Chaussées*, tom. ii.

$$V = A + B \frac{p}{\varrho}^{\frac{1}{\gamma}} \qquad V' = A + B \frac{p'}{\varrho'}^{\frac{1}{\gamma}}$$

$$p = k \varrho (1 + \alpha \theta) \qquad p' = k \varrho' (1 + \alpha \theta')$$

k and α being constants.

I will now introduce the additional condition that the heat is *proportional* to the temperature, in which case

$$V = C + D(1 + \alpha \theta)$$
$$V' = C + D(1 + \alpha \theta')$$

C and D being constants. These equations include implicitly the hypothesis attributed to Watt and also that of Southern, respecting the vapour of water: on the former $D = 0$. Hence

$$V = C + D(1 + \alpha \theta) = A + B \frac{p}{\rho}^{\frac{1}{\gamma}}$$

$$V' = C + D(1 + \alpha \theta') = A + B \frac{p'}{\rho'}^{\frac{1}{\gamma}}$$

$$1 + \alpha \theta' = (1 + \alpha \theta) \frac{p'^{\frac{\gamma-1}{\gamma}}}{p^{\frac{\gamma-1}{\gamma}}} \left\{ \frac{1 - \frac{D}{k B} p^{\frac{\gamma-1}{\gamma}}}{1 - \frac{D}{k B} p'^{\frac{\gamma-1}{\gamma}}} \right\}$$

If $\frac{D}{k B} = E$ and if θ correspond to the boiling point, $\theta = 180°$ in Fahrenheit's scale, if the pressure be measured in atmospheres $p = 1$, but generally

$$1 + \alpha \theta' = (1 + \alpha \theta) \frac{(p^{\frac{1-\gamma}{\gamma}} - E)\dagger}{(p'^{\frac{1-\gamma}{\gamma}} - E)} \qquad [1]$$

[* A translation of M. Poisson's memoir here referred to will be found in Phil. Mag., First Series, vol. lxii. p. 328.—Edit.]

† This equation must not be confounded with another equation which may be deduced from it by making $E = 0$, and which is not reconcileable with phænomena, as was long since noticed by M. Poisson in the case of steam. An equation of this kind is given by M. Pouillet in the form

$$x = \cdot 2669 \left(\frac{760}{p}\right)^{1 - \frac{1}{1 \cdot 375}}$$

as $p' = k\,\varrho'\,(1 + \alpha\,\theta')$

$$\frac{\varrho'}{\varrho} = \frac{p'\,(p'^{\frac{1-\gamma}{\gamma}} - E)}{p\,(p^{\frac{1-\gamma}{\gamma}} - E)}, \qquad [2]$$

$$= \left(\frac{p'}{p}\right)^{\frac{1}{\gamma}} \left\{\frac{1 - E\,p'^{\frac{\gamma-1}{\gamma}}}{1 - E\,p^{\frac{\gamma-1}{\gamma}}}\right\}$$

if $$\frac{E\,p^{\frac{\gamma-1}{\gamma}}}{1 - E\,p^{\frac{\gamma-1}{\gamma}}} = -H$$

$$\left(\frac{p'}{p}\right)^{\frac{\gamma-1}{\gamma}} = 1 - q \qquad \frac{1 - E\,p'^{\frac{\gamma-1}{\gamma}}}{1 - E\,p^{\frac{\gamma-1}{\gamma}}} = 1 - H\,q$$

$$\frac{\varrho'}{\varrho} = (1 - q)^{\frac{1}{\gamma-1}}\,(1 - H\,q),$$

if $\log\,(1 - H\,q) = -u \qquad c^{-u} = 1 - H\,q$

c being the number of which the hyperbolic logarithm equals unity

$$\frac{\varrho'}{\varrho} = H^{\frac{1}{1-\gamma}}\;c^{-u}\left\{c^{-u} - 1 + H\right\}^{\frac{1}{\gamma-1}}.$$

if $\frac{\varrho'}{\varrho} = 1 - \omega$

$$\omega = 1 - H^{\frac{1}{1-\gamma}}\,c^{-u}\left\{c^{-u} - 1 + H\right\}^{\frac{1}{\gamma-1}}$$

Since $C + D\,(1 + \alpha\,\theta) = A + B\,\frac{p}{\varrho}^{\frac{1}{\gamma}}$.

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for atmospheric air. *Elémens de Physique*, vol. i. p. 400, and by Navier, *Leçons données à l'Ecole des Ponts et Chaussées*, vol. ii. p. 310, in the form

$$v' = \frac{(1 + \alpha\,v)}{\alpha}\left(\frac{\varpi'}{\varpi}\right)^{\cdot 3748} - \frac{1}{\alpha}.$$

If $\frac{1}{\varrho} = v$

$$\alpha D(\theta' - \theta) = B p^{\frac{1}{\gamma}} \{v' - v\} = V' - V,$$

supposing the heat and the volume to vary, the pressure remaining constant.

According to Dulong the following laws obtain, which however, are not admitted by Dr. Apjohn (see L. & E. Phil. Mag. 1838, vol. xiii. p. 339):

"1°. Des volumes égaux de tous les fluides élastiques pris à une même température et sous une même pression, étant comprimés ou dilatés subitement d'une même fraction de leur volume, dégagent ou absorbent la même quantité absolue de chaleur. 2°. Les variations de température qui en résultent sont en raison inverse de leur chaleur spécifique à volume constant."—*Mém. de l'Institut*, tom. x. p. 188.

According to the first of these laws the quantity B must be the same for different vapours; of the second I am unable to offer any satisfactory interpretation.

In what follows I propose to ascertain how far the equations [1] and [2] satisfy the best observations on record. The general relation gives

$$1 + \alpha\theta'' = (1 + \alpha\theta)\frac{(p^{\frac{1-\gamma}{\gamma}} - E)}{(p''^{\frac{1-\gamma}{\gamma}} - E)}.$$

Eliminating E between this equation and that which connects θ' and p',

$$- \theta)\,(1 + \alpha\theta')(p'^{\frac{1-\gamma}{\gamma}} - p^{\frac{1-\gamma}{\gamma}}) = (\theta' - \theta)\,(1 + \alpha\theta'')(p''^{\frac{1-\gamma}{\gamma}} - p^{\frac{1-\gamma}{\gamma}})$$

If $\frac{1-\gamma}{\gamma} = \beta$

$$\frac{\left(\frac{p''}{p}\right)^{\beta} - 1}{\left(\frac{p'}{p}\right)^{\beta} - 1} = \frac{(\theta'' - \theta)\left(\frac{1}{\alpha} + \theta'\right)}{(\theta' - \theta)\left(\frac{1}{\alpha} + \theta''\right)}.$$

From this equation, knowing θ'', θ', θ, p'', p', p; β may be determined for any gas or vapour. Knowing β, E may be found from the equation

$$E = \frac{p'^{\beta}\left(\frac{1}{\alpha} + \theta'\right) - p^{\beta}\left(\frac{1}{\alpha} + \theta\right)}{\theta' - \theta}.$$

[To be continued.]

LXV. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* *By* M. Dumas.

[Continued from p. 329.]

Chemical Types.

CONSIDERED in itself, the law of substitutions has a practical importance quite sufficient to justify the necessity of distinguishing it from the more general actions of chemistry. But this distinction becomes necessary in quite another way, when to this kind of instinct which led us to view it at first as a law of nature, succeeds the nearly perfect certainty that it is connected with one of the most mysterious and most important phænomena of the science.

I mean the existence of chemical types, capable, without being destroyed, of undergoing the most singular transformations, and of which all the elements might successively disappear, others being substituted for them. I mean those organic types, the admission of which into the domain of organic chemistry seemed to me to be henceforward inevitable, in consequence of the best-characterized experiments which had been suggested by the law of substitutions.

Thus it being possible to convert acetic acid into chloracetic acid by the action of chlorine without its at all losing its capacity for saturation, I have considered the chloracetic acid which results from it as acetic acid in which chlorine had been substituted for hydrogen: these two bodies have appeared to me to belong to one type, to one kind.

Now, when I established, five years ago, the analogy of iodoform, of bromoform, of chloroform, and of anhydrous formic acid, when I added that electro-negative bodies, such as sulphur, phosphorus, and arsenic, might be substituted for iodine, chlorine, and oxygen, no one raised the least difficulty; this series of formulæ entered the science without obstacle.

It is then because the development of the experiment has led us to look upon chlorine and hydrogen as susceptible of taking the place of each other in a compound, that certain convictions revolt. We are then led to examine what we should understand by an *organic type*, and to discuss this point: for example, it being possible for iodine, bromine, chlorine, and oxygen to take each other's place in a compound without destroying its type, can this power (*rôle*) be denied to hydrogen? In other terms, iodoform, bromoform, chloroform, oxiform, being admitted as species of the same genus, can we refuse this character to hydroform, that is to say, to the gas of the acetates, because it contains hydrogen, and no longer an electro-negative body?

Before mentioning my opinion, which is however well known, it is necessary that the three points which make the difficulty, and which are the definition of chemical types, should be well understood, that of the fundamental properties, and the confounding of the function (*rôle*) which hydrogen and chlorine perform among chemical bodies. Prepossessed, for a long while with the necessity of establishing a good natural classification of organic bodies, I have sought for its basis in their chief characters. The discovery of chloracetic acid gave me an opportunity of developing a view of this nature. Acetic acid and chloracetic acid, as distinct bodies, constitute two species, which I have classed in one genus, by reason of the analogy of their fundamental properties and of the identity of their formulæ.

Thus I propose uniting into one genus all the compounds which unite identical formulæ with similar chemical properties. Chloroform, bromoform, iodoform, constitute one genus; olefiant gas and the various chloridated bodies, products which are derived from it, constitute another; acetic acid and chloracetic acid represent a third, &c.

I class then in one genus, or what comes to the same thing, I consider as belonging to the same chemical type, *the bodies which contain the same number of equivalents, united in the same manner and which possess the same fundamental chemical properties.*

The definition of a chemical type carries with it then that of the properties which I call fundamental. Now, by what do we recognize a fundamental property? This is a question easily answered by examples which appear to be conclusive. When we boil chloracetic acid with an alkali, it is at once destroyed and is changed into carbonic acid and chloroform. If we class, as I have done, acetic acid and chloracetic acid in one genus, we are compelled to conclude from this that acetic acid treated with alkalies will change to its turn into carbonic gas, and into a carburetted hydrogen corresponding to chloroform, and into marsh gas (*gas des marais*). This is precisely the result which is given by experiment.

But, say MM. Pelouze and Millon, these approximations are purely fortuitous. If acetic acid heated with barytes changes into carbonic acid and into marsh gas, it is that barytes simply determines the formation of the carbonic acid, that it takes from acetic acid all the carbonic acid which its constitution allows it to supply.

Let us admit this first point for the moment; why should the remainder of the elements of the acetic acid constitute marsh gas rather than anything else? There are four equations, which, at the low temperature at which the action takes place, are alike admissible. The acetic acid always furnishing carbonic gas, it may give besides,

1. Carbon and hydrogen............... $C^4 + H^8$,
2. Methylene and hydrogen...... $C^4 H^4 + H^4$,
3. Olefiant gas and hydrogen $2 C^2 H^2 + H^4$,
4. Marsh gas .... $C^4 H^8$.

Thus, when we only consult the general forces of chemistry, at least four suppositions are presented between which there is nothing to authorize a choice; these are the possible actions. When the consideration of the types is allowed to intervene, it chooses between these four possible actions the necessary action, that which will give rise to four volumes of a hydrocarburetted gas corresponding to chloroform, which it represents in this decomposition.

It is not sufficient then to explain the decomposition of the acetic acid by the alkalies, by saying that those determine the formation of all the carbonic acid which can be produced; we must besides give an account of the production of marsh gas. Now if four equations alike possible are presented, how shall we choose?

We see well that the notion of organic types meets the same difficulty as the law of substitutions. The types are sent back to the general forces of chemistry, as the substitutions were sent back to the equivalents.

The reply is then the same: when we put into play the general forces of chemistry only, the decomposition of acetic acid into carbonic acid and marsh gas is a possible fact. When we set out from the analogy which exists between acetic acid and chloracetic acid, it is a necessary fact. In the first case we should have perfectly understood that some carbon was deposited, that some methylene or olefiant gas was disengaged. In the second, some marsh gas must absolutely be disengaged.

But it is very evident, that the production of carbonic gas, and of marsh gas by the decomposition of acetic acid by means of alkalies is a fact which does not shock the general ideas of chemistry, who explains herself by the play of the general affinities at her disposal. That need not have been demonstrated: a true fact is always possible.

Thus in the view taken by general chemistry, marsh gas might be formed; but viewed by the theory of organic types, it was necessary that that compound should be formed.

Now let us go further: let us suppose that the marsh gas be subjected to the action of chlorine; very different actions might be produced, if we only consult the general forces of chemistry.

Viewed precisely by the theory of organic types, if the marsh gas corresponds to chloroform, to methylic æther, &c., it realizes the carburet of hydrogen, which constitutes the starting point of this series, and by means of chlorine it should give:

$C^4 H^8$

$\left.\begin{matrix} C^4 H^6 \\ Ch^2 \end{matrix}\right\}$ hydrochlorate of methylene.

$\left.\begin{matrix} C^4 H^4 \\ Ch^4 \end{matrix}\right\}$ monochloridated hydrochlorate.

$\left.\begin{matrix} C^4 H^2 \\ Ch^6 \end{matrix}\right\}$ chloroform.

$C^4 Ch^8$ chloride of carbon.

We know, by the experiments which I recently made known to the Academy, that marsh gas obtained from the acetates changes under the influence of chlorine into this chloride of carbon $C^4 Ch^8$, which the theory of types had predicted as the necessary product of the action.

Let me add too, that before giving this chloride of carbon, it also produces some chloroform. But if marsh gas (*gas des marais*) corresponds to the chloroform as the acetic acid does to chloracetic acid, the conversion of marsh gas into chloroform is as necessary a fact as the conversion of the acetic acid into marsh gas.

If when these necessary facts have been recognized as true be experience, it be then proved that they were possible, that they did not disagree with the general laws of chemistry, I contend that the difficulty has not been met. What ought to be done in such a case, is to show how the general theory allows us to foresee that acetic acid should give marsh gas, and that marsh gas should give chloroform.

Far from thinking that I have gone too far in establishing, as I have done, genera for uniting acetic acid and chloracetic acid, marsh gas, and chloroform, I have on the contrary been too cautious.

I therefore persist in my opinion as to the propriety of uniting into one genus those *bodies which contain the same number of equivalents united in the same manner, and which are endowed with the same fundamental chemical properties.*

In this discussion of the characters of chemical types and of the true acceptation of the fundamental properties of bodies, I have said nothing of the identical function attributed to the chlorine and to the hydrogen in acetic acid and in chloracetic acid, in the chloroform and in marsh gas.

Here, however, as it was easy to foresee, is the point which particularly arrested the attention of M. Berzelius, and which he combated by changing all my formulæ and substituting new ones for them.

Down to the present time I have made no reply. Indeed, what could I have added to the following note that M. Liebig authorized me to publish in his name?

" In my interest for the science," says M. Liebig, " I must declare that I do not share the opinions of M. Berzelius, because they rest upon a mass of suppositions which cannot be proved.

" In mineral chemistry the singular observation has been made that chlorine may be substituted for manganese in permanganic acid, without the form of the salts produced by this acid being changed. Nevertheless it is hardly possible to find two bodies between which there exists a greater difference in chemical properties than there is between chlorine and manganese.

" An experiment of this kind is not to be discussed; we must leave to the fact all its value, and say, chlorine and manganese may take each other's place without the nature of the combination being changed by it. From that time I do not see why this manner of acting should be considered as impossible for other bodies, such for example as chlorine and hydrogen.

" The interpretation of these phænomena, such as it has been laid down by M. Dumas, appears to me to give the key to most of the phænomena of organic chemistry.

" Without denying that bodies take each other's places in a great number of combinations, according to their place in the electric order, I think, from the manner of acting of organic combinations, we should draw this conclusion;—that *a reciprocal substitution of simple or compound bodies, acting in the manner of isomorphous bodies, should be considered as a true law of nature.* This substitution may take place between bodies which neither have the same form nor are analogous in composition. It depends exclusively on the chemical force which we call affinity."

These opinions are, in fact, quite conformable to those which I myself published, when I compared the principle of substitutions to the principle of isomorphism, and the bodies of the same chemical type to the isomorphous bodies themselves.

I do not pretend to say, that bodies of the same chemical type should offer the same form; everything leads us to believe that this condition does not always exist, but up to the present time researches are wanting upon this subject.

[To be continued.]

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION.

March 20.—Mr. Schomburgk on the aboriginal inhabitants of Guiana; their manners, customs, and present condition.

March 27.—Dr. George Gregory. Statistics of disease and mortality in the metropolis.

April 3.—Mr. Cowper on the manufacture of cotton.

April 10.—Mr. Nasmyth on the functions of the mouth and the structure of recent and fossil teeth.

April 17.—} No meetings.
April 24.—}

METEOROLOGICAL OBSERVATIONS FOR MARCH, 1840.

Chiswick.—March 1, 2. Cold and dry. 3. Cloudy. 4. Bleak and cold. 5. Frosty: cold and dry: sharp frost at night. 6, 7. Frosty haze: fine. 8, 9. Clear and frosty: fine. 10. Very fine. 11. Drizzly. 12. Cloudy. 13. Hazy: fine. 14. Overcast: very fine. 15. Slight rain. 16. Fine but cold. 17. Clear. 18. Overcast. 19, 20. Cloudy and cold: clear. 21. Very clear. 22. Overcast. 23. Fine but cold. 24, 25. Clear and cold. 26—28. Cloudy and cold. 29, 30. Cloudy and fine. 31. Drizzly.

It may be observed that the quantity of rain in this month was less than 3-10ths of an inch. The barometer stood remarkably high and in general very steady.

Boston.—March 1—3. Fine. 4. Stormy. 5—8. Fine. 9. Cloudy. 10. Fine. 11, 12. Cloudy. 13. Fine. 14. Rain. 15. Cloudy: rain P.M. 16, 17. Cloudy. 18. Rain: rain P.M. 19, 20. Cloudy. 21. Fine: snow early A.M. 22. Cloudy: rain P.M. 23. Cloudy: snow early A.M. 24. Hail: snow early A.M. 25. Fine: snow early A.M. 26. Fine: snow P.M. 27, 28. Cloudy. 29. Cloudy: rain P.M. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—March 1, 2. Fine clear day: frosty. 3. The same: getting cloudy P.M. 4, 5. The same: still freezing. 6. Remarkably fine day: gentle frost. 7—9. The same: hoar frost A.M. 10. The same, but threatening change. 11. The same, but cloudy: no frost. 12. The same: continuing cloudy: no frost. 13. Dry and boisterous: cloudy. 14. Dry but cloudy. 15. Fine day: rain A.M. 16. Slight rain morning: cleared up. 17. Fine: frosty early A.M. 18. Fine: the same. 19. Fine: without frost. 20. Fine: hoar frost. 21. Fine: strong frost. 22. Fine: getting cloudy. 23. Passing showers of snow and hail: frosty. 24, 25. The same: very cold: frosty. 26. Fair but cloudy. 27. Fine but dull. 28. Remarkably fine day. 29. The same after a shower A.M. 30. Wet morning: drizzly all day. 31. Occasional showers.

Sun shone out 29 days. Rain fell 5 days. Snow and hail 1 day. Frost and hoar frost 17 days.

Wind north 1½ day. North-east 8½ days. East 2 days. South 4 days. South-west 3 days. West 2 days. North-west 6 days. North-north-west 1 day. North-north-east 1 day. Variable 2 days.

Calm 15 days. Moderate 9 days. Brisk 5 days. Strong breeze 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. March.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max	Min.		Max.	Min.									
1.	30·374	30·422	30·399	30·12	30·42	30·45	32·7	33·2	30·7	39	29	35	33	26	ENE.	NE.	E.	ENE.	...	...	...	...	28
2.	30·438	30·503	30·459	30·20	30·51	30·52	36·8	37·4	31·2	45	32	37·5	33	27	NE.	E.	E.	E.	...	...	...	...	25
3.	30·430	30·481	30·408	30·12	30·51	30·53	38·8	39·6	35·0	48	30	41	39	28	N.	NE.	calm	NE.	...	...	...	...	32
● 4.	30·386	30·425	30·382	30·10	30·50	30·45	35·8	36·3	32·8	48	24	37	41	30½	NE.	NE.	E.	NE.	...	...	...	...	30
5.	30·408	30·538	30·413	30·10	30·38	30·33	35·4	36·0	32·6	50	19	37	46	28	E.	E.	calm	E.	...	...	...	..	29
6.	30·454	30·570	30·479	30·06	30·36	30·38	36·7	37·2	32·0	53	20	37·5	50	30½	ENE.	E.	calm	S.	...	...	...	0·00	30
7.	30·564	30·664	30·580	30·20	30·45	30·53	37·2	37·8	32·7	52	21	35	50	27½	N.	E.	calm	S.	...	...	...	...	32
8.	30·656	30·726	30·683	30·27	30·57	30·59	37·7	39·0	32·7	54	20	33	49	26½	N.	E.	calm	S.	...	...	...	...	30
9.	30·640	30·676	30·593	30·24	30·59	30·49	37·2	38·0	31·7	56	25	33	58	27½	N.	NE.	calm	SSW.	...	...	...	...	31
☽ 10.	30·392	30·430	30·322	29·95	30·35	30·32	39·0	47·4	35·3	56	41	37	53	29½	NNW.	N.	calm	W.	...	...	...	...	33
11.	30·284	30·369	30·301	29·88	30·32	30·30	42·8	53·7	38·6	46	34	43	49	40½	N.	NE.	E.	WSW.	...	·01	...	..	40
12.	30·274	30·295	30·244	29·82	30·20	30·10	43·8	46·3	40·8	51	29	42	49½	35	NNW.	N.	calm	NNW.	...	...	...	..	38
13.	30·106	30·143	30·050	29·62	29·97	29·95	45·0	46·0	40·7	53	39	43	47½	42	NW.	NW.	calm	W.	...	...	...	...	39
14.	30·050	30·069	30·060	29·59	29·94	29·93	45·8	46·5	42·0	49	35	43	52	39	WNW.	NW.	calm	NW.	...	...	...	0·05	40
15.	29·974	29·994	29·844	29·48	29·70	29·82	43·2	43·8	40·4	48	38	43	52	40	W.	W.	calm	WNW.	...	·11	·09	...	38
16.	30·042	30·283	30·019	29·65	30·10	30·28	42·4	43·0	42·0	42	35	42·5	44½	38	NE.	E.	calm	ENE.	·133	...	·13	...	39
17.	30·282	30·300	30·279	29·90	30·26	30·26	39·8	40·6	37·3	47	39	38	51½	30	N.	NE.	calm	var.	...	...	...	...	29
○ 18.	30·174	30·218	30·143	29·75	30·20	30·20	41·7	45·7	39·2	46	39	43	50	29	NNW.	N.	N.	var.	...	...	·03	...	38
19.	30·198	30·333	30·202	29·83	30·23	30·25	41·3	42·0	40·0	49	29	42	49	38½	N.	NE.	E.	NE.	...	...	·03	...	37
20.	30·318	30·399	30·319	29·87	30·27	30·37	41·3	41·8	36·4	52	29	42	48½	30	WNW.	NW.	calm	N.	...	...	...	...	37
21.	30·412	30·443	30·432	30·	30·43	30·32	37·7	39·3	32·6	46	25	34	44	28	NW.	N.	N.	NW.	...	...	...	0·00	31
22.	30·310	30·346	30·154	29·86	30·16	30·00	39·7	40·6	32·0	50	34	40	46	36	NW.	NW.	calm	NW.	...	·01	...	...	33
23.	29·998	30·202	30·032	29·60	30·12	30·25	39·0	46·4	37·4	50	30	37	43	30	NW.	N.	N.	N&NE.	·022	·03	·04	...	37
24.	30·152	30·235	30·175	29·80	30·27	30·33	36·7	38·2	33·0	43	30	34	38	32	N. var.	N.	N.	N.	·038	·01	...	...	32
25.	30·298	30·384	30·317	30·	30·34	30·33	37·4	41·0	32·0	44	26	36·5	42½	30½	NNW.	NE.	N.	NNW.	...	·01	·31	...	29
☾ 26.	30·324	30·362	30·237	29·93	30·32	30·22	35·3	40·3	30·9	40	31	35	45	30	NW	NE.	calm	NNE.	·022	...	...	...	30
27.	30·124	30·165	30·150	29·84	30·20	30·18	37·4	38·2	34·0	41	34	38·5	46	35	N.	NE.	NE.	NNE.	...	...	...	...	32
28.	30·116	30·155	29·957	29·78	30·05	29·87	38·7	39·5	35·8	42	36	38	50	33½	NW.	N.	calm	ENE.	...	...	...	0·10	32
29.	29·788	29·858	29·827	29·40	29·67	29·66	39·5	42·0	38·2	50	34	42	55	36	W.	W.	calm	S.	...	...	...	..	33
30.	29·894	29·930	29·892	29·36	29·73	29·70	46·7	48·3	38·2	54	43	44	44	42½	S.	S.	calm	SSE.	...	...	·07	...	36
31.	29·840	29·866	29·823	29·43	29·61	29·55	46·8	47·6	45·2	47	42	47	49½	40	NNW.	W.	calm	WSW.	...	·10	...	...	40
Mean.	30·248	30·315	30·231	29·86	30·217	30 208	39·7	41·7	35·9	44·87	31·35	39 0	46·7	32·7					Sum. 2·15	0·28	·70	0·15	Mean. 33·5

THE
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PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JUNE 1840.

LXVI. *On crystallized Native Oxalate of Lime.* *By* H. J. Brooke, *Esq., F.R.S.*[*]

I HAVE had in my possession for some months a specimen of calcite[†] in compound crystals of the metastatic form of Haüy, from $\frac{1}{2}$ to $\frac{2}{3}$ of an inch long, with irregular and curved surfaces occasioned by the intersections of the numerous individuals of which the crystals are severally composed, and which stand on a rather soft light gray matrix. It is uncertain from whence the specimen was brought, but Mr. Heuland supposes it to have come from Hungary.

On the crystals of calcite are deposited a few small crystals, from $\frac{1}{10}$th to $\frac{1}{4}$th of an inch long, of oxalate of lime, which had from their high lustre been supposed to be some ore of lead. It was, however, apparent that they differed in form from all the known lead ores, and on examination by Mr. Sandall at Mr. R. Phillips's laboratory, they were found to be oxalate of lime with one proportional of water.

The crystals appear to have been formed contemporaneously with those of the calcite, in which some of them are partly imbedded, a circumstance which excludes the supposition of their being of vegetable origin; and as only one other oxalate, that of iron, is known to exist in the mineral state, and as it occurs in a bed of wood coal, and the oxalic acid contained in it may be presumed to have been derived from vegetable matter, this oxalate of lime will afford the first instance of the occurrence of oxalic acid as a distinct mineral product.

The primary form of the crystals is an oblique rhombic prism, fig. 1. P on M measuring 103° 14′ and M on M′ 100° 36′: a cleavage parallel to P has induced me to adopt that as one of the primary planes, and there are other cleavages parallel to M, and to *c* of fig. 2. Most of the crystals are twins, and remarkably symmetrical in their form. Fig. 3 is a projection of the terminal planes; and fig. 4, a projection of the twin crystal in profile, the relation of which to fig. 2, and the other figures, is rendered sufficiently obvious by the corresponding letters. All the planes are bright and perfect except M, which is striated by its alternations with *u*; and *f*, which is also striated parallel to the edge between *f* and *f*′.

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[*] Communicated by the Author.

[†] In a list of minerals published a few years since, I proposed this name for the common rhombohedral carbonate of lime; calcite and aragonite thus denoting the two varieties of this substance.

Fig. 1.

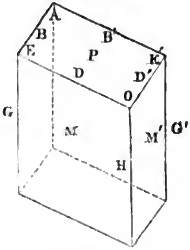
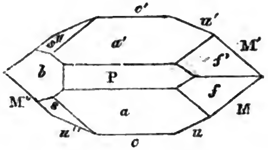
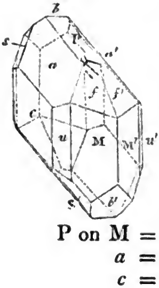


Fig. 3.



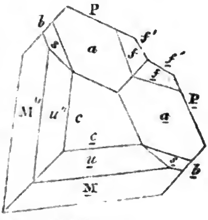
The following are the laws and measurements of the planes, for taking the trouble of calculating which I am indebted to the kindness of my friend Professor Miller of Cambridge.

Fig. 2.



a is B1 D1 G1
b is B1 B′1 H′1
c is B1 D1 G1
f is B1 D∞ G1
s is B2 D1 G2
u is B3 D1 G∞

Fig. 4.



P on M = 103° 14′
a = 127 25
c = 90
b = 109 28
s = 136 48
f = 143 4
a on *s* = 154 19
f = 143 18
c = 142 36
b = 101 41

M on M′ = 100° 36′
u = 160 45
c = 129 42
a = 111 37
b′ = 128 4
s′ = 136 48
f = 142 15

Very brittle. Fracture conchoidal. Hardness rather less than calcite. Spec. grav. 1·833.

Colourless. Transparent to opake. Lustre similar to that of sulphate of lead.

H. J. B.

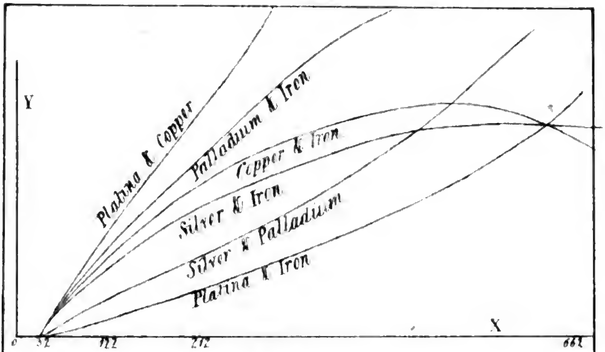


Fig. 2.

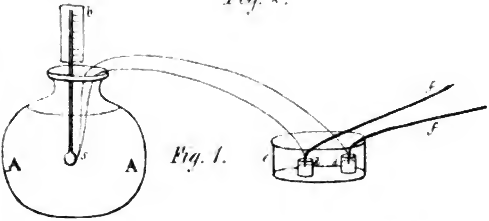


Fig. 1.

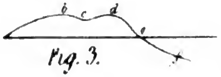


Fig. 3.

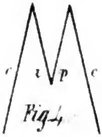


Fig. 4.

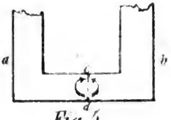


Fig. 5.

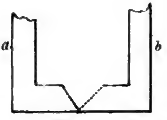


Fig. 6.

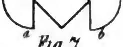


Fig. 7.

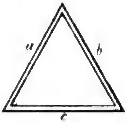


Fig. 8.

J Basire lith

Dr Draper's Experiments in Thermo electricity.

LXVII. *On the Electro-motive Power of Heat.* *By* JOHN W. DRAPER, *M.D., Professor of Chemistry in the University of New York.*[*]

[Illustrated by Plate VI.]

FROM the memoir of M. Melloni, on the Polarization of Heat, inserted in the second part of the first volume of the Scientific Memoirs, we learn, that M. Becquerel, as well as himself, has made experiments to determine the quantities of electricity set in motion by known increments of heat. From these experiments they conclude, that through the whole range of the thermometric scale, those quantities are directly proportional to each other.

But as thermo-electric currents are now employed in a variety of delicate physical investigations, and as there appears to be much misconception as to their character, I propose in this memoir to show,

1st. That equal increments of heat do not set in motion equal quantities of electricity.

2ndly. That the tension undergoes a slight increase with increase of temperature, a phænomenon due to the increased resistance to conduction of metals, when their temperature rises.

3rdly. That the quantity of electricity evolved at any given temperature, is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface.

4thly. That the quantities of electricity evolved in a pile of pairs, are directly proportional to the number of the elements.

First, then, as to the comparative march of electric development, with the rise of temperature, in the case of pairs of different metals.

The experimental arrangement which I have employed, is represented in fig. 1. (Plate VI.) A A is a glass vessel, about three inches in diameter, with a wide neck, through which can be inserted a mercurial thermometer *b*, and one extremity of a pair of electro-motoric wires. The wires I have employed have generally been a foot long, and $\frac{1}{16}$th of an inch in diameter. The extremity *s* of the wires thus introduced into the vessel, ought to be soldered with hard solder: their free extremities dip into the glass cups *d d*, filled with mercury, and immersed in a trough *e* containing water and pounded ice. By means of the copper wires *f f*, $\frac{1}{6}$th of an inch thick, communication is established with the mercury cups of the galvanometer. The coil of this galvanometer is of copper wire $\frac{1}{8}$th of an inch thick, and making twelve turns only, round the needles, which are astatic. The deviations were determined by the torsion of a glass thread, in the way described in the number of this Journal for October 1839.

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[*] Communicated by the Author.

It is surprising to those who have never before seen the experiment, with what promptitude and accuracy a copper and iron wire, soldered thus together, will indicate temperatures.

In the arrangement now described, when an experiment has to be made, the vessel A A is to be filled two-thirds full of water, the bulb of the thermometer being so adjusted as to be in the middle of the vessel, and the soldered extremity *s* of the two wires, being placed in contact* with it, and a small cover with suitable apertures adjusted on the top of the vessel, so that the steam as it is generated may rush up alongside of the tube of the thermometer, and bring the mercurial column in it to an uniform temperature. The communicating wires *f f*, are then placed in the cups, and the trough *e* filled with water and pounded ice, and carefully surrounded with a flannel cloth. The water in the vessel A A, is then gradually raised to the boiling point by means of a spirit-lamp, and kept at that temperature until the galvanometer needles and the thermometer are quite steady. The same plan must be followed, when any other temperature than 212 is under trial, for the thermo-electric wires changing their temperature more rapidly than the mercury in the thermometer, it is absolutely necessary to continue the experiment for some minutes, to bring both to the same state of equilibrium.

When a temperature higher than 212° Fahr., but under a red heat, is required, I substitute in place of the vessel A A, a tubulated retort, the tubulure of which is large enough to allow the passage of the bulb of the thermometer and the wires. A quantity of mercury, sufficient to fill the retort half full, is then introduced, and the tubulure, being closed by appropriate pieces of soapstone, the neck of the retort is inclined upwards, so that the vapour as it rises may condense and drop back again, without incommoding the operator. As in the former case, it is here also necessary to continue each experiment for a few minutes, to bring the thermometer and thermal pair to the same condition. There is not much difficulty in obtaining any required temperature, by raising or lowering the wick of the lamp.

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* If the extremity of the thermo-electric pair be allowed to rest on the bottom of the glass vessel, no accurate results can be obtained; the pair does not then indicate the temperature of the water.

The metals I have tried were in the form of wires. They were in the state found in commerce, and therefore not pure; they were obtained in the shops of Philadelphia.

TABLE I.

Names of the pairs of Metals.	Temperatures (Fahr.)				
	32 F.	122 F.	212 F.	662 F.	
Copper and iron.........	0	93	176	233	Quantities of Electricity.
Silver and palladium...	0	65	147	613	
Iron and palladium	0	112	223	631	
Platina and copper	0	11	26	122	
Iron and silver	0	89	137	244	
Iron and platina.	0	28	56	248	

In this table I have estimated the temperature of boiling mercury at 662° Fahr. The quantities of electricity evolved, as estimated by the torsion of a glass thread, are ranged in columns under their corresponding temperatures. Each series of numbers is the mean of four trials, the differences of which were often imperceptible, and hardly ever amounted to more than one degree.

Now if this table be constructed, the temperatures being ranged along the axis of abscissas, and the quantities of electricity being represented by corresponding ordinates, we shall have results similar to those given in fig. 2, in which it is to be observed, that the curves given by the systems of silver and iron, copper and iron, and palladium and iron, are concave to the axis of abscissas; but those given by platina and copper, silver and palladium, and platina and iron, are convex.

Let us now apply the numbers obtained by these several pairs, for the calculation of temperatures, which will set their action in a more striking point of view. The following table contains such a calculation, on the supposition that for the 90 degrees from 32° Fahr. to 122° Fahr., the increments of electricity are proportional to the temperatures.

TABLE II.

		Temperatures by the Mercurial Thermometer.			
		32 F.	122 F.	Water boils.	Mercury boils.
Temperature by a pair of	Copper and iron......	32	122	202	257
	Silver and palladium	32	122	235	880
	Iron and palladium ..	32	122	211	539
	Platina and copper...	32	122	244	1030
	Iron and silver.........	32	122	170	279
	Iron and platina	32	122	212	829

We are therefore led to the general conclusion, that in *these six different systems of metals, the developments of electricity do not increase proportionally with the temperatures, but in some with greater rapidity, and in others with less.*

The results here given, I have corroborated in a variety of ways and with a variety of wires. A pair consisting of copper and platina, gave for the temperature of tin when in the act of congealing 452° Fahr. instead of 442° Fahr. the point usually taken. For the melting point of lead, it gave $942\frac{1}{2}$° Fahr., instead of 612° Fahr. The melting points of tin, lead, zinc, and occasionally of antimony and bismuth, were in this manner employed, for they allow time for the working of the torsion balance, and with the exception of bismuth, their temperature appears to be steady all the while they are in a granular condition, before they finally solidify. The action of these metals on the thermo-electric pair is easily prevented by dipping it into a cream of pipe-clay.

A pair of copper and platina gave for a dull red heat 1416° Fahr., and for a bright red 2103° Fahr.

A pair of palladium and platina gave for a dull red 1850° Fahr., and for a bright red 2923° Fahr.

Some of the combinations into which iron enters as an element, give rise to remarkable results; thus if we project the curve given by a system of copper and iron, we shall find it resembling fig. 3, where the maximum ordinate *b* occurs at a temperature of about 650° Fahr.; the point *c* appears to be given between 700 and 800 degrees; *d* by a dull red heat; *e* is very nearly the point at which an alloy of equal parts of brass and silver melts, for if the pair be soldered with this substance, it fuses when the needles have returned almost exactly to the zero point. With harder solders or with wires simply twisted, the curve may be traced on the opposite side of the axis towards *f*, its ordinate increasing with regularity. At 60° Fahr., taking the length of the ordinate corresponding to a temperature of 212° Fahr. as unity, the length of the maximum ordinate at *b*, is 1·85 very nearly.

A system of silver and iron gives also a similar curve, the point *b* occurring at a temperature rather higher than the analogous one for the preceding system, but still below the boiling point of mercury.

Now all these things serve to show, that we cannot determine with accuracy unknown temperatures by the aid of thermo-electric currents, on the supposition that the increments of the quantities of electricity are proportional to the increments of temperature throughout the range of the mercurial thermometer.

Let us now proceed to the second proposition, "That the tension undergoes a slight increase with increase of temperature, a phænomenon due to the increased resistance to conduction of metals when their temperature rises."

It will be seen, on consulting the following table, that pairs of *different metals*, at the same temperature, have tensions which are apparently very different.

The currents, the tensions of which are here indicated, were generated by keeping one end of the thermal pair in boiling water, the other ends being maintained at a temperature of 32° Fahr.

Table III.

A pair of	Tension.	A pair of	Tension.
Antimony and bismuth	·137	Platina and iron......	·470
Copper and iron	·183	Copper and platina	·473
Silver and lead	·307	Platina and palladium	·500
Lead and palladium...	·313	Tin and iron	·518
Silver and platina......	·380	Platina and tin	·567

We perceive, therefore, that there apparently exist specific differences in the qualities of electric currents derived from different sources. If, for example, we take a pair of platina and palladium, and expose it to a temperature which shall generate a current capable of deflecting the torsion balance through 1000 degrees, and then abstract it by a wire of such dimensions as to stop one half, or only allow 500 degrees to pass, and repeat the experiment with a current generated by bismuth and antimony, the temperature being still so adjusted as to give a deflection of 1000 degrees, on making this pass through the same intercepting wire, perhaps not much more than one eighth of it will go through the galvanometer.

It might be supposed that these characteristic differences of thermal currents, derived from different sources, were due to some modification of the electricity itself, similar to those of radiant heat, derived from different sources, or at different temperatures, which M. Melloni has attempted to show are analogous to the colours of light, being like them of different degrees of refrangibility, and permeating absorbent media with different degrees of facility. For in the same way that we regard glass as transparent to light, and rock salt as transparent to heat, so too we might regard a copper wire or any conducting medium as transparent to electricity.

But this peculiarity of thermo-electric currents depends on the conducting resistance of the system that generates them. It is possible to give a current a higher or a lower tension, by simply making use of thin or thick wires to generate or to carry it. In the foregoing table the current from platina and palladium had a high tension, because slender wires of those metals happened to be used to generate it; and the current from antimony and bismuth had a low tension, because thick bars of those substances were employed. In the former case, the conducting resistance was greater than in the latter, and hence the tension of the current was higher.

That this is strictly true, will appear on examining the current evolved by any number of systems, under the same condition of resistance to conduction. I took a pair of copper and iron, and soldered it to a similar pair of platina and copper, as is shown in fig. 4, so as to form one continuous metallic line. The point of junction formed by the wires *i* (iron) and *p* (platina), was kept carefully at 63° Fahr., by immersion in a water-bath: the point of junction *p* (platina) and *c* (copper) was treated in like manner; but that of *e* and *i* was raised to 212° Fahr. Under these circumstances, it was found that 181 degrees of electricity were evolved, of which 50 went through a given secondary wire. Then raising the junction *p* and *c* to 212° Fahr., and bringing *e* and *i* to 63° Fahr., there passed at the galvanometer 71 degrees, of which 19 could traverse the same secondary wire, but

As 181 : 50 : : 71 : 19·6

and hence I infer, that where the conducting resistance is the same, the tension of currents from different sources does not differ.

These results inform us how much the tension of a current depends on the resistance to conduction of the system which it traverses, as well as on the dimensions of the system itself; an observation, the value of which we shall presently see.

In a great number of trials which I made, I failed in getting any trustworthy results, as respects tension of currents at high temperatures, on account of the difficulty of maintaining the thermo-electric pair at the same degree without variation. By employing, however, a small black-lead furnace, to which was adapted a covered sand-bath, into which the wires could be plunged, I succeeded at last; for with this arrangement a regulated temperature could be kept up for a length of time.

The experiment was made with care in the case of two systems of metals: 1st, copper and platina; 2nd, copper and iron.

1st. At the boiling point of water, a pair of copper and platina, the unexcited extremity of which was carefully maintained at 67° Fahr., evolved as a mean of four trials, three of which were absolutely identical, 123 degrees of electricity, of which 23 could pass a secondary wire.

Then, by the aid of the furnace and sand-bath, the temperature was raised until the pair evolved 783 degrees, as a mean of four trials; of these 163 could pass the secondary wire. Now,

As 783 : 163 : : 123 : 25½ instead of 23
showing therefore a slight rise of tension.

2nd. The pair of *copper* and *iron* gave at the boiling point of water 300 degrees, of which 57 passed the secondary wire. The temperature was now raised, with the following results:

490 degrees passing the primary, 95 the secondary wire.
553 — — — 113 — —
545 — — — 112 — —
493 — — — 110 — —

It will be understood, that although the quantities of electricity indicated in the first column do not regularly increase, that the temperatures were notwithstanding going regularly upwards: to this peculiarity of the systems into which iron enters I have already alluded. Let us now compare these measures with those obtained for the boiling point of water:

As 490 : 95 : : 300 : 58 instead of 57.
553 : 113 : : 300 : 61 —
545 : 112 : : 300 : 61 —
493 : 110 : : 300 : 67 —

we find, therefore, that in the case of both these systems of metals, the tension slowly rises with increase of temperature, being much better marked in the latter than in the former instance.

The increase of tension here detected, depends unquestionably on increased resistance to conduction, which the wires exhibit as their temperature rises, as the following experiments show.

A pair of copper and iron evolved a current at the boiling point of water, which passing through a wire of copper eight feet long, was determined at the galvanometer to be 176 degrees. Having twisted a part of this wire into a spiral, so as to go over the flame of a spirit-lamp, 8 inches of it were thereby brought to a red heat; the deviation of the needle fell now to 165, being a deficit of 11 degrees. In this experiment, care was taken that no heat should be transmitted along the wire to the connecting cups.

The same was repeated with a piece of iron wire, of the same length and under the same circumstances. The current at first being 90 degrees, as soon as the spiral was made red hot, it fell to 61 degrees, being a deficit therefore of nearly one third the whole amount.

To the increased resistance to conduction, occasioned by an increased temperature, we are to impute the slight rise of tension observed in thermo-electric currents. The quantities are of the same order.

We have next to show, "that the quantity of electricity evolved at any given temperature, is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface."

The quantities of electricity evolved by hydroelectric pairs has been shown to increase with their surfaces, but it is not so in thermo-electric arrangements. A pair of disks of copper and iron, two inches in diameter, were soldered together; they had continuous straps projecting from them, which served to connect them with the galvanometer cups. At the boiling point of water they gave 62 degrees; on being cut down to half an inch in diameter, they still gave 62. On the disk being entirely removed, and the copper made to touch the iron by a mere point, its extremity being roughly sharpened, the deflection was still 62.

By means of a common deflecting multiplier, I obtained the following results; 1st, a copper wire being placed in a bath of mercury, the temperature of which was 240° Fahr., I dipped into it a second copper wire, the temperature of which was about 60° Fahr.; the galvanometer needles moved through 15 degrees.

2nd. The cold wire being sharpened to a point, and plunged deliberately into the mercury to the bottom of the bath, the deflection was 19 degrees.

3rd. But when I touched the surface of the mercury with the *very point* of the cold wire, there was a deflection of 60 degrees.

Having laid a plate of tinned iron upon the surface of some hot mercury, it was touched with the point of the cold wire. There was a strong deflection of the needles in the opposite direction to what would have been the case had the mercury been touched and not the iron. The under surface of the iron was therefore acting as a hot face, and the parts round the point as a cold face, being temporarily chilled by the touch of the wire.

These results explain the anomalies observed by some of those who investigated the course of thermo-electric currents by means of small metallic fragments.

It would therefore seem, that when wires of the same metal are used as electromotors, the quantity of electricity evolved depends on the quantity of caloric that can be communicated in a given time. Time, therefore, under these circumstances must enter as an element of thermo-electric action. In the case of a single metal, the maximum effect would be produced, when the hot element is a mass, and the cold one a point.

And lastly, " That the quantities of electricity evolved in a pile of pairs are directly proportional to the number of the elements."

In the first trials which I made, to determine the effect of increasing the number of pairs in a pile, the results obtained were contradictory; by operating however in the following way, the proposition was at last satisfactorily determined.

1st. The resistance to conduction was made nearly constant by uniting all the pairs intended to be worked with, at once. The current, therefore, whether generated by one, two, three, four, or more pairs, had always to run through the same length of wire, and experienced in all cases an uniform resistance.

2ndly. By making each individual element of considerable length, the liability of transmission from the hot to the cold extremity was diminished.

Having, therefore, taken six pairs of copper and iron wires, $\frac{1}{16}$th of an inch thick and each element 38 inches long, I formed them, by soldering their alternate ends, into a continuous battery. Then I successively immersed in boiling water one, two, three, &c. of the extremities, their length allowing freedom of motion, and the other extremities not differing perceptibly from the temperature of the room.

The following table exhibits one of this series of experiments.

TABLE IV.

No. of pairs.	Calculated Deviations.	Observed Deviations.
1.	55	55
2.	110	111
3.	165	165
4.	220	220
5.	275	272
6.	330	332

Hence there cannot be any doubt, that the quantities of electricity evolved by compound batteries, at the same temperature, are directly proportional to the number of the pairs.

With some general remarks, arising from the foregoing subjects, I shall conclude this communication.

1. It is of importance to remember, that thermo-electric currents traverse metallic masses only on account of differences of temperature existing at different points.

2. When a current of electricity, flowing from the poles of a battery, is made to traverse a metallic sheet, the whole of it does not pass in a straight line from one pole to the other, but diffuses itself through the metal, diverging from one point and converging to the other. The greater part of the current is found, however, to take the shortest route.

3. Combining therefore the foregoing observations (1.2), we perceive, that there are certain forms of construction which will give to thermo-electric arrangements peculiar advantages. For example, the surfaces united by soldering must not be too massive. Let *a*, fig. 5, be a bar of antimony, and *b* a bar of bismuth; let them be soldered together along the line *c d*, and at the point *d* let the temperature be raised; a current is immediately excited; but this does not pass around the bars *a*, *b*, in as much as it finds a shorter and readier channel through the metals, between *c* and *d*, circulating therefore as indicated by the arrows. Nor will the whole current pass round the bars, until the temperature of the soldered surface has become uniform.

An obvious improvement in such a combination is shown in fig. 6, which consists of the former arrangement, cut out along the dotted lines: here the whole current so soon as it exists is forced to pass along the bars. And because the mass of metal has been diminished at the line of junction, such a pair will change its temperature very quickly.

One of the very best forms for a thermo-electric couple is given in fig. 7, where *a* is a semicylindrical bar of antimony, *b* one of bismuth, united together by the opposite corners of a lozenge-shaped piece of copper *c*. From its exposing so much surface, the copper becomes hot and cold with the greatest promptitude, and from its good conducting power it may be made very thin without injury to the current. With a pair of bars $\frac{3}{4}$ths of an inch thick, and a circular copper plate *c*, having both surfaces blackened, I have repeated the greater part of those experiments which M. Melloni made with his multiplier.

4. The currents which circulate in a steel magnet are to

all appearance perpetual. I thought for some time it might be possible to procure similar perpetual currents, by compound thermo-electric arrangements. Fig. 8 will serve to show the character of these combinations, and also the cause of their failure. Let *a*, *b*, *c*, be wires of three different metals soldered together so as to form a triangle. Now if these metals were selected, so that *a* and *b* could form a more powerful thermo-electric pair than *a* and *c*, or *b* and *c*, it might be expected that at all temperatures an incessant current would run round the system. Such, however, will not be found to be the case. In effect, any one of these three serves simply as a connecting solder to the other two, and hence no current is excited; for the ends that have the third metal between them, although that metal intervenes, are under exactly the same condition as the other ends which are in contact.

5. Thermo-electric currents, evolved by pairs of different metals, do not appear to differ specifically. As different gases during combustion burn with differently-coloured flames, and as different sources of caloric evolve rays of heat which are absorbed differently by different media, it might be expected that a pair of wires of copper and platina would give out a current of electricity unlike that of iron and palladium. I have made many trials on this point, adjusting a wire of copper and one of lead to each other, so as to stop equal quantities of electricity flowing from a pair of copper and platina, the galvanometer needles being brought to the same point, whether the long wire of copper or the short wire of lead was employed. But, in the case of every combination which I tried, these two wires acted alike, nor could I ever evolve a current which would pass with more or less absorption along the lead than along the copper.

LXVIII. *Illustrations of the History of the Inductive Sciences. No. I. The Reception of the Copernican Theory in England. By* J. O. Halliwell, *Esq., F.R.S., F.R.A.S., F.S.A., M.R.S.L., &c. of Jesus College, Cambridge.**

THE most zealous reader would not require, nor the most enthusiastic inquirer expect, to find the results of new and deep antiquarian research in so comprehensive a work as that by Professor Whewell on the History of the Inductive Sciences; much less would any one for a moment consider the credit of that eminently distinguished author in any degree shaken by the discovery of documents tending to alter any of his conclusions. All will agree, that with the materials before him, Professor Whewell has performed his task most admirably, for it would have been the labour of a hundred lives to have carefully examined every available document connected with the facts there brought together. Anxious to place my mite towards the discovery of truth in a secure position, I intend, with the permission of the Editors of the Philosophical Magazine, to commence a series of papers on some portions of the history of the inductive sciences which do not appear to have been as yet thoroughly investigated.

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* Communicated by the Author.

As regards the history of science in our own country, Professor Whewell has laboured under very great disadvantages. Thrown almost entirely on continental writers for his information, he has overlooked many important works of the early English authors. For instance, the *Quæstiones Naturales*[*] of Athelard of Bath, and the *Dialogus de philosophia*[†] of Gulielmus de Conchis, ought to have been analysed as grand landmarks in the period preceding the splendid epoch of Roger Bacon; and the contents of the present paper will prove how neglected the claims of the English writers have been in their early reception of one of the greatest advances ever made in natural knowledge.

Prejudice in favour of the authority of Aristotle, even in the sixteenth century the master of all the Universities, forbad any public belief in the Copernican system, and on that account we find many compilers of astronomical tables grounding them upon the new system, without professing, and sometimes denying, a belief in the heliocentric doctrine on which they were founded. Copernicus himself says, "neque enim necesse est eas hypotheses esse veras, imo ne verisimiles quidem; sed sufficit hoc unum, si calculum observationibus congruentem exhibeant:" so dangerous was it to invade the established belief. Astrologers then received more encouragement than those skilled in real science; a rascal of the name of Nicholas Kratzer, being the chosen "astronomyer" of Queen Mary, at the same time that Robert Recorde was her physician. Kratzer was the author of a little volume on astronomy, in which he even denies the rotundity of the earth:

> "He saw with his own eyes the moon was round,
> Was also certain that the earth was square,
> Because he had journey'd fifty miles and found
> No sign that it was circular any where."

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[*] MS. Cotton. Galba, E. iv. Mus. Brit.
[†] MS. Arundel. Mus. Brit. 377.

And his work is not a solitary specimen. In this state of popular ignorance it speaks volumes for the genius of Britain to be able to prove, that at least as early as the year 1556, our country rejoiced in three minds perfectly convinced of the truth of the new system and able to appreciate its value.

To Professor De Morgan we are indebted for having discovered in the Castle of Knowledge (fol. Lond. 1556) by Robert Recorde, a passage showing that the author was in heart a Copernican*; its extreme curiosity will well permit a repetition in modern orthography :—

"*Master.* But as for the stability of the earth, I need not waste any time in proving it, because that opinion is so firmly fixed in most men's heads, that they think it mere madness to bring the question in doubt. And therefore it is as much folly to labour to prove that which no man denieth, as it were with great study to dissuade that thing which no man covets nor any man grants; or to blame that which no man praises nor any man likes.

"*Scholar.* Yet some time it chances, that the opinion most generally received is not most true.

"*Master.* And so do some men judge of this matter; for not only Eraclides Ponticus, a great philosopher, and two great scholars of the school of Pythagoras, Philolaus and Ecphantus, were of the contrary opinion, but also Nicias Syracusius and Aristarchus Samius seem with strong arguments to approve it; but the reasons are too difficult for this first introduction, and therefore I will omit them till another time. And so will I do the reasons that Ptolemy, Theon, and others do allege, to prove the earth to be without motion; and the rather because those reasons do not proceed so demonstrably, but they may be answered fully of him that holdeth the contrary. I mean, concerning circular motion: any direct motion out of the centre of the world seemeth more easy to be confuted, and that by the same reasons which were before alleged for proving the earth to be in the middle and centre of the world.

"*Scholar.* I perceive it well: for as if the earth were always out of the centre of the world, those former absurdities would at all times appear: so that if at any time the earth should move out of its place, those inconveniences would then appear.

"*Master.* That is truly to be gathered: howbeit Copernicus, a man of great learning, of much experience, and of wonderful diligence in observation, has renewed the opinion of Aristarchus Samius, and affirmeth that the earth not only moveth circularly about its own centre, but also may be, yea and is, continually out of the precise centre thirty-eight thousand miles: but because the understanding of that controversy depends upon deeper knowledge than in this introduction may be uttered conveniently, I will let it pass till some other time.

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* Companion to the British Almanac for 1837.

"*Scholar.* Nay, sir, in good faith, I desire not to hear such vain phantasies, so far against common reason, and repugnant to the consent of all the learned multitude of writers, and therefore let it pass for ever and a day longer.

"*Master.* You are too young to be a good judge in so great a matter: it passeth far your learning, and theirs also that are much better learned than you, to disprove his supposition by good arguments, and therefore you were best to condemn nothing that you do not well understand; but another time, as I said, I will so declare his supposition, that you shall not only wonder to hear it, but also peradventure be as earnest then to credit it, as you are now to condemn it."

Who will not regret to learn that such a writer as Robert Recorde died in Newgate? the Newton of the sixteenth century perished in a jail!

In a communication to the Royal Astronomical Society, the Rev. Joseph Hunter was the first who noticed the Ephemeris for 1557, by John Feild, "juxta Copernici et Reinholdi canones," in the preface to which he avows his conviction of the truth of the Copernican theory. Now there is no precise date to Recorde's "Castle of Knowledge," other than the year of its publication; and Feild's preface is dated on the calends of June 1556: there is also prefixed to Feild's work a letter by Dr. John Dee, dated the 3rd of July 1556, in which he also avows his belief in the new system. But neither Feild nor Dee speak of their concurrence with the Copernican theory as anything new, and it is therefore quite impossible to say who of these three properly claims the priority. We may then safely award to JOHN DEE, JOHN FEILD, and ROBERT RECORDE, the high distinction of being the first who adopted the Copernican system, or as Mr. Hunter would call them, the PROTO-COPERNICANS OF ENGLAND.

We do not find, however, many other early supporters of this theory in England. The elder Digges certainly was not, although after his death his son professed his belief in it*. The edition of the "Prognostication Everlasting," published in 1578, contains an appendix by Thomas Digges, entitled "A perfite description of the Celestiall Orbes according to the most auncient doctrine of the Pythagoreans, lately revived by Copernicus, and by Geometricall Demonstrations approved." This appendix opens with the following powerful and well-written passage, which we cannot resist giving at full length:—

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* See three papers by me in the Magazine of Popular Science, vol. iii. and iv.

"Having of late (gentle reader) corrected and reformed sondry faultes that by negligence in printing have crept into my father's Generall Prognostication: Amonge other thinges I founde a description or modill of the world and situation of spheres cœlestiall and elementare according to the doctrine of Ptolome, whereunto all Universities (ledde therto chiefly by the auctority of Aristotle) sithens have consented. But in this our age one rare witte (seeing the continuall errors that from time to time more and more have bin discovered, besides the infinite absurdities in their theorickes, which they have bin forced to admit that woulde not confesse any mobilitie in the ball of the earth) hath by long studie, painfull practise, and rare invention delivered a new theorick or model of the world, shewing that the Earth resteth not in the centre of the whole world, but onely in the center of this our mortall world or globe of Elementes which environed and enclosed in the moones orbe, and together with the whole globe of mortalitie is caried yearely rounde aboute the Sunne, which like a king in the middest of all raigneth and geeveth lawes of motion to the rest, sphærically dispearsing his glorious beames of light through al this sacred cœlestiall temple. And the earth itselfe to be one of the planets having his peculiar and strayinge courses tourning everye 24 houres rounde upon his owne center whereby the sunne and great globe of fixed starres seeme to sway about and tourne, albeit indeede they remaine fixed. So many wayes is the sense of mortall men abused, but reason and deepe discourse of witte having opened these things to Copernicus, and the same being with demonstrations mathematicall most apparently by him to the world delivered, I thought it convenient together with the olde theorick also to publish this, to the ende such noble English minds (as delight to reache above the baser sort of men) might not be altogether defrauded of so noble a part of philosophy. And to the ende it might manifestly appeare that Copernicus mente not as some have fondly excused him to deliver these grounds of the earthes mobility onely as mathematicall principles, fayned and not as philosophicall truly averred, I have also from him delivered both the philosophicall reasons by

Aristotle and others produced to maintaine the earthes stability, and also their solutions and insufficiency, wherein I cannot a little commende the modestie of that grave philosopher Aristotle, who seeing (no doubt) the insufficiency of his owne reasons in seeking to confute the earthes motion, useth these words:—*De his explicatum est ea qua potuimus facultate*. Howbeit his disciples have not with like sobriety maintayned the same. Thus much for my owne parte in this case I will onely say there is no doubte but of a true grounde truer effects may be produced than of principles that are false, and of true principles falshod or absurditie cannot be inferred. If therefore the earth be situate immoveable in the center of the worlde, why finde we not theorickes uppon that grounde to produce effects as true and certain as these of Copernicus? Why cast we not away those *Circulos Æquantes*, and motions irregulare, seinge our owne philosopher Aristotle himselfe the light of our Universities hath taught us, *Simplicis corporis simplicem oportet esse motum?* But if contrary it be founde impossible (the earthes stability being graunted) but that we must necessarily fall into these absurdities, and cannot by any meane avoyde them, why shall we so much dote in the apparance of our sences, which many wayes may be abused, and not suffer ourselves to be directed by the rule of Reason, which the great God hath given us as a lampe to lighten the darcknes of our understandinge and the perfit guide to leade us to the golden braunche of veritie amidde the forrest of errours?"

Robert Tanner, in his "Mirrour for Mathematiques," published at London in the year 1587, makes no allusion to the Copernican theory: at fol. 37, he computes the number of miles that the *sun* has traversed since the creation at 43,688,316,000! Dee, in his later works, does not allude to his former opinion, and the next notice I find is in 1596, when John Blagrave of Reading again revived the theory in its full sway, in his *Astrolabium Uranicum Generale*.

Professor Whewell does not mention one English author of the *sixteenth* century as having adopted the heliocentric theory. He evidently fixes the year 1600 as the earliest period which received a glimpse of the new system, and gives Giordano Bremo, a twaddling Italian writer, the merit of having had "a considerable share in introducing the new opinions into England." But however little effect the writings of Recorde, Dee, or Feilde may have had, yet the extreme popularity of Digges's work places the conjecture out of the limits of probability. Moreover, there were doubtless several

Copernicans who never expressed their opinion in print, and it must be noted that Harriot had been styled a "profounde mathematician" as early as 1593*.

In Caxton's "Ymage or Myrour of the Worlde," printed in 1482, the world and its inhabitants are compared to a large apple surrounded with flies. I mention this, not that it has any relation to the subject in hand, but because I met with it while pursuing these researches, and because it ought always to be quoted in connexion with the most improbable and absurd tale of Sir Isaac Newton and the gravitation apple, which is gravely inserted in work after work. What a pity it is that no such tale has been invented to commemorate the discovery of Copernicus! for notwithstanding the prophecy of Rheinhold, his reputation requires it more than that of Newton:—"Tota posteritas grato animo Copernici nomen celebravit, cujus labore et studio, doctrina ipsa cœlestium motuum propemodum collapsa iterum restituta est: et magna ejus quoque lux, Dei beneficio accensa, inventis et patefactis ab eo multis, quæ ad hanc usque ætatem vel ignota vel obscura."

35, Alfred Place, London, March 7, 1840.

LXIX. *On certain Modifications of the Powers of Heat and Light when transmitted through Glass.* By Charles T. Coathupe, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SHOULD the following observations "On certain modifications of the powers of heat and light when transmitted through glass" appear to you to be a pertinent sequel to some of the experiments of Professor Draper of New York, which are recorded in the London and Edinb. Phil. Magazine for February 1840, they are presented to you with the author's best respects.

In a room about 16 feet square, having a window about 6 feet square in its eastern side, a small stove in the centre of its southern side, a door communicating with a passage at the northern end of the western side, and a blank wall constituting the north side, the following experiments were made.

(*a*.) A small 4-ounce stoppered bottle containing a fragment of camphor was placed upon a table near the centre of the window that occupied the middle portion of the eastern side of the room.

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* Pierce's Supererogation, by Gabriel Harvey, p. 190.

(*b.*) A similar bottle containing camphor was placed exactly opposite to the first bottle, upon a stand near the west wall (the stove being thus at equal distances from each bottle, and not in a line between them).

(*c.*) A third bottle containing camphor was placed in the centre of a line drawn between the door and the window, and exactly opposite to the stove, although at a distance of about 13 feet from it.

The temperature of the room varied at its extremes from 44° Fahr. to 54°.

After three days the camphor in the bottle in experiment (*a.*) had crystallized beautifully towards the east, ∴ towards the light.

That in bottle, experiment (*b.*) towards the west, ∴ opposite to the light.

That in bottle, experiment (*c.*) towards the east and west, ∴ towards the coolest places.

There was certainly rather more crystallization towards the east than towards the opposite side. A little also was apparent towards the north wall, opposite to the stove.

(*d.*) A clean bottle containing a fragment of camphor was now placed near the window, which was covered by two glass receivers, leaving air spaces between the bottle and the first and second receiver of about $\frac{3}{4}$ths of an inch.

The camphor showed no such particular tendency to crystallize towards the east as it had in experiment (*a.*), but it crystallized nearly uniformly all over the upper concavity of the bottle.

(*e.*) Decanters containing water were next substituted for the camphor bottles, and in each position the dew was invariably deposited upon the same relative side of the decanter, as the camphor had crystallized when exposed in the stoppered bottles.

In one of our glass houses (for I am unfortunately connected with the manufacture of glass) there is a small apartment about 9 feet long, 6 feet wide, and 7½ feet high, separated from the interior of the glass-house by a lath and plaster partition, which is hollow within, and comprehends a total thickness of 4½ inches. This little apartment is occupied by a superintendent of the founder's crew. Opposite to a wide bench upon which this person reclines, there is a window through which he watches the progress of the furnace. This window is so constructed that one half of it can be slid behind the other half, to enable the "surveillant" to protrude his head at a moment's notice, to issue his orders. The window is glazed with glass of $\frac{1}{16}$th of an inch in thickness. It is 24 feet from the nearest point of the furnace which emits flame, and 38 feet from the most distant flame that can emanate from the same side of the furnace.

(*f.*) A thermometer suspended from a pin fixed into one of the window bars during the process of founding, and hanging at a distance of one inch from the glass, exterior to the apartment, usually indicates a temperature of from 180° to 190° Fahrenheit, when a thermometer suspended at precisely the same distance from the same pane of glass in the interior of the apartment indicates only a temperature of from 120° to 130° of Fahrenheit. A difference of distance of only two inches, when influenced by the interposition of a plate of glass only $\frac{1}{16}$th of an inch thick, effects a difference of 60 degrees of temperature under the circumstances stated above.

(*g.*) Having suspended a thermometer in a flint glass bottle of 5 inches in diameter, and placed the stopper loosely *in situ*, I placed the bottle upon a non-conductor of heat (several layers of felt) just exterior to this window. The thermometer gradually rose, as might be expected, to 178 degrees of Fahrenheit. It was then placed upon the window frame, by sliding to one side the moveable part of the window until there was just sufficient space to admit the diameter of the bottle; another thermometer was suspended round the neck of the bottle, and adjusted so as to hang close to that surface of the bottle which presented to the interior of the apartment. The internal thermometer remained constant at 178 degrees, and the external thermometer, which faced the interior of the apartment, never indicated a higher temperature than 110 degrees of Fahrenheit.

(*h.*) A little moisture adhered to the interior of the bottle when thus introduced, and the dew became deposited upon that side which was most remote from the fire.

(*i.*) A bottle containing a piece of camphor was now substituted for that which contained the thermometer. In four minutes the side of the bottle which was most distant from the fire became beautifully spangled with crystals of camphor.

(*k.*) Cress seed which had been sown in a wooden trough supported beneath this window in the interior of the apartment, vegetated rapidly; but although it remained constantly exposed to a very brilliant fire light, its appearance was precisely similar to that which vegetated in the dark (viz. exhibiting white and attenuated stems, with pale yellow leaves). There was no tendency in the plants to grow towards the fire light.

I believe that whensoever dew is perceived upon one side only of a vessel containing water, the side upon which the dew is deposited is *the coldest side*, and that *light* has no influence whatsoever upon such a deposition. My experiments with camphor tend to a similar inference with respect to the deposition of crystals upon the sides of vessels containing this substance. How that surface which is presented towards the sun (as in Professor Draper's experiments) becomes the coldest, is a paradoxical problem yet to elucidate.

(*l*) If paper which has been prepared for heliographic purposes by a solution of nitrate of silver be partially covered by a piece of crown window glass, and then exposed to the direct rays of the sun, that part of the paper which has been thus covered will be rendered darker after a few minutes' exposure, than that which has been equally exposed, but uncovered.

The experiments which I made during the last summer upon the effects of *solar light* upon paper prepared with nitrate of silver, indicated that the maximum depth of tint ensued from its transmission through ordinary unstained crown window glass; that the next tint in intensity was produced by the direct solar rays upon the uncovered, and perfectly exposed surface of the paper. The third in intensity, and almost equal to the second, was produced by the transmission of the sun's rays through glass of a "Waterloo blue" colour.

The fourth, through dark violet coloured glass.
The fifth, through purple do.
The sixth, through amber do.
The seventh, through brown yellow do.
The eighth, through dark green do.
The ninth, through light olive green do.
The tenth, through blood red do.
The eleventh, through crimson do.
The twelfth, through bright red do.

From the ninth to the twelfth there was no very remarkable effect produced by the solar rays after an hour and a half's exposure.

The crown glass, the blue, the violet, and the purple glasses produced deep tints, with which none produced from the other coloured glasses could be compared but as contrasts. The colours here mentioned were the only colours tried.

There are one or two other peculiarities about crown glass which deserve notice.

1st. Why should the colour of unannealed window glass be much lighter and brighter than that of similar glass when annealed?

2ndly. The specific gravity of a specimen of very pure doubly-terminated quartz at 64° Fahrenheit was 2·6577.

The spec. grav. of Isle of Wight sand at 64° Fahrenheit was 2·644. The spec. grav. of crown window glass (as made at Nailsea,) is 2·532. Its ingredients are sand, soda, and lime.

Now, if we leave the silex a constant quantity, how is it that any increase of lime, or of soda, increases the specific gravity of the resulting glass?

Any addition of alumina will produce the same effect.

In mentioning an increase of lime, or of soda, or of alumina, I mean of course, an increase beyond the usual proportions of each ingredient commonly employed.

I have the honour to remain,
Gentlemen, yours, &c.
CHARLES THORNTON COATHUPE.

Wraxall, near Bristol,
March 4, 1840.

LXX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c. By* THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*

[Continued from p. 404, and concluded.]

POSTSCRIPT.

SINCE the preceding pages were committed to the press, a paper by Mr. Griffith, entitled "On the True Order and Succession of the Older Stratified Rocks in the neighbourhood of Killarney and to the north of Dublin[*]," has reached my hands. In reference to the vicinage of Killarney, I feel it incumbent on me to offer a few remarks.

I think it unfortunate that Mr. Griffith should persevere in placing in the same parallel, and designating by the same name, two series of strata which by his own showing are clearly in a different order and in a different position; thus pursuing the same course in Kerry as in Waterford, alike productive of obscurity and confusion, by a misapplication of the term "old red sandstone." Thus this formation, which is so well characterized in the Slieve Meesh range (Cahirconrée of Mr. Griffith), by its peculiar beds, horizontal disposition, and in overlying *unconformably* the older stratified rocks situated on the west (which latter generally approach the vertical position), is placed in parallel with those beds of conglomerate, sandstone, quartz-rock (greywacke), and clay-slate, occasionally coloured of a reddish hue, which form incidentally intercalated *conformable* portions of the consecutive transition series, and to all, or some, of such beds as the case may be, when thus coloured, affixing also the term "old red sandstone;" e. g., in Macgillicuddy's Reeks, in Dunloe Gap, in Purple, Tomies, and Glenaa mountains, in Brickeen island and Muckruss peninsula, and in the valley of Kenmare. Yet Mr. Griffith himself, in combating the positions of Mr. Charles W. Hamilton, insists that the red sandstone, where occurring in these localities, has been deposited *conformably* on the older slate (which latter he seems disposed to refer to the Cambrian or Silurian æras), and in a descending order graduates imperceptibly into that rock*. This being admitted, why not designate this a *transition* red sandstone (as I have always done) in contradistinction to the *old red sandstone* of the carboniferous epoch, found in *unconformable* position, and commonly distinguished, in some parts of its extent at least, by beds of red clay, red marly clay, and red slaty clay; e. g., in the Slieve Meesh range and in Kerry Head? Much ambiguity would be avoided by observing this difference in language. Again, after such an admission, it may be asked, why are these rocks, together with the limestone of Muckruss, of the islands of the Lower Lake of Killarney, and extending toward that town, blended by Mr. Griffith with the carboniferous series? This limestone within its own area not only alternates with certain beds of the older stratified rocks, the prevailing dip being to the south, but in their lateral extension it is enveloped by and interlocked with them, which latter relation is clearly proved by the position cf the greywacke, sandstone, and slaty-rocks in the north-western portion of Muckruss peninsula, intervening between the limestone of the islands on the north and that of the south-eastern portion of the peninsula. Mr. Griffith conceives he has solved the question by the introduction of a *fault* ranging from Dunloe Gap through Purple, Tomies, Glenaa, and the southern part of Brickeen Island, and *under Turk Lake* to the main land east, where it is confessedly not visible, diluvial matter occupying the surface. This supposed fault does not appear to rest on any proof of actual disruption by an *up* or *down throw*, or by a *lateral movement*, but to be merely an inference drawn from the difference of strike observable between the strata that have been interrupted in their eastern and western continuation by the excavation of Dunloe Gap or by the formation of Turk Lake or the Lower Lake. I have seen no such dislocation in the masses extending from Dunloe Gap through Purple, Tomies and Glenaa mountains, to Brickeen island (beyond which to the east everything must be imaginary), as is indicated in Mr. Griffith's plan and sections; nor does he clearly aver that he has seen such himself, his idea of dislocation appearing mainly to rest on a seeming unconformity arising from some differences of strike*. The whole question, I think, is simply solved by two considerations: 1st, by the inosculation of the several strata, whether schistose, conglomerated, or calcareous, in the line of their strike, while all dip conformably south; 2nd, that where the line of strike has been irregularly broken through by superficial excavations on the existing dry land, as in Dunloe Gap, or by the intervention of spaces occupied by water, as in the Lower Lake of Killarney, and in Turk Lake, thus separating and removing from observation the direct continuity of strata, if we find some difference of strike in the opposite dismembered masses, it is not necessary to imagine a fault in the case, and to represent the distant strata as forming, when protracted, abutting angles upon such a line of fault, since a simple flexure of the strata upon the line of strike explains the whole matter at once. And this is quite in accordance with the general observation I have made on the older stratified rocks of the south of Ireland, as possessing, when viewed on the large scale, an east and west strike, yet subject to inflections from that line, which are locally of greater or less extent†. The opposite shores of Turk Lake, and those of the Lower Lake of Killarney also, being thus brought into connexion by a simple curvature of the strata, remove all difficulty, bearing in mind that the constituent strata are not persistently continuous, but interlock with each other; and the same may be affirmed with respect to the relations in Dunloe Gap.

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[*] Lond. and Edinb. Phil. Mag. for March 1840, with a plan and two sections.

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* L. & E. Phil. Mag. March, p. 162, 166.

But if the supposed fault were even real and not imaginary, how would it prove that all the strata north of it belong to one æra, and those to the south of it to another, while analogous strata in the valley of Kenmare are in uninterrupted connexion with each other, being there also included in and interlocked one with the other, and not persistently continuous

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[*] Lond. and Edinb. Phil. Mag. for March 1840, pp. 163, 166, 171.

[†] Geol. Trans., vol. v., second series, Memoir on the South of Ireland, § 7.—It is as if we were to draw a straight line to denote the general east and west strike, and then upon this straight line to trace an undulated one, the successive curvatures of which rising above and falling below the straight line, would express the local strikes. In such a view it is obvious, that if the inflected line be divided into parts, and certain intervening portions be removed, that the remaining separate parts may appear to have a different strike relatively to each other, although in fact constituting portions of the same series, as, for example, in the opposite shores of Turk Lake.

beyond a certain extent? The key presented by Mr. Griffith does not appear to me to answer the purpose, nor can I perceive the anomaly which he conceives to arise from the southerly dip being persistent*; since among transition strata some are presented to us merely as intercalated bands with a corresponding dip, while others of a similar character are deposited in troughs, e. g., in Nassau and Belgium contrasted with the Eifel, as noticed in a preceding part of this paper.

The whole subject, so far from being of a mysterious character †, strikes me as sufficiently clear, and which I think may be made to appear by merely taking Mr. Griffith's own statements for our guide. Let us, in the first instance, follow him from the entrance of Dunloe Gap on the north and ascend to the summit of the Reeks on the south; and in the second, consider the view taken by him of a proposed section drawn across Dingle peninsula from Brandon bay on the north, to Feilaturrive on the south; in both cases employing his own language, more or less condensed, yet placing the series under numbers for the sake of greater distinctness.

From Dunloe Gap upward, the succession is thus given ‡:

1. Reddish-grey quartzose rock; coarse-grained reddish-grey conglomerate; coarse-grained brownish-red slate (quarried for roofing slate); red quartzose sandstone alternating with coarse slate, the sandstone presenting occasionally a conglomeritic character §.

2. Chloritic quartz-rock, alternating occasionally with thin beds of green and purple clayslate; grey quartzose beds, alternating with thin beds of purplish clayslate.

3. Reddish-grey quartzose beds, alternating with thin beds of purplish clayslate.

4. Higher up, in ascending to the summits of the Coumeen Peest or eastern ridge of the Reeks, the strata become more red, and pass into a brick or cherry-red quartz-rock with some beds of conglomerate, identical in colour, composition, and structure with the red sandstone situated to the north of

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* Lond. and Edinb. Phil. Mag. for March 1840, p. 163.

† *Ibid.*, p. 166. ‡ *Ibid.* pp. 163 to 165.

§ Compare this with the description which I have given of the entrance to Dunloe Gap, in my Memoir on the South of Ireland, in § 10, in which I have shown that these strata vary in their dip between the vertical and the horizontal, subject to undulations from north to south, yet with a general dip to the south. It is here Mr. Griffith introduces his supposed fault, but which, as before stated, I conceive to be merely an apparent deviation in the line of strike proceeding from an interrupted curvature of the strata.

the (supposed) fault in the Gap of Dunloe, but not so coarse-grained*.

The conglomerate on the top of the Reeks is perfectly *conformable* with the underlying strata, and, in fact, a regular gradation may be traced from the lower or chloritic portion of the series through the reddish-grey into the brick-red quartz-rock and conglomerate.

Proceeding now to the Dingle peninsula, the succession given also from north to south, namely, from Brandon bay to Feilaturrive, is as follows †:

1. Dark blackish-gray clayslate, the upper beds of which alternate with reddish-purple slate, some of which contain Silurian fossils.

2. Red slaty conglomerates, alternating with red and green slate and brown quartz-rock.

3. Chloritic quartz-rocks, with alternating purplish and reddish-grey clayslate, similar in composition and character to those of the Gap of Dunloe, and of that district generally.

Now, on the preceding I have to observe, that, as the prevailing dip of the strata is to the south, these rocks of the Dingle peninsula may be considered as lying deeper in the series than those which range in a more southerly parallel, namely, as extending from Dingle bay to the lakes of Killarney; yet the whole, on both sides of the bay, may assuredly be considered, in reference to their general composition, structure, and characters, as closely related to each other, and as forming one sequence; and taken in this point of view, there appears no difficulty in assigning the red conglomerate, red sandstone, and red slate at the entrance to and in Dunloe Gap to their proper position in the series. Mr. Griffith admits that in the higher parts of the Reeks (No. 4, as given above) the same red conglomerate is found as occurs below in the Gap of Dunloe (No. 1), the latter being succeeded by the chloritic quartz-rock (No. 2), which alternates with thin beds of green and purple clayslate. Again in the Dingle peninsula we find also red slaty conglomerates (No. 2 of that series), succeeded by chloritic quartz-rocks (No. 3), which alternate with purplish and reddish clayslate. Surely the analogy here is very close, bespeaking alternations of similar rocks in a line from north to south. In the Dingle peninsula the red conglomerates (No. 2) repose upon the blackish-grey clayslate (No. 1), with beds of reddish-purple slate, and some

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* Lond. and Edinb. Phil. Mag. for March 1840, p. 165. But the conglomerate visible in the western ranges of the Reeks, in Lisbug mountain, is fully as coarse-grained as that of the Gap of Dunloe.

† *Ibid.*, p. 167.

Silurian fossils. The immediate substrata of the Dunloe red conglomerate, sandstone, and red slate are not visible, being *unconformably* overlaid on the north by the carboniferous limestone in the western quarter, and by the coal formation in the eastern; while, on the other hand, the transition series of the Dingle peninsula, just described, are *unconformably* overlaid on the east by the old red sandstone of the Slieve Meesh range (Cahirconrée of Mr. Griffith); the difference in the æras of production between the transition series and the carboniferous series being thus in both cases clearly marked*.

To the south of the strata of which we have been speaking, there is a band of blackish-grey clayslate, which may deserve Mr. Griffith's attention. It is traversed by the new line of road from Killarney to Kenmare, as it passes up by the line of the Upper Lake, and if duly examined might perhaps be found productive of fossils. This instance may suffice to show that there is no want of repetition of similar beds in a line traversing the series from north to south. In the general succession occur also well-defined greywacke and greywacke slate, terms which being out of favour with some geologists, Mr. Griffith appears to avoid using, although highly distinctive and useful when employed in a legitimate sense.

Upon the whole, I see no reason to depart from the opinion which I have formerly given, namely, that the limestone of the region of Muckruss extending to Killarney, is a local deposit enveloped and intercalated in the general transition series. And the same view, as to their forming portions of the consecutive series, applies to the other limestone bands in the south of Ireland, whether inclosed in and interstratified with the adjacent rocks, or merely superimposed and interstratified in the form of a trough with the subjacent series. In the valley of Kenmare this latter position appears to be established by the detailed researches of Mr. Griffith, which, I confess, escaped my observation.

That among the strata in immediate association with the bands of limestone in the south of Ireland, some should be found, whether bearing the character of sandstone or clayslate, containing certain vegetable remains, cannot be held sufficient to invalidate the general view which I have taken; 1st, because such remains are not wholly foreign to a transition country; and 2nd, because it has been shown that the older stratified rocks of the south of Ireland form one consecutive series.

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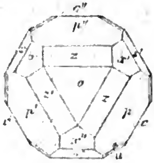
* See my Geological Map of the South of Ireland in Geol. Trans., vol. v., second series.

LXXI. *On the Form of* Eudialyte. *By Professor* Miller*.

THE angle between normals to two adjacent faces *p*, of one of the rhombohedrons of eudialyte, is stated to be 106° 20′ in some mineralogical treatises, and 106° 36′ in others. A very accurate determination of its form is difficult on account of the unevenness and dullness of its faces. In order if possible to render it a little less uncertain, I measured some very perfect crystals, for which I am indebted to Dr. A. Smith of Dublin, and also some of the best crystals in Mr. Brooke's collection. The resulting values of the angle between normals to the faces *o* and *p*, either given immediately by observation or computed from the angles between other pairs of faces, after excluding all the observations that were unsatisfactory on account of the largeness of their difference from the mean, or the indistinctness of the reflected images, are 67° 42′—40′—44′—48′—41′—39′—45′—41′—35′—43′—41′—47′—41′—36′—40′—47′—42′—47′. The angles between normals to the faces calculated from the mean of the preceding values of *o p*, are

o u	90°	0′	*c u*	30°	0′
o c	90	0	*z z′*	53	35
o z	31	22	*x x′*	84	4
o x	50	38	*p p′*	106	30
o p	67	42	*s s′*	116	4
o s	78	25	*u t*	13	59
o t	81	11	*p t*	22	46



The symbols of the forms to which the different faces belong are, in the notation which I have adopted in my treatise on crystallography,

$$o\ \{111\},\quad c\ \{2\bar{1}\bar{1}\},\quad u\ \{10\bar{1}\},\quad p\ \{100\},\quad x\ \{110\},$$

$$z\ \{211\},\quad s\ \{11\bar{1}\},\quad t\ \{201\}.$$

The faces *t*, which occur on two of the crystals in the possession of Mr. Brooke, do not appear to have been observed before.

St. John's College, Cambridge,
May 6, 1840.

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* Communicated by the Author.

LXXII. *On some Phænomena of the Voltaic disruptive Discharge. By* W. R. Grove, *Esq., M.A., M.R.S.*

My dear Sir,

I SHALL be much obliged by your insertion of the inclosed in the Philosophical Magazine at your earliest convenience.
Yours very sincerely,

To Richard Phillips, Esq., F.R.S. W. R. Grove.

In the number of the *Bibliothèque Universelle de Genève* for March, is published an extract from a letter of mine to Professor Schœnbein, giving an account of some experiments on the voltaic disruptive discharge[*]. In the kind and flattering remarks which follow, my friend Dr. Schœnbein deduces a conclusion from those experiments which I had not ventured to make, but as many of the readers of the Philosophical Magazine may not have seen the article in question, I must beg leave to give a summary of its contents.

In considering the experiment communicated to the Philosophical Magazine for 1839, by Mr. Gassiot, in which he points out the remarkable difference between the heating effects at the anode and cathode during the disruptive discharge, it occurred to me that it might be due to the interposed medium, and that were there any analogy between the state assumed by voltaic electrodes in elastic media, and that which they assume in electrolytes, it would follow that the chemical action at the positive electrode in atmospheric air would be more violent than that at the negative, and that if the chemical action were more violent, the heat would necessarily be more intense. Several experiments confirmed this view: the battery with which they were made consisted of 36 elements, each consisting of a square inch of platina foil and of zinc; it was charged with concentrated nitric and dilute sulphuric acid as in my first experiments (Phil. Mag., May 1839) and arranged in single series.

I cannot avoid here a short digression as to the œconomy of this combination: this little battery required a pound of nitric and an equal weight of sulphuric acid[†] to charge it; and thus for the expense of about a shilling I could experiment for 8 or 9 hours without fresh charge; the arc of light obtained was 0·4 of an inch long. I am anxious to avoid the bad taste of eulogizing my own productions, but I think it ought to be generally known that space is not the only thing œconomized in this combination. Professor Jacobi, who has recently written a paper on my battery, and who has wrought on a large scale, states that he has readily fused iridium, &c. &c. after it has been at work for a whole day. An obvious point in the practical œconomy of the voltaic battery is, that the more intense the power of a combination the greater the œconomy, e. g. if one combination can effect with a series of two pair, what another can only effect by a series of 20, the equivalents consumed are as 1 to 10 in favour of the former.

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[*] After writing that letter I had determined to delay the publication of the experiments, in order to add to them some others, but these have been unavoidably postponed.

[†] The dilute sulphuric acid should be of sp. gr. 1·2 and four or five times the volume of the nitric; where there is not this disproportion between the cells it will be well previously to mix the nitric with one or two measures of dilute sulphuric acid. By proper attention to these proportions I obtain by electrolysis oxyhydrogen gas at 6*d*. per cubic foot including zinc consumed, &c.

But to return: experiments made with this battery established the following points:

1st. If zinc, mercury, or any oxidable metal constitute the positive electrode, and platina the negative one, in atmospheric air, while the disruptive discharge is taken between them, a voltameter inclosed in the circuit yields considerably more gas than with the reverse arrangement.

2nd. In an oxidating medium the brilliancy and length of the arc are (with some conditions to be presently noticed) directly as the oxidability of the metals between which the discharge is taken. N.B. Platina is to be regarded as slightly oxidable when influenced by the voltaic discharge; if this be taken for some time between platina points in oxygen, the volume of the gas is diminished.

3rd. In an oxidating medium the heat and consumption of metal is, as observed by Mr. Gassiot, incomparably greater at the anode than at the cathode.

4th. If the disruptive discharge be taken in dry hydrogen, in azote, or in a vacuum *, no difference is observable between the light and heat, whether the metals be oxidable or inoxidable, or whether the oxidable metal constitute the positive or negative electrode.

5th. The volume of oxygen absorbed by the disruptive discharge taken between a positive electrode of zinc and a negative one of platina in a vessel of atmospheric air, is equal to that evolved by a voltameter included in the same circuit.

These experiments present a remarkable analogy between the electrolytic and disruptive discharges. There are, however, two important elements, alluded to in art. 2. which obtain in the latter, and which have little or no influence on the former; these are the volatility and the state of aggregation or tenacity of the metal or conducting body. This is remarkably shown in the case of iron. Iron in air or oxygen gives a most

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* I have not been able to experiment in a Torricellian vacuum; in a well-exhausted receiver, the difference, if any, was very slight.

brilliant voltaic arc, while in hydrogen or a vacuum with the same power a feeble spark only is perceptible at the moment of disruption. Mercury on the other hand gives a tolerably brilliant spark in hydrogen, azote, or a vacuum, and one more nearly approaching to that which it gives in air. Thus in an oxidating medium there are three requisites for a brilliant discharge, viz. oxidability, volatility, and looseness of aggregation: in other media the two latter alone obtain; and the brilliant arc given by charcoal appears to depend principally upon the last; thus wood charcoal gives a larger and more diffuse flame than the carbon from gas retorts.

In the following list the metals are arranged, as nearly as may be, in respect to the length and brilliancy of the arc they give in atmospheric air, the discharge being taken between two points of the same metal:

Potassium,
Sodium,
Zinc,
Mercury,
Iron,
Tin,
Lead,
Antimony,
Bismuth,
Copper,
Silver,
Gold,
Platinum.

It has been noticed by Sir H. Davy, Dr. Hare, and Mr. Daniell, that a certain portion of matter is projected from the positive to the negative electrode: the quantity thus perceptibly projected is indeed very small; but I observed that when the discharge was taken in a close vessel the whole interior was lined with a pulverulent deposit, which if the vessel contained atmospheric air was an oxide of the metal employed, but if it contained azote or hydrogen was a reguline precipitate of the metal. Faraday's researches have established, that in electrolysis a voltaic current can only pass by a derangement of the molecules of matter; that the quantity of the current* which passes, is directly proportional to the atomic disturbance it occasions: he deduces from this that the quantity of electricity united with the atoms of bodies is as their equivalent numbers, or in other words, that the equivalent numbers of different bodies serve as the exponents of the comparative quantities of electricity associated with them. Now what takes place in the disruptive discharge? When we see the dazzling flame between the terminals of a voltaic battery, do we see electricity, or do we not rather see matter, detached, as Davy supposed, by the mysterious agency of electricity, and thrown into a state of intense chemical or mechanical action? Matter is undoubtedly detached during the disruptive discharge, and this discharge takes its tone and colour from the matter employed. Now as this separation is effected by electricity, electricity must convey with it either the identical quantity of matter with which it is associated, or more or less; more it can hardly convey, and if less, some portion of electricity must pass in an insulated state, or unassociated with matter, and some with it. This seemed a highly improbable effect, and these considerations, immediately deducible from Faraday's researches, led me to suppose that the third alternative was most probably the true one, and consequently that the quantity of matter detached by the voltaic disruptive discharge was definite for a definite current, or bore a direct equivalent relation to the quantity electrolyzed in the liquid portions of the same circuit. The difficulties of testing this view by the weight lost, being insuperable without incurring an unjustifiable expense, led me to have recourse to Experiment 5. Zinc having only one oxide which sublimes when deflagrated in air by voltaic electricity, it should follow that if the particles of zinc detached were the equivalents of the matter electrolyzed in the same circuit, the quantity of oxygen absorbed by these particles would be exactly equal to that evolved in a voltameter placed in the circuit. The experiment presented more difficulties than I had at first anticipated: if the intensity of the battery was high, I frequently observed the metal, from the heat it had acquired, burn independently of the discharge; while if the intensity were lowered, the discharge could not be kept up without frequent contacts which gave a gush of gas to the voltameter. The following means afforded me the must uniform results. Between a positive electrode of distilled zinc of such size as to prevent by the cooling effect of its mass an independent combustion, and a negative electrode of platina, a moderated discharge was taken in a graduated vessel filled with atmospheric air: an average of 40 experiments gave me 1·17 to 1·00 as the inverse ratio between the volume of oxygen evolved in the voltameter, and that absorbed by the deflagration, and in several of these experiments the volumes were exactly equal. The nature of the experiment defied perfect accuracy, but considering a few unavoidable contacts, it appears to me to afford strong ground for presumption if not for conviction, that the separation of matter in the voltaic arc is definite for a definite quantity of electricity, and that the all-important law of Faraday is capable of much extension. Uniting this view with the experiments of Faraday on the identity of electricity from different sources, and with those of Fusinieri on the statical electrical discharge, it would follow as a corollary that every disturbance of electrical equilibrium is inseparably connected with an equivalent disturbance of the molecules of matter. In the remarks of Professor Schœnbein to which I have alluded, he says that my experiments seem to prove the transmission of the current from one electrode to the other to be effected only by chemical action. In Exp. 5, I employed the diminution of oxygen as a measure of the quantity detached, conceiving that at the intense heat which is produced not a particle of zinc would escape oxidation, and without concluding that chemical action was absolutely essential to the existence of the voltaic arc. I must at the same time state that the passage of the current is, as proved in these experiments, materially modified by the nature of the elastic medium through which it passes, and is greatly aided when such medium is capable of uniting chemically with the electrodes. In pure dry hydrogen, I have never yet been able to maintain a continuous arc, except with charcoal, which forms carburetted hydrogen; and Davy in reference to his experiment of burning charcoal *in vacuo* states, that a gas was formed which detonated with oxygen; the probability is that some slight portion of air obtained access, and thus carbonic oxide was formed. On the peculiar relation between the electrodes and the elastic intermedium, I feel at present unable to give a clear opinion; the subject is peculiarly difficult; I will content myself with stating my present notion to be, that the voltaic arc bears a similar relation to common flame, to that which electrolysis bears to ordinary chemical action.

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* However unpredicable the words quantity, current, &c. may be of an agent imponderable, and having no definite relation to space, these words express understood functions, and it seems impossible to devise others which would not be open to similar objections.

4, Hare Court, Temple, May 7, 1840.

LXXIII. *On two Norwegian Cobalt Ores from the Skutterude̲r Mine.* *By* Th. Scheerer.*

IN the cobalt mines of Modum in Norway (near Christiania) are found, besides the common cobalt glance, two other distinct cobalt minerals, which differ in their external characters from the usual ore. The following are the descriptions of these minerals:—

First kind. It occurs massive as well as crystallized, has the lustre of arsenical iron, and exactly the same crystalline form, even the characteristic brachydiagonal striæ. In hardness likewise, and also its spec. grav. = 6·23, it does not differ much from that mineral. It is shown by the blowpipe, however, that it contains a considerable quantity of cobalt; three analyses gave the following composition:—

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* An extract, communicated by the Author, from the original article published in Poggendorff's *Annalen*, vol. xlii. p. 546.

1.		2.		3.	
17·57	sulphur	17·34	sulphur	18·06	sulphur.
47·55	arsenic	46·76	arsenic	46·01	arsenic.
26·54	iron	26·36	iron	26·97	iron.
8·31	cobalt	9·01	cobalt	8·38	cobalt.
99·97		99·47		99·42	

In a fourth analysis merely the quantities of iron and cobalt were determined, and were found to be 28·77 iron, 6·50 cobalt. In a fifth the cobalt was 10·8.

All these analyses were performed with quantities of different crystals.

From the difference in the relative quantities of iron and cobalt it follows, that these two metals can remplace one another, which is still more evident if the quantities of both be added together, when each time the same sum is obtained.

	1.	2.	3.	4.
Iron	26·54	26·36	26·97	28·77
Cobalt	8·31	9·01	8·38	6·50
	34·85	35·37	35·35	35·27

The formula for this mineral would therefore be the same as that for arsenical iron, only that iron and cobalt are united in the combination, viz.

$$\left.\begin{matrix}\mathrm{Co}\\ \mathrm{Fe}\end{matrix}\right\}\mathrm{S}^2+\left.\begin{matrix}\mathrm{Co}\\ \mathrm{Fe}\end{matrix}\right\}\mathrm{As}^2$$

On measuring the crystals the following angles were obtained.

Fig. 1.

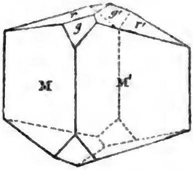
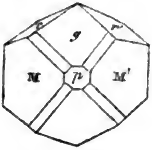


Fig. 2.



1. M : M′ = 111° 40′
2. M : M′ = 111 49
3. M : M′ = 112 2

In arsenical iron it is M : M′ = 111° 53′, consequently but slightly deviating.

The surfaces g, g' were found on different crystals to form the following angles:—

1. $g:g' = 58° 30'$
2. $g:g' = 58\ 32$
3. $g:g' = 58\ 29$
4. $g:g' = 58° 28'$
5. $g:g' = 58\ 30$

while in the arsenical iron $g:g' = 59° 22'$.

The proportion of cobalt appears therefore, as the deviation amounts to nearly a degree, to have acted in this way on the crystalline form of the mineral. From its properties it may most properly be called cobaltic arsenical iron.

Second kind. This is characterized by its strong tin lustre, approaching even to that of silver, and still more by its high specific gravity, which = 6·78. It occurs massive, with more or less distinct tessular surfaces of cleavage, and also in single crystals. There can be no doubt that it is the mineral described some years since by M. Breithaupt, under the name of Tessular pyrites (*Tesseralkies*). The analysis gave,

77·84	arsenic.
20·01	cobalt.
0·69	sulphur.
1·51	iron.
trace of copper.	
100·05	

which accords pretty accurately with the formula Co As^3, according to which the combination should consist of

79·26	arsenic.
20·74	cobalt.
100·00	

if we admit that a quantity of arsenic is remplaced by sulphur and a quantity of cobalt by iron.

The octahedron is constantly predominating in the crystals of this mineral, with subordinate surfaces of the cube, rhombic dodecahedron and icositetrahedron. The last-mentioned surfaces are wanting in none of the crystals. I measured the angles made by two such surfaces with those of the octahedron and found 160° 33′, whence it results that the parameters of the icositetrahedron are as 2 : 1 : 2, therefore just as in the common tin-white cobalt (*Speiskobalt*). It may be called Arsenical Cobalt-pyrites (*Arsenikkobaltkies*).

LXXIV. *On Galvanic Circuits composed of two Fluids, and of two Metals not in contact. By* Professor J. C. POGGENDORFF.*

IN support of the so-called Chemical Theory of galvanism, or rather of that view which places the source of voltaic electricity solely in the chemical affinity of the positive metal of the circuit, viz. the zinc, for the electro-negative constituent of the fluid, Mr. Faraday has of late brought forward in an especial manner the three following arguments:—

1. The production of a spark on completing the junction of a simple circuit.
2. The electrolytic law.
3. The preponderance of a circuit of zinc, platina, and sulphuric acid, over one formed of the same metals and a solution of the iodide of potassium.

With respect to the first point, the production of a spark on making the connexion between a single pair of plates, Faraday, in the Eighth Series of his Experimental Researches, lays much stress upon it, and for this reason;—because it must have passed *before* the metals could have come into contact, and thus not only proves its origin from pure chemical forces, but also the superfluousness of metallic contact for exciting voltaic electricity†; in the further progress of his labours the English philosopher seems, however, to have some doubt as to the reality of a spark on junction; at least he expresses himself in his ninth series in a manner which induces the belief that he considers this spark to be a heating and ignition of the mercury employed in the experiment *at* or *after* the moment of junction‡.

Moreover, Professor Jacobi, in an experiment performed for the very purpose of solving this doubt, in which the wire of junction of a simple, very powerful zinc-platina circuit was interrupted by a layer of air only 0·00005 inch, was not able to perceive the least sign of the passing of a spark at the place of disjunction§. And recently Professor Draper, in New York, could not even observe the spark in a perfect vacuum before direct contact between the mercury and the wire which formed the arc of junction of a simple circuit‖.

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* From Poggendorff's *Annalen*, vol. xlix. January, 1840: translated by Mr. W. Francis.

† Experimental Researches, Ser. viii. §. 915.

‡ *Ibid.* §. 1074.

§ London and Edinburgh Philosophical Magazine, vol. xiii. p. 401.

‖ *Ibid.* vol. xv. p. 349.

It can no longer, therefore, be a question whether there is a true spark on junction without metallic contact; moreover, other good reasons render very improbable the existence of the high tension requisite in the still open simple circuit*.

The electrolytic law is otherwise circumstanced. The correctness of this important law, with which Faraday has enriched the science of electricity, can be subject to no doubt; but many well-founded objections may be raised against its interpretation in favour of the chemical theory. The law consists in the fact that the quantities of the bodies decomposed in the single cells of the voltaic battery are in proportion to the chemical equivalents. It proves that the passage of like quantities of electricity is necessary for the decomposition of equivalent masses, but nothing more. It takes no part in the question respecting the origin of galvanic electricity. It is equally correct, whether the voltaic current originate from the contact of the metals, or from the chemical action on the zinc, or from any other cause†.

A proof in favour of the one or the other origin might perhaps be drawn from it, were it exclusively peculiar to the voltaic current, which would at the same time establish quite an essential distinction between voltaic electricity and magnetic, thermal, frictional, and animal electricity. But if, on the other hand, it is no peculiarity of the voltaic electricity, if it is rather a property of *all* electric currents to decompose on their passage through a series of different fluids equivalent quantities of each, then *à priori*, every consequent conclusion from the law with respect to the origin of voltaic electricity is impossible. That the latter is then actually the case, that in fact the electrolytic law is common to *all* electric currents, that consequently the identity of electricities of various origin (manifoldly proved by Faraday himself in other respects) is established with reference to this law also, the simple experiment, published by me last year, on the simultaneous decomposition of two portions of water by the same magneto-electric current, can leave not the least room for doubt*.

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* [At the time when the sheets of the German original were sent to press, Prof. Poggendorff had not seen the collective edition of the Experimental Researches, in the preface to which Mr. Faraday acknowledges himself to have been in error with respect to the production of the spark without contact.—W. F.]

† The error with respect to using this law as an argument has originated from presupposing what first ought to have been proved by it, that the excitation of the electricity was effected by the solution of the zinc; while in reality, this solution, when amalgamated zinc and dilute sulphuric acid are employed, is solely the effect, the product of the electric current, and, when common zinc is used, arises partly from this current and in part from a pure chemical process entirely foreign to the circuit. It has happened with this law as with the well-known fact that in general the easily oxidable metals are the more positive. A connexion between oxidability and positiveness is accordingly evident, but which of these is causal, this or that, remains totally undecided. That it is, nevertheless, still continued to be interpreted in favour of the chemical theory, is the less justifiable, as several cases are known (and may every moment be increased by new ones) where the negative metal, notwithstanding that it is actually much more powerfully acted upon by acids than the positive one, still remains negative towards this. We need only call to mind the old experiment of Berzelius (*Gilb. Annalen*, vol. xxxv. p. 27), which, it is true, De la Rive conceives he has refuted, but which has long since been satisfactorily confirmed by Ohm. Moreover, the fact observed by Ritter, Davy, and many others, and which I have recently confirmed, that amalgamated zinc, notwithstanding it is little or not at all attacked by dilute acids, is in these considerably positive towards the so easily soluble unamalgamated zinc, deserves to be rescued from oblivion.

Of the arguments advanced in favour of the chemical theory, there is now left in force for the present only the third.

The experiment upon which this is mainly founded consists in the fact, that two strips, one of zinc and the other of platina, are separated at their extremities, on the one side by sulphuric acid, on the other by a solution of the iodide of potassium. An electric current then occurs in a direction which indicates the preponderance of the sulphuric acid circuit over that of the iodide of potassium. The iodide of potassium, which, in case the two metallic slips are in direct contact at their other end, is decomposed in such manner, that its electro-negative constituent, the iodine, passes over to the zinc, gives it to the platina as soon as metallic contact is suspended at those ends and sulphuric acid inserted there.

Faraday places this experiment at the head of his researches on the origin of voltaic electricity. He regards it, as it were, as a scale for weighing two chemical affinities, that of the oxygen and that of the iodine for the zinc. Both endeavour, according to him, to excite an electric current; but that of the oxygen being the strongest, sets more electricity in movement than that of the iodine; the latter is therefore overpowered, and a current thus originates in the direction of the affinity of the oxygen, which, at the same time, since the two metals do not touch, affords another proof of the non-necessity of metallic contact to excite voltaic electricity.

The experiment is so remarkable, and the explanation given has in appearance so much plausibility, that it is not to be wondered at if the supporters of the chemical theory of galvanism have regarded it as a main prop of their opinion. Upon the defenders of the contact theory it made, however, but little impression, probably from their believing that no regard need be had to an isolated fact speaking apparently in favour of the chemical theory, considering the numerous objections which may be urged against it.

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* Poggendorff's *Annalen*, vol. xliv. p. 642.

In general they may have contented themselves with this otherwise perfectly correct position, that one metal, as soon as it is in contact with two fluids, can no longer be regarded as a single metal; so, for instance, that in Faraday's experiment that end of the zinc-bar which touched the sulphuric acid would be positive towards that which was moistened by the solution of the iodide of potassium. This at least is the opinion which Pfaff advances in his *Revision der Lehre vom Galvano-Voltaismus*[*]. Pfaff is also, as far as I am aware, the only philosopher on the continent who has hitherto publicly discussed this experiment, without, however, performing more than a mere repetition of it experimentally.

From the great importance which, be it with justice or not, is assigned by the majority of the philosophers of the present day to the decision of the question respecting the origin of voltaic electricity, a more accurate examination of the process in the Faradayan circuit appeared to me not to be without value, and I therefore undertook the experiments, the results of which will be enumerated in the following pages as briefly and clearly as possible.

Galvanic circuits of two fluids and two metals not in contact may truly, as is readily conceived, be constructed without number. The English philosopher has examined a few, and these few always composed only of zinc and platina with various fluids. With this slight number of cases he always obtained results favourable to his view.

It appeared in the first instance to me to depend on this point: let it be seen whether among the great number of possible cases there may not be some which could not be brought under this view. If this question were affirmed by experiment, another demand would arise; namely, to subject Faraday's explanation even for the apparently favourable cases to a more rigorous examination.

Above all, I considered a greater change with the metals to be necessary, as, from long-known experience in other cases, it seemed to me highly improbable that the negative metal of the circuit should act so passive a part as is assigned to it according to the theory at present prevailing in England.

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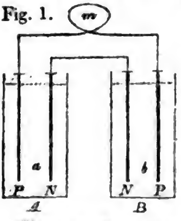
[*] P. 81. It stands there, it is true, quite reversed, that the platina was positive in the acid, and the zinc negative; but the connexion shows that this is only founded in a permutation or typographical error. In this memoir that metal is always termed positive which acts towards a second as zinc to copper in the common circuit.

I therefore made choice of the six metals: *platina, silver, copper, tin, iron,* and *zinc,* (*common* as well as *distilled* and *amalgamated*). In some cases I examined all the combinations which might be formed into pairs from these elements; in most cases, however, I was satisfied with circuits containing *zinc, iron,* or *tin,* as the positive element, since the three noble metals combined *inter se* give rise only to very slight effects. All the metals were as pure as they could possibly be obtained, and formed plates of nearly equal size, viz. somewhat about 3·5 inch in length, and 1 inch in breadth.

The fluids employed were: *water*, dilute *sulphuric acid* (acid of 1·827 spec. gr. diluted with 9 times its volume of water), dilute *nitric acid* (acid of 1·321 sp. gr. with 6 times its vol. of water), dilute *hydrochloric acid* (acid of 1·138 sp. gr. with 6 times its vol. of water), saturated solution of *chlorine,* liquid *caustic ammonia* (of 0·97 spec. gr. diluted with 4 times its vol. of water), and solutions of *caustic potassa* (1 part in weight to 4 parts in weight of water), *carbonate of soda* (1 in 3 water), *sulphate of magnesia* (1 to 3 water), *borax* (saturated), *sulphate of zinc* (1 to 4 water), *salt* (saturated), *sal-ammoniac* (saturated), and *iodide of potassium* (1 part in weight to 4 in weight of water). Distilled water was employed for the solutions; but where the water alone was tested, spring water was generally made use of, as it conducts better than distilled water, and does not sensibly differ in its electromotive action. All the above substances were as pure as possible; thus the sulphuric acid was free from nitric acid, the hydrochloric acid from chlorine.

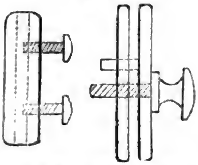
The mode of performing the experiments was as follows: into two small glass vessels (*A, B*) I poured two of the above fluids (*a, b*) to the height of 2·5 inches, placed in each a heterogeneous pair of plates (*P, N*), and connected the plates of like nature by copper wires, of which one was the wire of the multiplier (*m*), whose needle had to indicate by its deflection the presence, the direction, and also comparatively the force of the electric current.

Fig. 1.



Two arrangements, which I will here describe more fully, from their great usefulness and manifold applicability in all galvanic experiments, served to effect an easy and certain combination of the wires with the plates; the first, it is true, has long been known to some experimentalists. They are both represented of the actual size in the annexed Fig. 2. The first

Fig. 2.



consists of a bored copper cylinder with two side screws. In this is inserted from the one side the end of the connecting wire, and from the other the piece of wire soldered to the plate, or instead of that the plate itself, which can be so cut that it may be inserted in it. When the screws are tightened connexion is made. The second is constructed of two copper plates, which can be pressed against one another by a screw. In order that this may be effected without disturbing the plates, the one is provided with a pin which fits into an aperture in the other; this latter has also in the centre, on its inner side, a wedge-shaped furrow for its whole length. By means of this second clamp, thin plates, to which it is not desirable to solder pieces of wire, may be, as will easily be conceived, combined with wires, for the better insertion of which the above-mentioned furrow is made. The two clamps in this respect serve perfectly well all the purposes of mercury, without possessing its numerous inconveniences.

In general the current of a circuit of the kind described has but slight intensity; nevertheless it is always very perceptible with a sensitive multiplier. Frequently it was even so energetic that the magnetic needle of the instrument beat with violence against the pins which were erected at the points 90° to prevent its entire reversion; but often the deflections were but feeble, and in these cases especially I always took the precaution to exchange reciprocally the four plates employed for each experiment, and to take the mean from all the readings. In this way, it is true, each experiment became four experiments; but this exchange is quite necessary, as we never find, especially in plates of non-noble metals, iron and zinc, two homogeneous to such a degree, even when cut out of the same mass, as not to produce, when immersed alone in a conducting fluid, a frequently considerable current, which might easily overpower that which is intended to be observed.

Only in some cases where I was already acquainted with the direction of the current, for instance on repeating some experiments which I had previously performed, was I satisfied with employing the zinc plates in the combination, so that their heterogeneity should act against the current, instead of exchanging them. If the current was merely weakened by this, and not reversed, the result can be noted as certain.

I had hoped that the heterogeneity of the zinc would be less with the *distilled* metal, but found it nearly quite as great as with the common zinc, and even, as with the latter, increasing on long use in acids, so that plates which fresh were nearly homogeneous, acquired with time a considerable heterogeneity. Filing the surfaces thoroughly bright was the sole means of restoring them to their original state. Nevertheless I employed in the further progress of my inquiries, and on the repetition of experiments, only distilled zinc, to be more certain.

For a similar reason I also had recourse to *amalgamated* zinc. Plates of ordinary zinc, possessing a very considerable degree of heterogeneity, lose it in fact by amalgamation (produced by rubbing them with dilute sulphuric acid and mercury), and become almost perfectly homogeneous; but they remain so only immediately after this operation, so long as the surface of the metal is, as it were, fluid. After some time the amalgam hardens to a crystalline mass, generally sooner or to a greater extent on one plate than on the other, and then the heterogeneity again makes its appearance. The plate with the fluid surface is in acids positive to that with the crystalline dull appearance. By rubbing the latter with mercury the heterogeneity may again be suppressed. Although it is in our power to bring amalgamated zinc plates to any degree of homogeneity previous to each experiment, yet it did not appear advisable to employ solely such, as amalgamated zinc must in a certain degree be regarded as a different metal from ordinary zinc, and its easy liability to change might produce disturbances. The sequence, however, showed that in general it affords the same results as the non-amalgamated zinc, and in numerous cases possesses considerable advantages over the latter.

Besides this, no precaution was neglected that is indispensable in this kind of experiments*; for instance, the cleansing of the plates after each experiment, by immersion in water, drying with bibulous paper, or, if it seemed requisite, by rubbing with sand-paper, scouring with sand and acids or water; although these operations are exceedingly troublesome from frequent repetition, yet they were never neglected. Moreover, the platina was, previous to each experiment, heated over an alcohol lamp after having been cleansed, as it otherwise only produces weak effects. I therefore believe that the following results merit some confidence, especially as most of them are deduced from several experiments repeated on various days.

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* Here among other things might be reckoned the order of immersion, for it is a well-known fact, that of two plates of one and the same metal, on being immersed in a like fluid, that dipped in last is always *negative* to the one first inserted. I have found this to be perfectly true, but have likewise observed that on the *contemporaneous* immersion of two zinc plates in water slightly acidulated with sulphuric acid, several reversions in the direction of the current are produced; for instance, in the first moment a westerly deflection of 10°, followed by an easterly of 70°, which was soon succeeded by a permanent easterly deflection of 20°, that gradually sunk down to 0°, and passed into a westerly deflection of 38°; sometimes a couple more reversions occurred. After several contemporaneous takings out and immersions only the westerly deflection was evident. A mere knocking one of the plates against the bottom of the vessel, which produced an elevation of about $\frac{1}{3}$ line, rendered this plate negative, or, causing with respect to the deflection, an increase or diminution in this deflection, although but transitory. The plates employed for this purpose were recently scoured, and at the termination of the experiments, which lasted nearly an hour, they had lost from the weakness of the acid little or nothing of their brightness. Both became covered with bubbles of hydro-

Before, however, communicating these results, I must still add one remark.

By whatever cause the electricity may be produced in circuits of this kind, it is evident that there can be no doubt respecting the *where*, that it can only occur at the places of contact of the fluids with the metals,—since a contact of heterogeneous metals does not take place, or rather each of the two metallic slips contains two such contacts, which, from their being of opposite nature, must necessarily nullify each other. There are, therefore, in this kind of circuits four possible places of excitation, two in each vessel; and if we combine the electromotive force developed in each vessel, we have two such forces, e and e', which act in opposition to each other. If, moreover, we call r the total resistance of the circuit, then, according to Ohm's fundamental law for the intensity of the resulting current, we obtain the expression $\frac{e-e'}{r}$.

Accordingly, the direction of the current, *i. e.* the direction of the deflection of the needle of the multiplier, depends solely on the sign of the difference $e-e'$; on the other hand, the intensity of the current, or the magnitude of the deflection, *both* on the value of the difference $e-e'$ and on the value r of the resistance. The amount of the deflection of the needle affords

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gen, the temporarily negative one, so it seemed, always to a greater extent; yet but few bubbles ascended from it.

Amalgamated plates exhibit the same phænomena; but since in this case stronger acid may be employed without giving rise to any disturbance, it can be observed that the mere raising one of the plates about one inch renders it considerably negative; re-immersion again increases the negativeness. I observed this with sulphuric acid of 1·827 spec. gr. diluted with 9 times its volume of water, into which the plates were immersed to the depth of 2·5 inch.

These enigmatical currents are all of them, however, but of transitory duration, and they can therefore, in the following experiments, in which the positive metal, situated in the acid, was in most cases immersed last, at the utmost have only effected the first deflections, and then only when these were feeble. But in general the heterogeneity which originates for the positive plates from the contact with two fluids, is far stronger. At times I have also convinced myself that the results were essentially the same, whether the positive plates were immersed at the same time, or one after the other, before or after the negative plates.

alone, therefore, no measure for the difference of the electromotive forces here entering into action, the determination of which, however, is the sole object of the present inquiry. I have therefore, in the following experiments, chiefly directed my attention to the accurate determination of the direction of the current; its relative intensity, being dependent on too many circumstances, is only given approximately: the magnitude of the deflections of the needle were, however, always carefully noted.

M. Vorsselman de Heer, in his valuable memoir on Electric Telegraphy*, states of this numerator, after having observed that it depends on the nature of the metals, and not on their dimensions; that its value is not altered when salts, alkalies, or acids, which, like sulphuric acid, or nitric acid, are not electrolyzable, are added to the water; but that it undergoes a change when the body which is added is itself an electrolyte, for instance hydrochloric acid, in which case the numerator would be smaller.

According to Faraday, who, however, is not acquainted with Ohm's theory, this numerator would be greater, the stronger the affinity of the positive metal, the zinc, is for the oxygen, chlorine, or electro-negative constituent of the fluid in general; it must, however, be here observed, that we possess at present nothing more than approximate valuations from which we may judge of the energy of a chemical affinity.

The main result of my experiments has proved in the most positive manner *that the value of the numerator in Ohm's formula, or the magnitude of the electromotive force in general is altered, sometimes increased, sometimes diminished, by any substance added to the water, be it an electrolyte or not, and indeed (which should be well observed) increased for one metal combination, and diminished for another, by the same substance, added to the water in the same proportion.*

Nor have I been able to find that this force stands in direct ratio to the energy of the affinity between the positive metal and the negative constituent of the fluid. It is weak *in cases where this energy must be considered as* strong, *and on the contrary* strong *where but a* weak *affinity can be admitted. Frequently, indeed, a current originates, and at times a powerful one, where, to judge from the affinity, not the slightest action should be expected.*

Sufficient proofs of this will be found in the statements contained in the following Tables.

To render these Tables intelligible, it may be observed that the signs of inequality express which of the two fluids (*a* and *b*, fig. 1, page 489) developes the strongest electromotive force when a pair of plates of the metals stated at the head of the column are immersed in them. The expression $s < w$, for instance, in the space common to the columns *zinc-silver*, and *hydrochloric acid water*, indicates that in contact with *zinc* and *silver* the *water* excites a greater electromotive force than the *hydrochloric acid*; or, in other words, that the current has a direction as if the zinc plate in the water were positive to the zinc plate immersed in the acid, the word positive being taken in the sense previously mentioned.

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* Poggendorff's *Annalen*, vol. xlvi. p. 516.

I might also have added to the direction of the current, or the direction of the magnetic deflections, their magnitudes, as I always noted them carefully. But I have omitted this, on the one hand, because the intensity of the current is no measure of the difference of the electromotive forces under consideration, and also because in the present case it is at the same time a very variable element. In general the intensity of the current in this kind of circuits is most powerful in the first moment, and then rapidly decreases; the amount and velocity of the decrease vary however considerably. This causes among other things that in two cases the difference of the two first deflections of the needle, to the right and to the left, may indeed be equal, but the magnitudes of the deflections differ considerably. In the one case these deflections amounted perhaps to 10° and 8°, in the other to 30° and 28°. In general the intensity of the current only descends to zero, but at times it also increases towards the opposite side. Sometimes this reversion of the current takes place so rapidly, that, for example, the first deflection of the needle, perhaps of 50° to the right, is immediately succeeded by one on the left of 70°. Now and then the intensity of the current is but feeble in the first moment, increases then for a time, and then gradually sinks to zero. Such variations render a numerical comparison of the intensity of the current in the different cases quite impossible, especially as in general, on the repetition of one and the same experiment, highly different values are obtained for the magnitudes of the deflections. I have therefore merely stated in general, in the notes to the Table, when the current was especially strong or feeble: when reversions of the current result, they are for the most part already indicated in the Table by the signs of inequality placed beneath one another.

DIFFERENCE OF THE ELECTROMOTIVE FORCES ON THE CONTACT OF TWO METALS WITH TWO FLUIDS.

No.	Fluids of the degrees of concentration mentioned.	Zinc. Platina.	Zinc. Silver.	Zinc. Copper.	Zinc. Tin.
1	Sulphuric acid (*s*) Water (*w*)	$s > w$ $s < w$	$s < w$	$s < w$	$s < w$
2	Nitric acid (*s*) Water (*w*)	$s > w$	$s < w$	$s < w$	$s = w$
3	Hydrochloric acid (*s*) Water (*w*)	$s < w$	$s < w$	$s < w$	$s < w$
4	Solution of chlorine (*c*) Water (*w*)	$c > w$	$c < w$	$c < w$	$c = w$
5	Solution of chlorine (*c*) Hydrochloric acid (*w*)	$c > s$	$c > s$	$c > s$	$c < s$ $c > s$
6	Caustic potash (*a*) Water (*w*)	$a > w$	$a > w$	$a > w$	$a < w$
7	Ammonia (*a*) Water (*w*)	$a > w$	$a > w$	$a < w$	$a > w$
8	Carbonate of soda (*n*) Water (*w*)	$n < w$	$n < w$	$n < w$	$n < w$
9	Sulphuric acid (*s*) Borax (*b*)	$s > b$	$s > b$	$s > b$	$s > b$
10	Sulphuric acid (*s*) Iodide of potassium (*i*)	$s < i$	$s > i$	$s > i$	$s < i$
11	Hydrochloric acid (*s*) Iodide of potassium (*i*)	$s < i$	$s > i$	$s > i$	$s < i$
12	Sulphuric acid (*s*) Sal-ammoniac (*l*)	$s < l$	$s < l$	$s > l$	$s < l$

1. With *platina* $s > w$ merely weak, already diminishing on the second immersion, and then $s < w$ stronger than with silver, copper, tin; with *tin* $s < w$ increasing.

2. Results differing with *silver*, sometimes $s = w$, sometimes $s > w$, in most cases however decidedly $s < w$.

3. All action feeble; with *copper* however pretty strong.

4. With *platina* and *copper* very strong, somewhat weaker with *silver*. With *tin* an inclination to $c < w$.

5. With *platina* and *copper* very strong, with *silver* weaker, with *tin* only the first (strong) deflection $c < s$, immediately succeeded by strong $c > s$.

6. With *silver* $a > w$ strong, after which, though to a slighter extent, with *tin* the $a < w$.

7. Strongest effects with *silver* and *copper*.

8. Action feeble, strongest with *platina* and *copper*.

9. Action pretty strong, weakest with *silver*.

10. With *platina* in two experiments, first $s > i$ *very strong*, which however rapidly decreased and changed into $s < i$ *quite as powerful*. In all the following experiments, although the platina was always previously heated, the first effect was $s < i$ and generally very strong, sometimes moderate at the commencement, and on repeated immersion *increasing*.—With *silver* and *copper* action very strong, with *tin* likewise but opposite.

11. With non-heated *platina*, sometimes, but very feeble, $s > i$, passing into $s < i$. But with heated platina immediately and *very strong* $s < i$. With *silver* and *copper* $s > i$ very strong, with *tin* $s < i$ moderate.

12. With *copper the* $s > l$ strong, still stronger $s < l$ with *tin*. With a circuit of *tin* and *copper* likewise $s > l$.

No.	Fluids of the degrees of concentration mentioned.	Zinc. Platina.	Zinc. Silver.	Zinc. Copper.	Zinc. Tin.
13	Hydrochloric acid (*s*) Sal-ammoniac (*l*)	$s > l$ $s < l$	$s < l$	$s < l$	$s < l$
14	Salt (*k*) Water (*w*)	$k > w$ $k < w$	$k < w$	$k < w$	$k = w$
15	Salt (*k*) Hydrochloric acid (*s*)	$k < s$ $k > s$	$k > s$	$k > s$	$k > s$
16	Salt (*k*) Sal-ammoniac (*l*)	$k < l$	$k < l$	$k < l$	$k < l$
17	Sulphate of zinc (*z*) Borax (*b*)	$z > b$	$z > b$	$z > b$	$z > b$
18	Sulphate of magnesia (*m*) Borax (*b*)	$m > b$	$m > b$	$m > b$	$m > b$

No.	Fluids of the degrees of concentration mentioned.	Iron. Platina.	Iron. Silver.	Iron. Copper.	Iron. Tin.
19	Sulphuric acid (*s*) Water (*w*)	$s > w$ $s < w$	$s = w$	$s < w$	$s < w$
20	Hydrochloric acid (*s*) Water (*w*)	$s < w$	$s < w$	$s < w$	$s < w$
21	Caustic potash (*a*) Water (*w*)	$a < w$	$a < w$	$a < w$	$a < w$
22	Ammonia (*a*) Water (*w*)	$a < w$	$a < w$	$a < w$	$a < w$
23	Sulphuric acid (*s*) Iodide of potassium (*i*)	$s > i$	$s > i$	$s > i$	$s < i$
24	Hydrochloric acid (*s*) Iodide of potassium (*i*)	$s > i$ $s < i$	$s > i$	$s > i$	$s < i$
25	Sulphuric acid (*s*) Borax (*b*)	$s > b$	$s > b$	$s > b$	$s > b$
26	Sulphuric acid (*s*) Sulphate of zinc (*z*)	$s < z$	$s < z$	$s < z$	$s < z$

13. With *platina* at first $s > l$ slowly passing over into a stronger $s < l$. *Silver* weak, *copper* strong (although of opposite nature as with sulphuric acid), *tin* very strong.

14. *Platina* weak, *silver* and *tin* moderate, *copper* strong.

15. *Silver* weak, the other metals *moderate*.

16. *Copper* strong, *platina* almost nil.

17. *Platina* and *copper* pretty strong.

18. *Copper* strongest.

19. All actions feeble.

20. Likewise: with *copper* strongest.

21. *Platina* moderate, *silver* weak, *copper* pretty strong, *tin* very strong.

22. *Silver* weak, *tin* moderate, *platina* pretty strong, *copper* very strong.

23. *Platina* strong, *silver* and *copper* very strong, *tin* feeble.

24. With *platina* $s > i$ at first and slowly passing into $s < i$ only once observed; subsequently always immediately $s < i$. With *silver* and *copper* $s > i$ very strong.

25. With *copper* and *tin* strong.

26. With *platina* strong, after which *tin*.

No.	Fluids of the degrees of concentration mentioned.	Tin. Platina.	Tin. Silver.	Tin. Copper.
27	Sulphuric acid (*s*) Water (*w*)	$s < w$	$s > w$	$s > w$
28	Nitric acid (*s*) Water (*w*)	$s < w$	$s < w$	$s < w$
29	Hydrochloric acid (*s*) Water (*w*)	$s < w$	$s < w$	$s < w$
30	Caustic potash (*a*) Water (*w*)	$a = w$ $a > w$	$a > w$	$a > w$
31	Ammonia (*a*) Water (*w*)	$a < w$	$a < w$	$a < w$
32	Sulphuric acid (*s*) Iodide of potassium (*i*)	$s > i$	$s > i$	$s > i$
33	Hydrochloric acid (*s*) Iodide of potassium (*i*)	$s > i$ $s < i$	$s > i$	$s > i$

No.	Fluids of the degrees of concentration mentioned.	Amalg. of Zinc. Platina.	Amalg. of Zinc. Silver.	Amalg. of Zinc. Copper.	Amalg. of Zinc. Tin.	Amalg. of Zinc. Iron.	Amalg. of Zinc. distilled Zinc.
34	Sulphuric acid (*s*) Water (*w*)	$s > w$ $s < w$	$s < w$	$s < w$	$s < w$	$s > w$ $s > w$	$s < w$ $s > w$
35	Hydrochloric acid (*s*) Water (*w*)	$s > w$ $s < w$	$s < w$	$s < w$	$s > w$	$s > w$ $s > w$	$s < w$ $s > w$
36	Caustic potash (*a*) Water (*w*)	$a > w$	$a > w$	$a > w$	$a < w$	$a > w$	$a < w$ $a > w$

27. With *silver* and *copper* the $s > w$ weak, often $s = w$.
28. All weak, yet with *platina* the first deflection = 20°.
29. Actions weak, the least so with *copper*.
30. With *silver* and *copper* stronger than with *platina*.
31. With *silver* and *platina* weak, with *copper* pretty strong.
32. *Silver* and *copper* strong, *platina* very feeble.
33. With *platina* both the $s > i$ as also the subsequent $s < i$ weak. With *silver* and *copper* the $s > i$ very strong.
34. In all the experiments with amalgamated zinc plates these were constantly kept in the fluids, and the negative plates only were taken out and immersed both at the same time.—With *platina* only the first deflection of 10° to 20° in the direction $s > w$, succeeded by one of 90° in the direction of $s < w$. The effect far more energetic than with *silver*, *copper*, *tin*, and than with distilled, not amalgamated, zinc in similar experiments (although here as well as in No. 1 the *platina* had been heated to redness).—With *iron* immediately a very slow deflection of 20° to 30°, then a tranquil increase of the deflection to a permanent value of 40° and above.—With *zinc* immediately 70° in the direction $s < w$, rapidly decreasing and passing into $s > w$ to 20°.
35. In this case with *platina* the reversion evident, but making its appearance slowly; after repeated immersions only $s < w$.—*Tin* forming an exception from No. 3, 20, 29.—With *iron* no increasing effect as in No. 34, but giving a stronger one than there.—*Zinc* as in No. 34.
36. With *platina* weak, with *copper* somewhat stronger, but very strong with *silver* and *iron*. The $a < w$ with *tin* pretty powerful; both effects weak with *zinc*.

No.	Fluids of the degrees of concentration mentioned.	Amalg. of Zinc. Platina.	Amalg. of Zinc. Silver.	Amalg. of Zinc. Copper.	Amalg. of Zinc. Tin.	Amalg. of Zinc. Iron.	Amalg. of Zinc, distilled Zinc.
37	Ammonia (*a*) Water (*w*)	$a > w$?	$a > w$	$a < w$	$a > w$	$a > w$	$a > w$
38	Sulphuric acid (*s*) Iodide of potassium (*i*)	$s < i$	$s > i$	$s > i$	$s < i$	$s < i$ $s > i$	$s < i$ $s > i$
39	Hydrochloric acid (*s*) Iodide of potassium (*i*)	$s < i$	$s > i$	$s > i$	$s > i$ $s < i$	$s < i$ $s > i$	$s < i$ $s > i$

37. With *platina*, *copper*, and *zinc* feeble, stronger with *silver*, still stronger with *tin*, and exceedingly strong with *iron*.

38. Even with *non-heated* platina the $s < i$ powerful. The $s > i$ also strong with *silver* and *copper*, less so the $s < i$ with *tin*. With iron the action exceedingly energetic, first a deflection of 40° in the direction $s < i$, immediately succeeded by one of 90° and oscillations between 90° and 80° in the direction $s > i$.—With *zinc* just the same, the first deflection in the direction $s < i = 90°$, the second in the direction $s > i = 90°$ followed by oscillations between + 90 and + 75°.

39. Even with *non-heated* platina $s < i$ powerful, with it at least no reversion. With *silver* and *copper* action very strong.—With *tin* the $s > i$ weak, slowly, and especially after repeated immersion passing into $s < i$.—With *iron* only the first deflection of 60° to 70° in the direction $s < i$, the second immediately in the direction $s > i = 90°$, then oscillations from + 90° to + 80°.—With *zinc* precisely the same.

A careful glance at the facts contained in these Tables will justify the correctness of the positions above advanced. I will here draw attention to some distinct cases which will establish them more perfectly.

[To be continued.]

LXXV. *On the moving the Knight over every Square of the Chess-board alternately. By* GEORGE WALKER, *Author of various Works on Chess; and Honorary Secretary of the St. George's Chess Club.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOUR Number for the present month has just come into my hands, and I find it contains an interesting paper upon the knight's move, by Dr. Roget, to which my attention is particularly drawn, from the circumstance of that essay having, it appears, been suggested by one of my chess articles in Fraser's Magazine (see Fraser of March 1840.). In that memoir, the subject of the knight's circuitous leap was merely touched upon *en* *passant*; my object being only to give a method by which the cavalier could be guided mechanically through the labyrinth of the sixty-four squares, without the director's seeing the board, but ordering the move from memory alone; and the means furnished by me seem fully to meet the end in view. Dr. Roget's solution of the problem is highly ingenious, and it has given me much pleasure to follow his knight over the field. The subject having been once broached in your scientific Journal, it is my present aim to add in some degree to the materials already contributed by the learned secretary of the Royal Society.

Those who have not gone deeply into chess are hardly aware that a whole library has been written upon the knight's move, and ten thousand modes are printed in which the feat may be performed. Many of these methods are coupled with the most curious conditions, and must have taken long years to perfect. In addition to the authorities quoted by Dr. Roget, I append a list of works and writers, exclusively devoted to the problem in question. That so much time has been well spent, I am not prepared to admit; although the matter is as fair a hobby to ride as any other species of solitary calculation: but attached enthusiastically as I am to the practice of chess as a game, I cannot but regret the same energies have not been applied to illustrate points more immediately connected with the conduct of our scientific sport. Many parts of chess, particularly the openings and endings of the game, are capable of mathematical demonstration; and to name one, the celebrated study of what should be the legitimate termination of the strife by King, Rook, and Bishop, against King and Rook only, has been an *open question* from earliest time. Philidor, at the head of a strong body of chess scribes, considers the Rook and Bishop ought to win by force; while an equal number of writers entitled to authority dismiss this quantum of conflicting strength as a drawn game. To him who shall first give a printed solution of this highly difficult problem, the thanks of the European chess circle would be eagerly and gratefully tendered; and we should be proud to enrol him in the St. George's Chess Club, as the man who had achieved a task, compared with which the labours of Hercules were but as typical.

To lay down a general principle by which the march of the knight may be unhesitatingly guided over the 64 squares alternately is, however difficult, possible to be done; and this without burdening the memory with letters, words, figures, or other cumbersome elements of similar machinery. The following is the best, because the most simple, of the numerous different plans of action which have come under my observation relatively to the subject. I take it from the most voluminous work extant upon the subject, being the German treatise of Warnsdorf.

Place the knight to commence on any square of the board you please, marking such square with a wafer or counter to show it has been used, and marking in a similar manner each square on to which the knight leaps in succession. The knight being at his post as agreed on, let his first move be to that square from which, in play, he would command the fewest points, observing that if on two or more squares his power would be equal, he may go indifferently to either of those squares; and that, as matter of course, a square once covered is not to be reckoned amongst those he commands, but must be dismissed altogether as done with. Continue moving him on this principle, and he will traverse the sixty-four squares in as many moves; reckoning the setting him down originally on the square chosen for the starting-point as the first move. To exemplify this, suppose the knight to start upon the king's bishop's second square. In this case his first move must be of necessity to the corner; since upon the rook's square he would then command but one point, viz. the knight's third; the king's bishop's second, on which he started, not to be again counted, but as a *caput mortuum*. Further explanation of matter so simple cannot be necessary.

Permit me, Gentlemen, to express my gratification at seeing chess at length take its legitimate place among the higher branches of science, and its mysteries allowed to develop themselves upon your pages. The king of games is, indeed, now fully appreciated as the only rational mental recreation,—the strongest auxiliary in the way of sedentary sport towards weaning the young from frivolous and exceptionable amusements, and furnishing their minds with healthy exercise. It has been recently enacted that chess may be played in the Royal Institution (Albemarle-street); and I cannot but hope the example will be followed, until chess-boards and chess-men will be found placed in the halls of meeting of every learned and scientific association throughout the kingdom.

I have the honour to be,
Gentlemen, your obedient servant,
George Walker.

1, Devonshire Place, Haverstock Hill,
April 1840.

List of Works and Writers above referred to.

R. Willis. Attempt to analyse the Automaton Chess-player. Lond. 1821. 8vo, pp. 40.

Pratt's Philidor. Lond. 1825, 8vo. (I pass over many Magazine articles, and brief notices of the knight-problem, in numerous works on chess; as well in various collections of a general character, as Guyot's Recreations, &c.).

Ciccolini. Del Cavallo degli Scacchi. Paris, 1836, 4to, pp. 70, and 20 large plates. (This volume treats not only on the problem of the knight's move over the common chess-board of 64 squares; but also on the larger field of 100 squares; as well as the circular board of 64.).

La Corso del Cavallo per tutti gli Scacchi della Scacchiere. Bologna, 1766, 4to.

Collini. Solution du problème du Cavalier, &c. Manheim, 1773, 8vo. pp. 62.

Essai sur les problèmes de situation. Rouen, 1783, 8vo. pp. 74.

Der Rœsselsprung, &c. bei Eduard Billig. 1831, 24mo. pp. 64.

Warnsdorf, H. C. von. Des Rœsselsprunges, &c. Schmalkalden. 1823. 4to, pp. 68.

Lettre adressée aux auteurs du Journal Encyclopédique sur un Problème de l'Echiquier. Prague, 1773.

Dollinger, 24 verschiedene Arten den Springer, &c. Wien, 1806, 8vo.

Netto das Schachspiel:—And numerous other German writers.

LXXVI. *Memoir on the Law of Substitutions, and the Theory of Mechanical Types.* *By* M. Dumas.

[Continued from p. 447.]

Mechanical Types.

AFTER having verified in a manner which satisfied my own conviction, the existence of certain chemical types, I tried the general application of this theory of types to all the known series produced by substitution, and last year at the School of Medicine I made this system of ideas the basis of my lectures.

But always constant to the experimental progress of the science, and wishing never to swerve from it, I asked myself whether it was necessary to class together bodies having the same formula, produced by substitution, but essentially different in their most prominent chemical properties?

I said, the bodies produced by substitution are divided into two different classes: some evidently belong to the same genus, to the same chemical type: the others could not take their place there. What place can be properly assigned to these kinds of bodies?

We have not to wait long for a reply, and it carries the law of substitutions to a degree of generality and importance which it does not belong to me to develop here, but which the order of ideas obliges me to indicate.

The admirable work of M. Regnault on the æthers has indeed given an unexpected development to the theory of types. There is nothing more natural than to class in one genus bodies which approach so near as acetic acid and chloracetic acid: but there must be good reasons for admitting that there is a true analogy between

Marsh gas	$C^4\,H^2\,H^6$
Methylic æther	$C^4\,O\,H^6$
Formic acid	$C^4\,H^2\,O^3$
Chloroform	$C^4\,H^2\,Ch^6$
Bromoform	$C^4\,H^2\,Br^6$
Iodoform	$C^4\,H^2\,I^6$
Chloridated methylic æther	$\left\{C^4\,O\,\begin{matrix}H^4\\Ch^2\end{matrix}\right.$
Bichloridated methylic æther	$\left\{C^4\,O\,\begin{matrix}H^2\\Ch^4\end{matrix}\right.$
Perchloridated methylic æther	$C^4\,O\,Ch^6$
Hydro-chlorate of methylene	$C^4\,Ch^2\,H^6$
Chloridated hydrochlorate of methylene	$\left\{C^4\,Ch^2\,\begin{matrix}H^4\\Ch^2\end{matrix}\right.$
Bichloridated hydrochlorate of methylene	$\left\{C^4\,Ch^2\,\begin{matrix}H^2\\Ch^4\end{matrix}\right.$
Chloride of carbon	$C^4\,Ch^2\,Ch^6$

Amongst these bodies, to which, without forcing anything, we might add prussic acid and ammonia, we meet with acids, bases, neutral bodies, and consequently substances the most unlike in an ordinary chemical view.

M. Regnault admits, and, he purposes to prove, that all the bodies so unlike, chemically speaking, which this series contains, that all those which can be united in analogous series, have this in common, that they belong to the same mechanical system.

I repeat that it is not for me to explain views which will be explained hereafter by their inventor, but I have to show in what these views differ from those which preceded them, to cause it to be felt how they complete what may now be called the general theory of substitutions. Moreover, it is easy to class these ideas in the clearest manner, by the three following propositions:—

1. Experience proves that a body may lose one of its elements and take another in its place, equivalent for equivalent; this is the general fact of substitutions.

2. When a body is modified in this way, we may admit that its molecule has always remained intact, forming a group, a system in which one element has taken the place of another purely and simply.

In this point of view, which is altogether mechanical, and which is that of which M. Regnault pursues the study, all the bodies produced by substitution present the same grouping and may be referred to the same molecular type. I look upon them as constituting a *natural family*.

3. Amongst the bodies produced by substitution, there are a great number which evidently keep the same chemical character, acting the part of acid or of base in the same manner and in the same degree as they did before the modification they have undergone.

These are the bodies which I have considered as belonging to one chemical type, as making a part of the same genus, to speak the language of naturalists.

Thus is explained the law of substitutions, thus do we give an account of the circumstances in which it is not observed.

Every time that a body is modified without departing from its molecular type, it is modified according to the law of substitutions.

Every time that a body in becoming modified passes into another molecular type, the law of substitutions ceases to be observed in the action which ensues. Blue indigo is not a body of the same type as white indigo; the perchloride of carbon does not belong to the type of olefiant gas; aldehyd has departed from the type of alcohol; hydrated acetic acid does not belong to the type of aldehyd, &c.

The Academy will observe how, in this long series of researches, which has required six years of labour and the concurrence of the most skilful French chemists, we have risen from an obscure corner of the science, gradually and by the force of experience only, to the most general ideas of natural philosophy.

I admit then that through all the substitutions that a compound molecule can have undergone, when for all its elements others have been substituted successively, so long as the molecule remains intact the bodies obtained always belong to the same *natural family*.

When through the effect of a substitution, a body is transformed into another which presents the same chemical actions, these two products belong to the *same genus.*

Alcohol, hydrated acetic acid, chloracetic acid, belong to the same natural family. Acetic acid and chloracetic acid belong to the same genus.

Such are the bases of the natural classification of organic substances, which I shall have soon an opportunity of developing before the Academy.

Before going further, it is just to notice here the labours of the chemists who have directed the science towards the point of view which now occupies us.

M. Regnault not only takes the first place in this respect, by the date of his observations, but by the importance of his researches and with respect to the ideas he has deduced from them, we must consider this young chemist as having advanced more than any one the state of the science on this point.

In my own name I can speak more freely than when I was commissioned to express the opinion of the Academy, and I think it my duty to declare here that the views of M. Regnault are connected with physical studies of the highest order, and that they give to the theory of substitutions a development as fortunate as it was unlooked for, in its application to the study of the most intimate physical properties of bodies.

At the same time with M. Regnault, two other chemists well known to the Academy, MM. Persoz and Laurent, were also occupied in researches concerning the theory of substitutions.

One of them indeed, M. Persoz, did not appear to occupy himself with the application of this theory; but the formula by the help of which he endeavoured to express the composition of a great number of mineral bodies, agreed perfectly with the developments which the theory of substitutions received by degrees from experience. The system of formulæ adopted by M. Persoz, and the views which they express in mineral chemistry, have then found a fortunate application in a great number of facts which the theory of substitutions has led to the discovery of in organic chemistry.

M. Laurent on his side has made a multitude of researches, and has published a great number of memoirs in support of the laws by which he sought to foresee and to explain all the phænomena of substitutions. As we saw above that the principal difficulty which is opposed to the approximation of acetic acid and of chloracetic acid consists in the similar function which we are compelled to attribute to chlorine and to hydrogen, it is of importance to remark here, that M. Laurent insisted on the identity of the function of the chlorine with that of the hydrogen in bodies formed by substitution long before it had been positively established by experience.

It will not be my object now to write the history of the theory which occupies us; when experience shall have sounded all the parts in succession, it will be useful to go into the discussion of the *à priori* ideas which may have often predicted the results.

Thus putting aside every historical question, and stopping only at facts, at the experiments which have served as a basis to my own convictions, in a word, consulting only my personal impressions, I must say, that the first results in which I believed I could recognise in a decisive manner the elements of a view arrested by this subject, are those which organic chemistry owes to M. Malaguti. In fact, we know that this skilful observer has recognised, that æther, whether free or combined, may always lose two equivalents of hydrogen and gain two equivalents of chlorine, without any of its essential chemical characters undergoing alteration; for its power of combination remains exactly the same; chloridated æther then is still æther.

My conviction became complete, when I was able to recognise the precise nature of chloracetic acid, and when I saw chlorine take the place of all the hydrogen of the acetic acid, without modifying its capacity of saturation, without in any way altering what I term its *fundamental* properties; chloridated acetic acid then is still acetic acid.

It is by setting out from these two facts, it is by adding those which M. Regnault had himself observed in the action of chlorine on the *liqueur des Hollandais*, that I have tried to show that there exist, in organic chemistry, types capable of undergoing, without being destroyed, the most singular transformations as to the nature of their elements.

More recently M. Regnault, in the memoir on the æthers which I have already quoted, giving a still greater extension to those views, considered the bodies formed by substitution as belonging to one mechanical system. We may wait with confidence for the developments which he promises to give to these first views.

[To be continued.]

LXXVII. *On an apparent Inversion of Perspective in viewing Objects with a Telescope. By* James D. Forbes, *Esq., F.R.S., Professor of Natural Philosophy in the University of Edinburgh.*[*]

IN October last, Sir John Robison directed my attention to a curious anomaly in the apparent perspective of objects seen through a telescope which had been first mentioned to him by Mr. Whitwell.

It consists in a complete seeming inversion of the true inclination of two horizontal lines towards a vanishing point when seen through an ordinary telescope. The top and bottom lines of a row of windows, for instance, viewed obliquely, seem, within the limit of the field of view of the telescope to converge to a point on the opposite hand of the spectator from that indicated by the common rules of perspective, and by the experience of the naked eye. There is no better exemplification of the fact than by viewing the figure of a common sign-board, not far from the eye and considerably foreshortened, with a common pocket telescope. The letters appear gradually to increase from the nearer towards the more remote part of the inscription.

That the appearance is such as we describe no one will readily admit who does not make the experiment for himself; but once made, the fact appears so certain as to create surprise, that it does not always strike us, and that it has not (so far as I am aware) been mentioned in books on such subjects.

The first time I saw the anomalous appearance in company with Sir John Robison and Mr. W. A. Cadell, the explanation which I am about to state occurred to me as the true one. Not being particularly conversant with the subject of perspective, I contented myself with stating my opinion in writing at the time, and should probably have never recurred to the subject, had I not been lately requested to examine an ingenious paper, in which the *reality* of this distortion was admitted, and an attempt made to account for it by tracing the path of the rays through the telescopic lenses. Conceiving these investigations to be unsatisfactory, I made one or two simple experiments, which satisfied me completely of the accuracy of the view which I had previously taken of the matter, and which I now proceed to state.

The fact to be accounted for is, that a parallelogram, A B C D, or a word composed of letters of equal height, which by common perspective assumes to the naked eye the figure A′ B′ C′ D′, when viewed through an erecting telescope has the inclination of the lines thrown the other way, or the surface then resembles the figures A″ B″ C″ D″; at all events no one will hesitate to affirm that the letters really the most distant seem to be larger than the nearer ones *.

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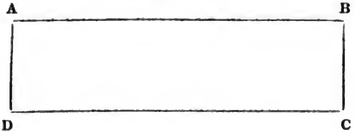
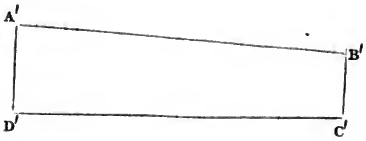
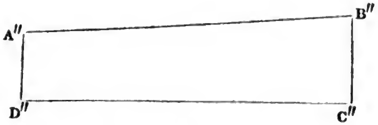
[*] Communicated by the Author.







That this singular effect is a mere *optical illusion* I never doubted: and I recently ascertained the fact by measuring roughly with a micrometer the apparent angles under which A″ D″ and B″ C″ are respectively seen; the former was always found to exceed the latter, in other words the telescopic image is *really* convergent in the same direction with the unmagnified one, though the imagination in this case gives so completely the lie to the senses, that even when persuaded of the deception, and with the invariable standard of the micrometer divisions before our eyes, it is impossible to relinquish the preconceived idea.

The cause to which I assign this effect will appear a natural one to persons who are aware of the unperceived and momentary train of reasoning by which we arrive at conclusions apparently almost intuitive.

In every case in which the illusion now described is observed, the spectator has previously determined by his eye the real position of the plane of the object towards which he directs his telecope: and when he views that object with a magnifying power of two, he believes himself to be looking at an object twice as large in the same plane as before; or else (what comes to the same thing) the same object as before brought twice as near to him, but moved parallel to itself. In either of these cases, the vanishing point ought to remain exactly the same for the enlarged as for the original object. If $a\ b\ c\ d$ be a board 4 feet long and 1 foot high, the eye expects to see through the telescope magnifying twice, a figure similar to that which a board 8 feet long and 2 high would present in the same situation, that is, a figure $a'\ b'\ c'\ d$ of which the upper and lower lines converge to the same point V as before; ut the eye *really* sees through the instrument a merely magnified image of $a\ b\ c\ d$, namely $a'\ \beta\ c'\ d$, in which $a'\ \beta$ is parallel to $a\ b$, and consequently the vanishing point V is thrown twice as far off. What must the mind, reasoning through the information lent by the eye, infer respecting this enlarged object? One of two things. *Either* that the signboard so seen is really not a parallelogram, but has its further extremity $b'\ c'$ higher than the nearer one; *or else*, that the board is a true parallelogram, but that the plane in which it lies is more nearly perpendicular to the line joining the eye and the object, a plane in short which will give to horizontal lines a vanishing point as far beyond V as that is from d.

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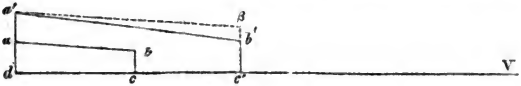
* It is scarcely necessary to observe that this anomalous appearance is wholly independent of the position of the image in the field of view, and is therefore independent of the common errors of aberration.



The former is the case when we look at an object to which we direct a telescope after having mentally formed an estimate of its position; the latter, or an erroneous estimate of a plane of the object, occurs when a person looks suddenly through a telescope previously pointed in an unknown direction.

I am not sufficiently conversant with works on perspective to be aware whether such a circumstance has before been noticed, but it was new to those whom I have had occasion to consult.

At all events it is very singular that it should have remained so long generally unknown that all objects (generally speaking) are seen through a telescope in *false perspective.*

The general principle may be thus stated in a single sentence. By common perspective, all parallel lines in a single plane, or in any plane parallel to that, have a single vanishing point; but the act of magnifying increases the distance of the vanishing point in the same proportion as it does the apparent dimensions of the object; consequently the magnified object is not seen in true perspective for its own plane.

Edinburgh, March 13, 1840. JAMES D. FORBES.

Postscript.—A casual circumstance brought to my recollection a few days ago an optical illusion mentioned (if indeed it was not shown) to me some years since by Sir David Brewster, the nature of which I could not perfectly recollect. Having applied to Sir David Brewster, he obligingly referred me to the Edinburgh Journal of Science * for a notice of it, when it proved, as I expected, to be referable to the principle I have just been applying.

"A field," says Sir David, "may be so situated, that" (from the perspective of the furrows or drills upon its surface) "when seen through the telescope it appears like a perpendicular or *vertical wall of earth*. This phænomenon we have often seen in directing a telescope to a field above Melrose Abbey on the northern acclivity of the north-west Eildon Hill. This field is capable of being ploughed in the direction of its greatest declivity; but when it is viewed through a telescope, the slope is such that *the furrows do not appear to converge*, and the eye cannot readily perceive any difference between the breadth of the furrows at the remote end of the field and their breadth at the near end. The observer therefore immediately concludes that the field must be nearly in a vertical plane rising in front of him. This deception is a very remarkable one, and produces a singular effect on the mind when the field is covered with a crop, and when crows, &c. light upon it."

A more perfect illustration of the *second form* of the optical illusion which I have described could not be desired. Every one knows how imperfectly the eye estimates the acclivity of a plane in full view. The parallelism of the ridges is tacitly assumed, and as their apparent convergence diminishes exactly in proportion as the magnifying power of the telescope increases, the mind is forced to the conclusion that the plane is more nearly perpendicular to the line joining the eye and any point of it, than it really is.

Hence it appears that Sir David Brewster noticed and published fourteen years ago one case of the curious observation of Mr. Whitwell.

March 17, 1840.

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* First Series, iii. p. 88.

LXXVIII.—*On the Heat of Vapours and on Astronomical Refractions.* *By* John William Lubbock, *Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from p. 441.]

ON THE PRESSURE OF STEAM.

THE most accurate and extensive experiments by which the accuracy of these relations can be tested are those which have been made upon the conditions of steam. The following are the experiments of Arago and Dulong, as recorded in tom. x. of the *Mémoires de l'Institut*, p. 231; together with the temperatures calculated by the best empirical formulæ*.

Nos. des observations.	Elasticité en mètres de mercure à 0°.	Elasticité en atmosph. de 0m·76.	Température observée.	Température calculée par la formule de Tredgold.	Température calculée par la formule de Roche cöeff. moyen.	Température calculée par la formule de Coriolis.	Température calculée par la formule adoptée.
			Cent.	Cent.	Cent.	Cent.	Cent.
1	1·62916	2·14	123°·7	123°·54	123°·58	123°·45	122°·97
3	2·1816	2·8705	133·3	133·54	133·43	133·34	132·9
5	3·4759	4·5735	149·7	150·39	150·23	150·3	149·77
8	4·9383	6·4977	163·4	164·06	163·9	164·1	163·47
9	5·6054	7·3755	168·5	169·07	169·09	169·3	168·7
15	8·840	11·632	188·5	188·44	188·63	189·02	188·6
21	13·061	17·185	206·8	206·15	207·04	207·43	207·2
22	13·137	17·285	207·4	206·3	206·94	207·68	207·5
25	14·0634	18·504	210·5	209·55	210·3	211·06	210·8
28	16·3816	21·555	218·4	216·29	218·01	218·66	218·5
30	18·1894	23·934	224·15	222·09	233·4	224·0	224·02

There are reasons which make it probable that in inquiries of this nature the scale of temperature as indicated by the expansion of air is to be preferred, although the difference between the indications of a mercury thermometer with that of air is not considerable.

The following table is given by M. Pouillet (*Elémens de Physique*, vol. i. p. 259) for the centigrade scale:

Températures indiquées par le therm. à mercure, à enveloppe de verre.	Températures indiquées par un therm. à air, et corrigées de la dilatation du verre.	Volumes corréspondans d'une même masse d'air.
− 36°	− 36°	0·8650
0	0	1·0000
100	100	1·3750
150	148·70	1·5576
200	197·05	1·7389
250	245·05	1·9189
300	292·70	2·0976
Ebull. du merc. 360	350·00	2·3125

[* Many of the results stated in the table of the French chemists are absolutely identical with those which had been published by Mr. Philip Taylor in 1822. See his Paper in the Philosophical Magazine, First Series, vol. lx. p. 452., and the accompanying engraved Table of his experimental results.—Edit.]

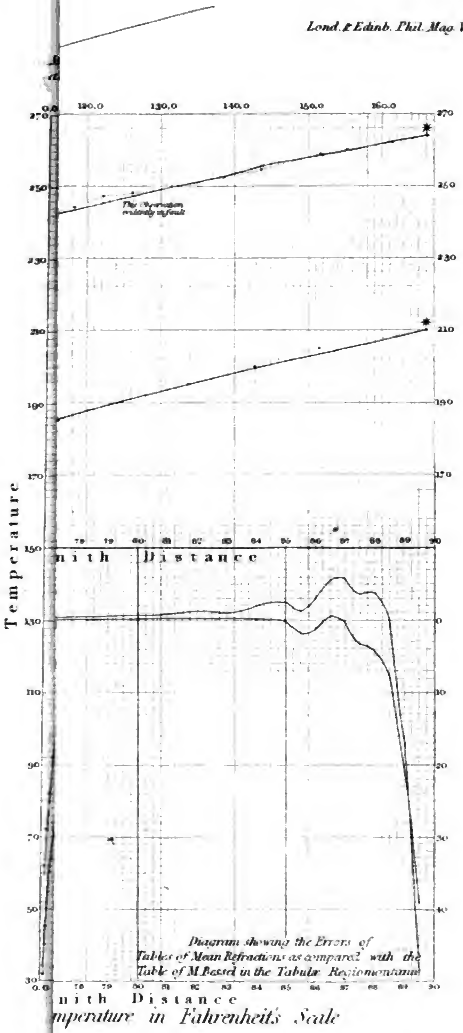

Diagram showing the Errors of Tables of Mean Refractions as compared with the Table of M. Bessel in the Tabulæ Regiomontanæ

From the above I have deduced the following Table for Fahrenheit's scale:

Merc. therm.	Air therm.	Merc. therm.	Air therm.
212°	212°	482°	478°·1
302	299·7	572	558·9
392	386·7	680	662·0

I now proceed to determine for steam the constants γ and E by means of the observations of Dulong and Arago which I have quoted in the preceding page.

For the air thermometer on Fahrenheit's scale the experiments of Dulong and Arago (*Mém. de l'Institut*, vol. x.) give, θ being reckoned in Fahrenheit's scale and from the freezing point of water,

$$p = 1 \qquad \theta = 180 \qquad \frac{1}{\alpha} = 480^\circ$$

$$p' = 11{\cdot}632 \qquad \theta' = 334{\cdot}7 \qquad \frac{1}{\alpha} + \theta' = 814{\cdot}7$$

$$p'' = 23{\cdot}934 \qquad \theta'' = 396{\cdot}4 \qquad \frac{1}{\alpha} + \theta'' = 876{\cdot}4.$$

I find from these observations

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0{\cdot}1140623],$$

the quantity within brackets being the logarithm of the corresponding number; and hence I find

$$\beta = {\cdot}0134^{*} \qquad \gamma = {\cdot}98677 \qquad \frac{1}{\gamma} = 1{\cdot}0134$$

$$E = 1{\cdot}17602 \qquad \log E = {\cdot}0704184 \qquad H = 6{\cdot}6809.$$

The pressure at the boiling point of water (212°) being unity,

$$\frac{1}{\alpha} + \theta = -\frac{[2{\cdot}0651059]}{p^{{\cdot}0134} - 1{\cdot}17602};$$

so that if τ is the number of degrees on Fahrenheit's scale of the air thermometer, and the pressure p be reckoned in atmospheres,

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* This value of β appears to me to be the only one which will satisfy the equation.

$$\tau = -\frac{[2{\cdot}0651059]}{p^{\cdot 0134} - 1{\cdot}17602} - 448^{\circ},$$

and if ϱ be the density of steam, the relative volume

$$\frac{\varrho}{\varrho'} = \frac{p\,\{p^{\cdot 0134} - 1{\cdot}17602\}}{p'\,\{p'^{\cdot 0134} - 1{\cdot}17602\}}.$$

In order to ascertain how far the new expression here given for τ represents the totality of the observations, I have calculated the temperatures corresponding to all the observed pressures in the observations of Arago and Dulong, and the results are exhibited in the following table.

Pressure in atmospheres.	Temperature.				Error of temperature calculated by Lubbock. Fahr.
	Observed.			Calculated.	
	Merc. therm. Cent.	Merc. therm. Fahr.	Air therm. Fahr.	Air therm. Fahr.	
2·1400	123°·7	254°·66	253°·6	252°·8	− °·8
2·8705	133·3	271·94	270·4	270·1	− ·3
4·5735	149·7	301·46	299·2	299·4	+ ·2
6·4977	163·4	326·12	323·0	323·2	+ ·2
7·3755	168·5	335·30	331·9	332·3	+ ·4
11·6320	188·5	371·30	366·7*	366·7	0
17·1850	206·8	404·24	398·6	398·9	+ ·3
17·2850	207·4	405·32	399·5	399·4	− ·1
18·5040	210·5	410·90	404·9	405·3	+ ·4
21·5550	218·4	425·12	418·5	418·8	+ ·3
23·9340	224·15	435·47	428·4*	428·5	+ ·1

The observations marked with an asterisk were employed in determining the constants.

The error of the temperature calculated by the formula adopted by Arago and Dulong corresponding to the first observation is − ·73 cent. or − 1°·3 of Fahr. I have no doubt that the observed temperature is in excess, and the agreement with the rest of the observations is so complete, that within this range of temperature the formula may, I think, be considered as exactly representing the phenomena. The errors of the temperatures, calculated by the various empirical expressions which have been hitherto proposed, are much greater, as may be seen in the table of Dulong and Arago. The following observations are those of Southern, given in p. 172, vol. ii., of Dr. Robison's Mechanical Philosophy.

Pressure.	Temperature.		Error of calculated temp.	Pressure.	Temperature.		Error of calculated temp.
	Observed.	Calculated.			Observed.	Calculated.	
Inch.				Inch.			
·52	62	59°·5	−2°·5	4·68	132	131·4	−°·6
·73	72	69·3	−2·7	6·06	142	141·3	−·7
1·02	82	79·3	−2·7	7·85	152	151·6	−·4
1·42	92	89·9	−2·1	9·99	162	161·7	−·3
1·95	102	100·2	−1·8	12·64	172	171·8	−·2
2·65	112	110·7	−1·3	15·91	182	182·0	0
3·57	122	121·3	− ·7	29·80	212	212·	

The formula deviates slightly from the observations at very low pressures. Dalton says that it is next to impossible to free any liquid entirely from air; of course if any air enter, it unites its force to that of the vapour.—*Manchester Memoirs*, vol. v. p. 570. It must be recollected that according to theory the constants γ and E are the same only as long as the chemical constitution of the vapour remains the same, and they vary for different substances.

With regard to the nature of the accurate expression which connects the pressure with the temperature, opinions have hitherto been various. According to Dr. Robison, Mr. Watt found that water would distil *in vacuo* when of the temperature of 70°, and that in this case the latent heat of the steam appeared to be about 100°; and some other experiments made him suppose that the sum of the sensible and latent heats is a constant quantity. This, Dr. Robison says, is a curious and not improbable circumstance. Southern, on the contrary, concluded from experiments on the latent heat of steam at high temperatures that the *latent heat* is a constant quantity, instead of the latent heat + sensible heat being so. M. de Pambour, in speaking of Southern's view, says, "Cette opinion a paru plus rationelle à quelques auteurs, mais le première nous semble mise hors de doute par les observations que nous allons rapporter." It appears to me by no means clear that Watt entertained the opinion here attributed to him, for in a note in the Appendix to Sir David Brewster's edition of Robison's Mechanical Philosophy, vol. ii. p. 167, he professes to agree in the opinion there delivered by Southern. In p. 166 Southern records three experiments, from which he obtained 1171°, 1212°, and 1245°, for the sums of the latent + sensible heat corresponding to the temperatures or sensible heat 229°, 270°, 295°. If we take the two extreme observations, we find a difference in the sum of the latent + sensible heat of 74 degrees, corresponding to a difference in the sensible heat of 66 degrees.

If the conditions under which Laplace obtained the equation

$$V = A + B\frac{p}{\varrho}^{\frac{1}{\gamma}}$$

are admitted, the value of E different from zero shows that the absolute heat is not constant; but the preceding theory does not appear to me to furnish the means of determining the value of D, and hence of deciding with certainty whether the latent heat is constant, and whether in augmentations of heat the sensible heat only varies. I think there can be little doubt that the conditions assumed by Laplace actually obtain, and that the hypothesis attributed to Watt[*] must be abandoned. The experiments recorded by Mr. Parkes in the 3rd volume of the Transactions of the Society of Civil Engineers, p. 71, which show that the quantity of fuel required to evaporate a given weight of water is nearly the same whatever be the pressure of the steam, do not seem to me to authorize a different conclusion. For this is precisely what would take place if the *latent* heat be constant, and if the quantity of fuel required to generate the *latent* greatly exceed that required to generate the concomitant *sensible* heat.

The quantity γ has never before been determined for steam[†] or for the vapour of any liquid, properly so called, as far as I am aware. It may excite surprise that the value of γ should come out less than unity. Both Poisson and Dulong assert that it is evident that γ must surpass unity, but the reason which they assign appears to me inconclusive.

[To be continued.]

LXXIX. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron. By* Dr. C. SCHAFHAEUTL, *of Munich.*

[Continued from p. 434.]

THE brown residuums of all white irons, when boiled with hydrochloric acid before ignition, parted with their iron with extreme difficulty. In one trial after boiling the mixture in a bottle whose neck was shut up with a capillary tube; first no apparent change took place, and only hydrochloric acid escaped; after boiling an hour the contents of the bottle began to become thickish, a disagreeably smelling gas escaped, which when ignited burned with a small but intensely blue-coloured flame.

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[*] Mr. Sharpe has maintained the same opinion in the 2nd vol. of the Manchester Memoirs. See Dr. Thomson's Outline of Heat and Elasticity, p. 198.

[†] "Quant à la valeur de γ, elle nous est jusqu'à present tout-à-fait inconnue."—Poisson, *Méc.*, tom. ii. p. 652.

After boiling for more than twenty-four hours, 20·4 grains left a dark-green residuum, weighing only 2·64 grains, and this residuum burnt in a dried stream of air in a horizontal glass tube, blackened with the first action of heat, and drops of water collected on the upper part of the tube. After the water was driven into its receiver, brown spots remained on the glass; the drops of water collected smelt as if impregnated with tobacco-smoke, and the gas which escaped from the apparatus for collecting carbonic acid had the same smell.

I collected carbonic acid 0·954: water 0·238: and there remained white silicon = 2·35.

For 0·954 carbonic acid is = 0·2637810 carbon; and 0·238 water is = 0·0263942 hydrogen. Sum of carbon and hydrogen 0·2901752. This last amount is only less 0·1998848 grain and to account for the loss of carbon, hydrogen and azote which passed through the bulb glass containing the caustic ley in the form of the before-mentioned nicotianic gas.

By boiling the residuum of white iron for a short time only in hydrochloric acid, very little iron is dissolved, and the remainder assumes a greyish colour, and becomes white or grey after the first ignition without first glowing like tinder.

By boiling it with caustic alkalis only traces of silica are extracted, whilst a species of brownish humus was dissolved, which I found never to exceed in amount 2 per cent.; the silica therefore must be contained in a chemical combination with carbon, azote and iron.

A short ignition of the residuum on the contrary is sufficient to make the greatest part of the iron soluble in acids. If it is only heated, till it begins of itself to glow, the iron is dissolved by acids, and the evolution of hydrogen shews that the iron must be contained in a metallic state in the residuum. When, on the contrary, this remainder is heated as long as it absorbs oxygen, and then treated with acids, no evolution of gas takes place, and a mixture of protoxide and peroxide of iron is dissolved.

As soon as this residuum begins to glow, carbonic acid, azote and a little hydrogen are invariably evolved, which shows the intimate connexion between the solubility of the iron and the carbon, azote and hydrogen in the residuums.

Not only the evolution of hydrogen gas, but the powerful action on the magnet, proves, that the iron in these residuums must be contained in the metallic state. Even were we to consider it as protoxide, the great increase of weight, that is, the absorption of oxygen, which takes place during ignition, could not be accounted for. But as the iron, notwithstanding its metallic state, is not soluble in acids before ignition, we must assume that the metallic iron is in combination with carbon, azote and hydrogen, or perhaps in a state forming a sort of cyanet of iron.

The relation of the quantity of constituents of the brown residuum of the iron is so stable and so accurately in atomic proportions, that we may safely conclude the quantity of carbon from the amount of oxygen absorbed.

If we denote the increase of weight by letter (a), the oxygen of silicon as (s), the oxygen of iron by (f), and the carbon by (x), we obtain always the following simple formula, particularly when we consider that the iron is oxidized according to the above-mentioned formula $\dot{\mathrm{F}}^{3}\dddot{\mathrm{F}}$:

$$x = s + f - a.$$

In some French irons I found the relations of carburets of iron to the siliciuret approach

$$\mathrm{S} \quad \mathrm{C}^{4}.$$
$$\mathrm{F}^{4} \quad \mathrm{C}^{4}.$$

If we further consider, that the white silica only remains when the iron residuum has been exposed to a number of ignitions, and that after only a few ignitions it remains always dark black, soluble neither in alkalis nor acids; we may find therein likewise a proof, that silicon is combined chemically with carbon. Further, the remainder of the residuum five times ignited had gained 0·028 grains, after the black-coloured scales had disappeared; this increase in weight can only be ascribed to the oxidation of those black scales, which must have been a metal whose oxide was white. Further, if we consider that the remainder of the residuum only once ignited, after having changed its deep-black colour to white, and after emitting a great quantity of smoke, had only lost 0·0511 grains; we may take all these circumstances as a certain proof that the silica obtained from these residuums must have been contained in the metallic state, and in combination with carbon, which is likewise to be suspected from its emitting bright red sparks when thrown into the flame of a candle.

Having now considered partially by induction the relative combination of the ingredients of the residuums of white irons, we proceed to ascertain by experiment the quantity of the volatile bodies in the residuums.

60 grains of the same Maesteg white iron, freed from all fine powder, and treated in a retort with 4 oz. of hydrochloric acid of sp. gr. 1·103, generated no spongy foam whatever; the liquid in the retort always remained transparent, and the action of the acid had ceased the next day.

13·711 grains of this residuum, corresponding with 35 grains of iron, gave by combustion in Liebig's and Dumas's apparatus,

Carbon	1·22200
Hydrogen . . .	0·03684
Azote	0·264

The residuum of the same iron, treated with hydochloric acid, sp. gr. 1·14, left

Carbon . . .	0·3862
Hydrogen . .	0·0297
Azote	0·2833

The actual quantity of azote and the other contituent calculated by direct chemical analysis, is shown in the following table:—

	For 35 grains	in per cent.
Silicon . . .	0·353 =	1·00867
Aluminum . .	0·030 =	0·08571
Antimony . .	0·559 =	1·59710
Phosphorus . .	0·030 =	0·08553
Sulphur . . .	0·112 =	0·32018
Azote	0·268 =	0·76371
Carbon . . .	1·505 =	4·30000
Iron	32·033 =	91·52282
Loss	00·110 =	00·31428
	35·000 =	100·00000

Iron of the works of Creuzot, departement de Saone et de Loire. Specific gravity = 7·536; the colour of the fracture *dead grey*. Treated as usual with hydrochloric acid of specific gravity 1·103. The evolution of hydrogen gas continued for fourteen days. The escaping hydrogen had an intolerable smell, somewhat resembling the smell of *selenium*. The remainder in the retort had retained the shape of the fragments of the iron previous to solution. When washed upon a filter, as soon as boiling water was poured upon the partially dry fragments, the air contained between the pores of the fragments escaped with the same hissing noise for a considerable time, as hydrogen escapes when hydrochloric acid is poured upon iron. In 35 grains of this iron 0·3213 grains of sulphur were contained, and the remainder was = 14·880 of a yellowish-brown colour. Concentrated hydrochloric acid when cold had no further action upon it; when boiled with the same acid for ten minutes it had lost 4·586 grains of iron. The remainder = 10·294, heated in a platinum crucible, did not glow or take fire spontaneously; but brought to a red heat, it at once attained a lively white appearance of ignition, which immediately subsided into its original state, viz. a dull red heat. After cooling it had assumed a light-gray colour, arising from the intermixture of black with white spots, some of the white spots had aggregated into rather large reddish-white lumps. It gained in weight = 0·795 and was very powerfully attracted by the magnet. I boiled it again with concentrated hydrochloric acid and obtained a residuum = 4·217 grains. Heated on the lid of a platinum crucible it began to ignite spontaneously, and had afterwards lost 0·124 grains. Its colour now assumed a lighter gray appearance, and it looked more woolly and voluminous.

It was still attracted by the magnet. The black spots must therefore be a compound of carbon, iron and silicon, which could be destroyed neither by fire nor by acids.

I weighed again 14·880 grains of the above-mentioned residuum, and boiled it with hydrochloric acid for nearly thirty minutes. The remainder of the powder weighed now only 1·68 grains, was of a dirty yellowish-brown colour, and still retained the well-known smell of hydrogen gas. When heated it did not ignite spontaneously. It was apparently increasing in bulk, and began to assume a black colour, throwing up some dust on the sides of the crucible. After the crucible had acquired a white heat, the black colour rapidly disappeared, and a beautiful lemon-coloured powder remained, which after cooling, changed to a greenish yellow, and weighed 1·12 grains. It consisted of 0·687 silica, and oxide of chromium and iron = 0·526. I mention these two experiments, to show the great difference betwixt the residuums of the two remainders, boiled for a shorter or longer time. The remainder loses its spontaneous power of ignition the longer it is boiled in hydrochloric acid, and the more iron that is extracted from it, notwithstanding the powder to a certain degree always retains the property of spontaneously igniting on the application of a certain heat, but the degree of temperature sufficient to cause this ignition rises higher the longer it is boiled.

On being boiled for a short time only, and afterwards slowly ignited, the iron and perhaps silicon remain in a state of carburet, which is unalterable either by heat or acids, and is powerfully attracted by the magnet. I mention here the curious circumstance, that when, in the above-mentioned remainder, which we found = 1·68, we assume, instead of metallic iron, peroxide of iron, the loss is just equal to the quantity of carbon contained in 35 grains of iron.

Oxide of iron . .	0·583
Silicon	0·281
Loss	0·866
	1·680

I now proceed to show the results obtained when the abovementioned residuum is treated, instead of acids, with caustic leys.

14·88 grains of this residuum, powdered and boiled with strong caustic potash ley for ten minutes, lost 0·261 grains. When the ley was poured over the powder it foamed like soap suds. A little hydrogen was disengaged, emitting (during the whole boiling,) an odour partly resembling that of heated asphaltum and partly that of fresh peat. Shortly after the boiling commenced, the powder appeared to be swimming in the clear liquid like distinct *black scales*, closely resembling so-called graphite scales, separated from gray cast iron; but when dried on the filter, those scales resumed their former yellowish-brown colour, and looked again like powder, without any trace of scales remaining; which again confirms my former assertion, that all *residuums of iron dissolved in hydrochloric acid*, are separated in scales.

14·619 grains of this boiled residuum, after being carefully heated, became dark-red brown, and had increased in weight	0·000
After the second ignition it became light red, and had increased in weight	1·269
After the third ignition it had increased in weight	0·608
After the fourth ignition it had increased in weight	0·296
After the fifth ignition it had increased in weight	0·222
	2·395

After being boiled in hydrochloric acid, only 0·6901 grains of a light gray colour remained. After a lively ignition it weighed only 0·4006 grains, which consisted of a distinct mixture of white and black grains.

Another residuum of 14·88 grains, boiled with caustic potash ley for more than thirty minutes, lost 0·325 grains. The remaining 14·555 grains, after being ignited had assumed

a light red colour, and had increased	1·871
Second time	0·486
Third time	0·249
Fourth time	0·000
	0·606

During the process of boiling, the leys had extracted a mixture consisting of carbon, hydrogen, azote, and oxygen, with a very minute portion of silica. 35 grains of another specimen of Creuzot iron, whose specific gravity was = 7·305, treated in the usual way with hydrochloric acid, left a residuum = 12·005. When heated carefully in a platinum crucible, it began to glow, and being quickly removed from the fire, after cooling it became black, and had gained . . . = 0·0000

After the second ignition it was observed to glow from within, and after cooling was found to be red within, covered with a black crust. It had gained	0·4500
After the third heating, a speck like a pin's head only began to glow, and the whole mass only became ignited after the sides of the crucible were red hot, and had gained	0·8460
After the fourth ignition, (and it had now become light red)	0·8108
After the fifth ignition	0·2100
,, sixth ,, ...	0·1624
,, seventh ,, ...	0·1440
,, eighth ,, ...	0·0180
,, ninth ,, ...	0·0540
,, tenth ,, ...	0·0000
	2·6949

After being boiled with hydrochloric acid, the usual light gray powder remained = 1·603. When ignited, it lost 0·3500, and a reddish-white powder was left amounting to 1·253 grains. The hydrochloric acid extracted from it peroxide of iron, contaminated with a little chromium = 0·5176, and left silica = 0·7353. The hydrochloric acid held in solution oxide of iron and chromium, and the whole remainder consisted therefore of

Iron combined with silica .	0·3589
Iron and manganese . . .	9·3703
Phosphorus	0·9450
Silicon	0·3532
Aluminum	0·0212
Azote.	0·2521
Carbon	0·6685
Loss	0·0358
	12·0050

The hydrochloric acid in the retort had dissolved

Iron	21·9549
Chromium	0·4837
Phosphorus	0·1697

The sulphuret of lead yielded

Sulphur	0·3867
	35·0000

Or to calculate it centesimally,

Silicon	1·0090
Aluminum	0·0606
Manganese	0·2412
Arsenic	0·0000
Antimony	0·0000
Tin	0·0000
Chromium	1·3820
Phosphorus	3·1848
Sulphur	1·1050
Azote	0·7202
Carbon	1·9100
Iron	62·7282 27·5566
Loss	00·1023
	100·0000

I only state here briefly, that in order to separate chromium, iron, phosphorus and alumina, I proceeded in the usual way,—by melting the first-dissolved and evaporated quantity of iron to be analysed with carbonate of soda in a platinum crucible, and dissolving the fused mass in distilled water. The solution contained the phosphoric acid, the chromium, and a little alumina and silica; the remainder was oxide of iron, silica and alumina, and a little alkali.

The solution kept quiet for some time in a dish covered with paper, in order to allow the manganese to separate, was neutralized with nitric acid and evaporated to dryness. After having been moistened by nitric acid and dissolved again, the silica was left behind. The filtered liquor, neutralized with ammonia, let fall a basic phosphate of alumina. From the again filtered liquor, acidulated with acetic acid, acetate of lead precipitated phosphoric acid, sulphuric acid and chromium in combination with oxide of lead. Hydrochloric acid and alcohol separated the chromium from the precipitate, which consisted now only of phosphate of lead, sulphate of lead, and chloride of lead. From the solution, freed by means of sulphuretted hydrogen from all traces of lead, ammonia threw down the chromium in the state of oxide.

We here see a new feature in white iron; the carbon of white coke-iron is always combined with a constituent hitherto totally overlooked, that is, *azote*. No white coke-iron which I analysed was free from it, nor any gray iron free from aluminum.

The best *English cast steel* as well as *wootz* and *malleable iron*, possesses the same property of being combined with *azote*. Now as malleable iron produced from gray pig iron contains nevertheless a quantity of azote, it must have been taken up during the process of puddling.

Silicon is also a necessary ingredient in all sorts of cast iron; *no carburation* of iron would take place without its presence, and the iron appears only to have dissolved the carburet of silicon. Silicon shares this property of carbonizing iron with manganese, and both are in some degree equivalent to each other, so that in certain cases the action of manganese is substituted for that of silicon.

Such iron, in which manganese is substituted partially for silica, has that peculiar property of producing, in a certain species of refining fire, *steel* instead of malleable iron; it is always obtained from decomposed spathic iron ores; its fracture is silver white, and possessing a highly large laminous crystalline structure, and is the well-known *spiegeleisen* of the Germans.

To give the reader an idea of the above-mentioned curious fact, I refer to the analysis of four sorts of white cast iron, made by the celebrated chemist Gay Lussac. The first specimen is white, very similar to the other three. It produces excellent malleable iron, but very inferior steel in the direct way.

Iron from Champagne.		De L'Iser.	Siegen	Coblentz
			in Germany.	
Carbon	2·324	2·636	2·690	2·441
Silicon	0·840	0·260	0·230	0·230
Phosphorus ...	0·703	0·820	0·162	0·185
Manganese......	traces	2·137	2·390	2·490
Iron	96·133	94·687	94·328	94·654

We see in the first specimen of the cast iron of Champagne, which produces excellent malleable iron, 0·840 of silicon with only a trace of manganese. In the second specimen, on the contrary, only 0·260 per cent. silicon are to be found, but, instead, manganese amounting to 2·137 per cent.

If we consider the quantity of silicon in the first example as a standard, the three other specimens contain only 0·26, and therefore 0·58 silicon less; now, 0·58 silicon wants 0·627 parts of oxygen to become oxidized to silica, but the quantity of oxygen which is necessary to convert the 2·137 of manganese into protoxide is 0·615; in fact, almost the same as for the silicon of the weight 0·58 before-mentioned.

This quantity of manganese gives to such iron the property of producing *steel* immediately in the refining fire, instead of malleable iron; and the explanation of this enigmatical property is given by the following facts, of which I shall treat more largely in another place.

The quantities of manganese and silicon are as 2·54 to 1; now, if we follow the process during the transition of this cast iron into steel, both those bodies have to burn away with the same quantity of carbon. By chemical analysis we find in the first two-thirds of the process, the quantity of manganese very rapidly diminishing in comparison with the carbon, leaving the silicon quite untouched. On the contrary, when manganese is replaced by silicon, this last is oxidized so very rapidly, as it is in relation to manganese only like 1 : 2·54, so that the greatest part of the silicon is oxidized when more carbon is present than is necessary to prevent the iron from fusion at a white heat. When the carbon is so far diminished, that the iron begins to become sufficiently solid for the hammer—all the silicon is oxidized, which gives, as we shall soon perceive, a hardness and tenacity to the steel.

Before we take leave of these three specimens of cast iron, I must return to the solution of acetate of lead of specimen A, in which the sulphuretted hydrogen had been collected.

The precipitated sulphuret of lead, as before mentioned, was not crystallized in scales; on the contrary, it had a viscous dark brown appearance; and after some days standing, I found it to contain rhomboidal columns of white crystals deposited on the bottom of the bottles in needle-like aggregations, and in a vertical position on the sides. On first inspection I considered them as crystallized acetate of lead, but their difficult solubility, however, in distilled water, induced me to examine them more closely; and by trying them with the blowpipe, the rising copious white fumes covering the charcoal immediately showed the presence of antimony, and the well-known yellow ring around the crystals on the charcoal plainly signified the presence of lead. The crystals themselves, treated with concentrated sulphuric acid, disengaged acetic acid, and the crystals therefore consisted of antimony, lead, and acetic acid.

The above-mentioned treatment of 60 grains of white iron with hydrochloric acid of the specific gravity 1·103, left 2·92 grains of sulphuret of lead mixed with antimony and acetate of lead, and the precipitate contained 0·833 antimony.

It is remarkable, that not only almost all the antimony escaped with the hydrogen, but that the antimony was likewise deposited in the acetate of lead, being also oxidized and converted into an acetate, and that no trace of antimony followed the hydrogen after it had passed the second bottle.

[To be continued.]

LXXX.—*Notices respecting New Books.*

Solutions of the principal Questions of Dr. Hutton's Course of Mathematics; forming a general Key to that work, designed for the use of Tutors and private Students. By THOMAS STEPHENS DAVIES, F.R.S., *Lond. and Edinb. Royal Military Academy, Woolwich.*

THE work of Dr. Hutton is too well known to require any detailed description in our pages. Drawn up half a century ago for the use of the gentlemen cadets of the Royal Military Academy, its object was to remove the inconvenience which had been felt to arise from the use of detached parts of a multitude of works on different branches of mathematical science. Compiled, too, for the use of boys who entered the institution at the age of fourteen, and whose periods of study varied from two to four years,—for boys who beside mathematics were instructed in their applications to mechanical and physical science,—the course was necessarily rendered a brief one.

Those of our readers who have paid attention to the history of mathematical science in this country, do not need to be told that, at the time of its first publication, it was by far the best treatise on the subject existing in our language. During the time which has elapsed, many improvements have been introduced into it by Dr. Gregory, to keep pace with the progress of science; but in the last edition the alterations were much more extensive and important than any which had preceded them. The number of new questions which were introduced into that edition have rendered it one of the most valuable books of examples in any language; and many subjects were treated there which for the first time found their way into an elementary book,—amongst which may be particularly specified Horner's general method of solving algebraical equations with numerical coefficients.

The work before us contains either the entire or indicated methods of solution of all the questions in this edition where it appeared likely that there could arise the least difficulty. We have always thought that such works were useful to teachers, and to private students of honest purpose; but we saw a drawback to this utility in the probability of their falling into the hands of pupils themselves. In the present case, however, we do not anticipate this evil; for the work cannot be used for the purpose of deception when any master exercises common vigilance, inasmuch as no student can proceed through the writing and especially filling the occasional blank steps without at least *understanding* the solution, and seeing its application to collateral problems. To a mere boy of ordinary capacity this is the utmost that can be accomplished even by the most diligent teacher: and hence, in this case, the labour of the master will be diminished and the acquirements of the pupil still secured.

To both we consider this work invaluable. We were much struck with the general elegance of the solutions, and much pleased with the systematic working formulæ adapted to numerical application. Such examples are calculated to improve the mathematical taste of teachers themselves, whilst they will form in the *bonâ fide* student a regard for good order, a judicious discrimination between different methods of operation, that cannot fail to be of the utmost future use to him. The almost universal use of Hutton's Course both in private schools and by private students, therefore, will be rendered still more conducive to the general diffusion of good mathematical taste, by the publication of the present appendage to it.

We have not space to enter into an analysis of the contents of the 551 pages contained in Mr. Davies's work—filled, as it is, with original views on every branch of elementary mathematics. The author has even touched in various places on the matter of scientific history, and we value this innovation as much as any. How preposterous is it to despise the labours of our predecessors in whatever field of literature or science we are labouring! As this portion of Mr. Davies's work is more immediately open to criticism than any other, perhaps it will not be considered irrelevant if we devote a few lines to its consideration.

The author's observations on the middle-age abacus are sensible and valuable. The change between the manual abacus and the membranaceal tablets is, indeed, easily conceived; and in all probability was transferred in that manner to the Arabian system of computation. "Though this mode of notation," as Mr. Davies observes, "may never have been necessary, and very rarely employed by chroniclers and other persons merely literary, it would be of extreme value in *the performance of computations*. Amongst these expedition would be of great consequence, and this would often be facilitated by writing the symbols in ruled columns, instead of placing the dice upon the abacus. This, again, would lead to running the several component letters of any one number into a single and continuous figure, which would represent the number." We must, however, observe, in all this, that we prefer *facts* to wholesale conjecture, however pretty and ingenious this last may be.

The derivations of the signs + and — as given at pp. 11, 13, are, we think, very improbable, and show that the late Professor Rigaud and Mr. Davies, who have worked together on this subject, are not well versed in ancient handwriting. On a point of this nature, where the subjects in dispute were introduced five centuries ago, it is necessary, we humbly submit, to take into consideration the mode of writing at the period,—if, indeed, the symbols are not altogether arbitrary,—and not reason on the *et* and the *P* of the present day, as if our rough-writing were the same with that of the Italians in the fifteenth century. We are sorry to speak disparagingly of what we infer to be a favourite theory with our author, but its very improbability is quite sufficient to bring with it a condemnation, and perhaps might eventually have done so from less merciful critics.

We beg our readers to study attentively the various remarks tending to the completion of the new method of solving numerical equations. The method of synthetic division, which forms the basis of that process, is here developed at length, and traced to the common method as its source (pp. 93–96); whereas Mr. Horner's investigation, though perfectly valid, rested on considerations too elevated for the supposed progress of the student. We were also much struck with the very simple and perfectly general method of investigating the criteria of DeGua and Budon for the reality of the roots in any given interval.

The new and extremely elegant solution of the singular problem, known as Colonel Titus's problem, given by Mr. Davies, well deserves attention. A singular fact with respect to the history of this problem was discovered by Mr. Halliwell in a MS. in the British Museum*. It appears that Harriot was the originator of it, and that it was proposed as early as the year 1649 by William Brereton, who was one of Harriot's pupils.

Here we close our brief memoranda on a work which is certainly one of the most valuable in its kind that has ever appeared. The style in which it is written, and its entire conception, bespeaks no ordinary mind; and though, from the author's reputation, we should have expected a work superior to those generally published with the same professed objects, yet our expectations did not reach to the possibility of so varied a mass of information being couched under such a form as that to which the author was tied down by the nature of his undertaking. We only regret that his limits prevented him from continuing his original plan of giving varied solutions and entering into the history of the different branches of elementary mathematics. We venture to hope that at some future period this latter part of the plan may be accomplished in another form.

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 7, 1840.—A PAPER was read, entitled "Researches in Embryology, Third Series: a Contribution to the Physiology of Cells." By Martin Barry, M.D., F.R.S., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh.

In the second series of these researches, the author had traced certain changes in the mammiferous ovum consequent on fecundation. The object of his present communication is to describe their further appearances obtained by the application of higher magnifying powers; and to make known a remarkable process of development thus discovered. In order to obtain more exact results, his observations were still made on the same animal as before, namely, the rabbit, in the expectation that, if his labours were successful, it would be comparatively easy to trace the changes in other mammals. By pursuing the method of obtaining and preserving ova from the Fallopian tube which he recommended in his last paper, he has been enabled to find and examine 137 more of these delicate objects; and has thus had ample opportunity of confirming the principal facts therein stated. He has now procured in all 230 ova from the Fallopian tube. But being aware that repeated observations alone do not suffice in researches of this nature, unless extended to the very earliest stages, he again specially directed his attention to the ovum while it is still within the ovary, with a view to discover its state at the moment of fecundation, as well as immediately before and after that event.

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* Life of Sir Samuel Morland, p. 28.

The almost universal supposition, that the Purkinjian or germinal vesicle is the essential portion of the ovum, has been realized in these investigations; but in a manner not anticipated by any of the numerous conjectures which have been published. The germinal vesicle becomes filled with cells, and these again become filled with the foundations of other cells; so that the vesicle is thus rendered almost opake. The mode in which this change takes place is the following, and it is one which, if confirmed by future observation, must modify the views recently advanced on the mode of origin, the nature, the properties, and the destination of the nucleus in the physiology of cells. It is known that the germinal spot presents, in some instances, a dark point in its centre. The author finds that such a point is invariably present at a certain period; that it enlarges, and is then found to contain a cavity filled with fluid, which is exceedingly pellucid. The outer portion of the spot resolves itself into cells; and the foundations of other cells come into view in its interior, arranged in layers around the central cavity; the outer layers being pushed forth by the continual origin of new cells in the interior. The latter commence as dark globules in the pellucid fluid of the central cavity. Every other nucleus met with in these researches has seemed to be the seat of changes essentially the same. The appearance of the central portion of the nucleus is, from the above process, continually varying; and the author believes that the nature of the nucleolus of Schleiden is to be thus explained. The germinal vesicle, enlarged and flattened, becomes filled with the objects arising from the changes in its spot; and the interior of each of the objects filling it, into which the eye can penetrate, presents a repetition of the process above described. The central portion of the altered spot, with its pellucid cavity, remains at that part of the germinal vesicle which is directed towards the surface of the ovum, and towards the surface of the ovary. At the corresponding part, the thick transparent membrane of the ovum in some instances appears to have become attenuated, in others also cleft. Subsequently, the central portion of the altered spot passes to the centre of the germinal vesicle; the germinal vesicle, regaining its spherical form, returns to the centre of the ovum, and a fissure in the thick transparent membrane is no longer seen. From these successive changes it may be inferred that fecundation has taken place; and this by the introduction of some substance into the germinal vesicle from the exterior of the ovary. It may also be inferred, that the central portion of the altered germinal spot is the point of fecundation. In further proof that such really is the case, there arise at this part two cells, which constitute the foundation of the new being. These two cells enlarge, and imbibe the fluid of those around them, which are at first pushed further out by the two central cells, and subsequently disappear by liquefaction. The contents of the germinal vesicle thus enter into the formation of two cells. The membrane of the germinal vesicle then disappears by liquefaction.

Each of the succeeding twin cells presents a nucleus, which, having first passed to the centre of its cell, resolves itself into cells in the manner above described. By this means the twin cells, in their turn, become filled with other cells. Only two of these in each twin cell being destined to continue, the others, as well as the membrane of each parent-cell, disappear by liquefaction, when four cells remain. These four produce eight, and so on, until the germ consists of a mulberry-like object, the cells of which do not admit of being counted. Nor does the mode of propagation continue the same with reference to number only. The process inherited from the germinal vesicle by its twin offspring, reappears in the progeny of these. Every cell, whatever its minuteness, if its interior can be discerned, is found filled with the foundations of new cells, into which its nucleus has been resolved. Together with a doubling of the number of the cells, there occurs also a diminution of their size. The cells are at first elliptical, and become globular.

The above mode of augmentation, namely the origin of cells in cells, appears by no means to be limited to the period in question. Thus it is very common to meet with several varieties of epithelium-cells in the oviduct, including those which carry cilia, filled with cells; but the whole embryo at a subsequent period is composed of cells filled with the foundations of other cells.

In the second series of these researches, it was shown that the mulberry-like object above mentioned, is found to contain a cell larger than the rest, elliptical in form, and having in its centre a thick-walled hollow sphere, which is the nucleus of this cell. It was further shown that this nucleus is the rudimental embryo. From what has been just stated, it appears, that the same process, by which a nucleus in one instance transforms itself into the embryo, is in operation in another instance, where the product does not extend beyond the interior of a minute and transitory cell. Making allowance, indeed, for a difference in form and size, the description given of the one might be applied to the other. It was shown in the second series, that in the production of the embryo out of a nucleus, layer after layer of cells come into view in the interior, while layers previously formed are pushed further out; each of the layers being so distinctly circumscribed as to appear almost membranous at its surface. The same membranous appearance presents itself at the surface of the several layers of a nucleus in many situations. Further, in the formation of the embryo, a pellucid centre is the point around which new layers of cells continually come into view; a centre corresponding to that giving origin to similar appearances in every nucleus described in the present memoir. It was shown that in the embryo this mysterious centre is present until it has assumed the form of the cavity, including the sinus rhomboidalis, in the central portion of the nervous system.

The process above described as giving origin to the new being in the mammiferous ovum, is no doubt universal. The author thinks that there is evidence of its occurrence in the ova of batrachian Reptiles, some osseous Fishes, and certain of the Mollusca; though the explanation given of these has been of a very different character. It has hitherto been usual to regard the round white spot, or cicatricula, on the yelk of the bird's laid egg, as an altered state of the discus vitellinus in the unfecundated ovarian ovum. So far from thinking that such is the case, the author believes the whole substance of the cicatricula in the laid egg to have its origin within the germinal vesicle, in the same manner as in the ovum of Mammalia.

There is no fixed relation between the degree of developement of ova, and their size, locality, or age. The variation with regard to size is referable chiefly to a difference in the quantity of fluid imbibed in different instances by the incipient chorion. Vesicles filled with transparent fluid are frequently met with in the Fallopian tube, very much resembling the thick transparent membrane of the ovarian ovum. These vesicles are probably unimpregnated ova, in the course of being absorbed. The so-called "yelk" in the more or less mature ovarian ovum, consists of nuclei in the transition state and exhibiting the compound structure above described. The mass of these becomes circumscribed by a proper membrane. They and their membrane subsequently disappear by liquefaction, and are succeeded by a new set, arising in the interior, and likewise becoming circumscribed by a proper membrane, and so on. This explains why some observers have never seen a membrane in this situation. After the fecundation of the ovum, the cells of the tunica granulosa, that is, part of the so-called "disc," are found to have become club-shaped, greatly elongated, filled in some instances with cells, and connected with the thick transparent membrane by their pointed extremities alone.

That the thin membrane described by the author in his second series as rising from the thick transparent membrane in the Fallopian tube, and imbibing fluid, is really the incipient chorion, was then shown by tracing it from stage to stage, up to the period when villi form upon it. There remained, however, two questions undecided; viz., whether the chorion is formed of cells, and if so, whether the cells are those of the so-called "disc," brought by the ovum from the ovary. The author now states that the chorion is formed of cells, which gradually collect around the thick transparent membrane, and coalesce; and that the cells in question are *not* those of the "disc" brought with the ovum from the ovary. The cells which give origin to the chorion are intended to be more particularly described in a future paper.

The existing view, namely, that a nucleus, when it leaves the membrane of its cell, simply disappears by liquefaction, is inapplicable to any nucleus observed in the course of these investigations.

The nucleus resolves itself into incipient cells in the manner above described. In tracing this process, it appears that the nucleus, and especially its central pellucid cavity, is the seat of changes which were not to have been expected from the recently advanced doctrine, that the disappearing nucleus has performed its entire office by giving origin at its surface to the membrane of a single cell. It is the mysterious centre of a nucleus which is the point of fecundation; and the place of origin of two cells constituting the foundation of the new being. The germinal vesicle, as already stated, is the parent cell, which, having given origin to two cells, disappears, each of its successors giving origin to other two, and so on. Perpetuation, however, at this period, consists, not merely in the origin of cells in cells, but in the origin of cells in the pellucid central part of what had been the nucleus of cells.

The author shows that neither the germinal vesicle, nor the pellucid object in the epithelium-cell, is a cytoblast. He suggests, that the cells into which, according to his observations, the nucleus becomes resolved, may enter into the formation of secondary deposits —for instance, spiral fibres; and that they may contribute to the thickening which takes place, in some instances, in the cell-membrane.

The germ of certain plants passes through states so much resembling those occurring in the germ of mammiferous animals, that it is not easy to consider them as resulting either from a different fundamental form, or from a process of developement which even in its details is not the same as what has been above described; the fundamental form in question in Mammalia—and therefore it may be presumed of Man himself—being that which is permanent in the simplest plants,—the single isolated cell.

LXXXII. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTION OF ELECTROTYPES. BY ALFRED SMEE, ESQ., SURGEON.

[Illustrated by Plate VII.]

THE mode of taking copies of medals by the galvanic current* is deservedly occupying much of the public attention, and each is striving to add his mite to the perfection of this elegant and useful process. There are two or three points to which I am desirous of drawing the attention of your readers, as they appear to open a new and important field for investigation for which I have not the time at present. With regard to the precipitation of the copper, I beg leave to submit a modification of a plan first proposed by Mr. Mason, in a paper read before the Electrical Society, but I believe also contemporaneously used by other persons, that of making copper form the oxygen side of the battery, which being dissolved is again thrown down at the platina or hydrogen end upon the medal or mould placed for its reception.

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* We learn from the foreign journals, that Prof. Steinheil, of Munich, is applying this process for making a cast in copper, from a composition by the celebrated sculptor Schwanthauler, representing the labours of Hercules, and containing 140 figures.—Edit.

The mode which I adopt is, first to obtain a long dish or trough, and then to place a wire in the inside along its bottom, which is connected to the zinc of one of the cells of my battery along the opposite side of the vessel; a large piece of copper is placed in connexion with the silver of the battery, and a solution of sulphate of copper is then added. By this arrangement the current is generated at the zinc, passes to the medal, reduces the copper whilst the oxygen and acid are transferred to the refuse copper, and dissolves a corresponding quantity of copper, and by this means the solution is always kept saturated with the metal.

When medals are to be copied, they are singly placed in contact with the wire in connexion with the zinc of the battery, and in this way many may be done in the same vessel, and any may be taken out and examined without the slightest interruption to the others. The rapidity of the process may be increased without detriment by the use of two to six or even more cells of the battery, as the copper will still be extremely tough. It will be found that my battery will require not the slightest alteration, except once a day, when the liquid should be changed. I have tried other solutions of copper, such as the nitrate; but although the process is hastened, the metal is apt to be brittle, or to have other imperfections.

When engraved plates are to be copied, the first copy is in basso relievo, and therefore a second is required to be made which is in "intaglio," and then ready for printing. Copies may even be taken of non-conducting substances, as wood-cuts, &c., by brushing them over with black-lead, taking care that the copper wire is in good contact with the plumbago.

The great advantages of this mode of proceeding above all others are, first, the quality of the copper is far better than when reduced in the usual way as described by Messrs. Spencer and Solly; this advantage is owing to the use of the copper at the oxygen end as suggested by Mr. Mason; secondly, all the plates or medals, for there is no limit to the number, are in the same vessel; thirdly, the process may be hurried or retarded, accordingly as the number of plates of the battery are increased or diminished; fourthly, the plates will not require to be interfered with till the precipitation is completely finished; and there are many other more trifling advantages which it would be tedious to enumerate.

The mode of proceeding here detailed differs but little from others which have been described; but these trifling differences are so important in practice, that this mode will probably supersede every other. In fact, I have had the pleasure of seeing many most valuable copper-plates subjected to this process, and the specimen which accompanies this paper is I believe the first which has ever undergone the ordeal of having the large number of impressions, required for any publication, printed from it. Of course it is a perfect facsimile, and therefore this method would be of the greatest importance to bankers for their notes, and is far superior to Mr. Perkins's process for the multiplication of plates, because in that case they almost invariably require to be touched up afterwards, and therefore absolute identity is destroyed. The cost of their manufacture would be trifling, being merely the value of the zinc* dissolved in the battery, and a pound of zinc of the value of sixpence would produce a copper-plate weighing about two pounds; and I trust that copper will again, from its beauty, take the place of steel engravings.

So much for the precipitation of the copper; and the next thing to which I have to direct your attention, is a mode of making a copper-plate engraving without an engraving in the first instance. This is done by drawing upon a smooth piece of copper (such as a plate used for engraving) with any thick varnish or pigment insoluble in water, and then exposing the plate in the usual way to the influence of the current, when first copper will be thrown down upon the uncovered parts and will gradually grow over the drawing, and the electrotype when removed will be ready for printing. A practical difficulty, however, arises in the application of this in the arts, for unless very thick oil paint is used, sufficient depth is not obtained to hold the ink. However, judging from the sharpness of the edges of the lines, I have but little doubt that this difficulty may be overcome by these who are accustomed to drawing; and it possesses, as an additional advantage to its cheapness, the valuable property of not requiring the artist to reverse the design. An opposite effect to this may be produced by placing a piece of copper similarly drawn upon at the oxygen end of the battery, when the metal will be acted upon, leaving a drawing in basso relievo.

Bank of England, April 21, 1840.

ON THE REDUCTION OF CHROMATE OF LEAD. BY R. F. MARCHAND.

The employment of chromate of lead, instead of oxide of copper, in organic analysis, is in many cases recommended by Richardson. It is preferable in the examination of substances containing chlorine, iodine, bromine, and sulphur, but particularly in the examination of the two latter. Erdman and I have frequently employed it with Hess's apparatus, and observed that the reduced chromate of lead will again absorb oxygen and might be then again employed. This circumstance induced me to make various experiments upon the reduction of this salt.

The chromate of lead used in these experiments was prepared by the precipitation of a solution of nitrate of lead with an excess of bichromate of potash, and afterwards carefully washing it: the salt was heated to dryness; it became of a dark red colour approaching cinnabar red; on cooling it returned to its former yellow colour provided it was not fused. If fused it turned to a dark brown colour, which on reducing to powder was of a brownish yellow colour. When the fused salt is quickly cooled by throwing it into cold water it becomes of a permanent red colour, giving also a red powder.

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* The zinc in the fluid might be precipitated as a carbonate, for which there is great demand in the arts, and thereby the expense of the electrotype would be further diminished.

Many persons imagine that in organic analysis with chromate of lead it is necessary to use a very strong heat in order to perfect the operation. This is a mistake, for carbon as well as hydrogen very easily reduces the chromate. If however it is required to liberate oxygen, then the temperature must be very great, and the salt must be fused. This circumstance, as may be easily conceived, is inconvenient and liable to introduce error.

When chromate of lead is heated in a current of hydrogen gas, it commences to glow at a heat far below redness, and a quantity of water is formed. The yellow colour of the salt disappears; it becomes black, and very small metallic globules are disseminated through the mass.

3·049 grammes lost 0·307 gram. or 10·07 per cent. of oxygen; this loss may be increased by a continued and strong heat.

1·91 grammes lost in another experiment 0·224 gram., or 11·8 per cent. oxygen. At the commencement of the reduction of this portion the temperature was kept moderate, by which it lost 0·2045 grm., or 10·7 per cent. Oxygen gas was then passed over it while in a heated state. At a low degree of heat, the mass burnt with great brilliancy, turned brown, at least partly so, which was very evident on cooling. It absorbed 0·133 grm. oxygen, which calculated for the original quantity (1·91) amounted to 7 per cent.; a small quantity of water was formed during the operation, amounting, however, to but a few milligrammes. It would therefore appear that hydrogen was condensed in the pores of the reduced mass, but in a small quantity; heating in a stream of carbonic acid gas would have entirely driven it out. The oxidated quantity 1·839 grm. which had lost from the first 3·7 per cent. of oxygen, was again reduced in hydrogen gas, by which it lost 0·152 grm., this upon the whole quantity is equal to 8 per cent. Upon heating in oxygen gas the same appearances again took place, and the mass absorbed 0·128 grm., therefore, a similar quantity as before. A subsequent reduction at a very high temperature occasioned a loss of 0·128 grm. while a repeated oxidation only gave an increase of 0·119. This was again driven out by hydrogen, but without any further decrease of oxygen.

If we examine these experiments, keeping in view the results, we shall find that 1·91 grm. lost 0·224 grm. equal to 11·8 per cent. while the last oxidation was only 1·19 grm. or 6·2 per cent. Chromate of lead contains 19·54 per cent. of oxygen, equal to four atoms, $\dddot{\mathrm{Cr}}\,\dot{\mathrm{Pb}}$. 11·8 per cent. is equal to 2·4 atoms or nearly $\frac{5}{8}$ths of the whole amount of oxygen. This would be 12·2 per cent. The reduction, if complete, would convert all the oxide of lead into the metallic state and the chromic acid into the state of oxide of chrome.

$$\begin{array}{rr}
2\,\mathrm{Cr} + 6\,\mathrm{O} & 2\mathrm{Pb} + 2\,\mathrm{O} \\
3\,\mathrm{O} & 2\,\mathrm{O} \\
\hline
2\,\mathrm{Cr} + 3\,\mathrm{O} & 2\,\mathrm{Pb}.
\end{array}$$

By oxidation about half of the oxygen is recombined; this takes place the more readily when the metallic lead is in a finely divided state and not melted by too strong a heat. It is not the lead alone, but also the oxide of chrome, which absorbs oxygen. If oxide of chrome alone be heated in oxygen gas, it is not converted into chromic acid; but this takes place, as is well known, if an alkali be present. I therefore consider that oxide of lead has the same effect in this respect as an alkali. In order to obtain an intimate mixture of oxide of chrome and oxide of lead, I endeavoured by means of heat so to decompose the chromate of lead, that all the chromic acid should be converted into oxide of chrome. The temperature must be very high for this purpose, and it requires a long time before any considerable quantity of oxygen can be driven out of this salt. 1·409 gramme was fused in a very thin platina crucible by the strongest heat of a spirit-lamp before any appreciable loss took place. 0·057 grm. were then given off equal to 4 per cent. which is nearly $\frac{3}{10}$ths of the whole quantity contained in it; this would be 3·9 per cent. It is therefore very probable that at first the chromate of lead is so decomposed that basic chromate of lead and oxide of chrome are formed.

$$2\,(Cr\,O_3\,Pb\,O) = Cr\,O_3\,Pb_2\,O_2 + Cr\,O\,1\tfrac{1}{2} + O\,1\tfrac{1}{2}.$$

The compound obtained in this manner I considered very favourable for the conversion of chromic oxide into chromic acid. I heated it, and passed a stream of oxygen through it. However, to my great surprise, not the least alteration took place, and I found no increase in weight.

In the oxidation of the reduced salt, oxygen must have combined with the oxide of chrome, as 6·2 per cent. was in all absorbed, while the whole of the oxygen of the oxide of lead in the salt amounts only to 4·89 per cent.; and it is not at all likely that even the whole of this 4·89 per cent. was absorbed by the reduced lead, as it was for the most part fused into small globules, which must have very much prevented the action of the oxygen upon it.

I again fused 2·057 grammes of chromate of lead in an æther lamp supplied with oxygen: after a long-continued heat it lost 0·091 grm. or 4·4 per cent. The reduction was in this case carried on rather further than the conversion into basic chromate of lead and oxide of chrome. When, however, oxygen gas was passed over this compound very little of it was absorbed.

I at last prepared an intimate mixture of oxide of chrome and superoxide of lead; this was heated to redness and oxygen gas passed through it. This compound contained 0·445 grm. oxide of chrome. When the absorption had terminated, the increase of weight was found to be 0·066 grms. The colour was changed from green to brown. 0·455 grm. oxide of chrome contain 0·132 oxygen, therefore just double the quantity it had taken up. Therefore one atom of oxide of chrome upon heating with oxide of lead had combined with 1½ atom of oxygen.

This gives $2\,Cr\,3\,O + 2\,Pb\,O + 1\tfrac{1}{2}\,O$, or,

$$Cr\,O_3\,Pb_2\,O_2 + Cr\,O\,1\tfrac{1}{2}.$$

It is therefore the same compound which is formed when chromate of lead is fused by itself.

It follows from the foregoing experiments:

That chromate of lead is very easily converted by means of carbon and hydrogen into a mixture of oxide of chrome and metallic lead.

This mixture by heating is in a state to combine with oxygen; and this combination takes place not only with the metallic lead, but also with the oxide of chrome.

By heat alone it is very difficult to deprive chromate of lead of oxygen. It is at first converted into a mixture of basic chromate of lead and oxide of chrome: in order to reduce all the chromic acid into oxide of chrome, an uncommonly highly temperature is required.

When a mixture of oxide of chrome and oxide of lead is heated, it is also converted into basic chromate of lead and oxide of lead.

It is therefore chromate of lead, which is often employed in organic analysis, from which this latter mixture is derived.—*Journal für Praktische Chemie*, No. 2. 1840.

PORTRAITS IN DAGUERREOTYPE.

Professor Draper, of the University of New York, informs us in a note dated March 31st, that he has succeeded during the winter in procuring portraits by the Daguerreotype, and that they have all the beauty and softness of the most finished mezzotint engraving, and only require from 20 to 45 seconds for execution.

METEOROLOGICAL OBSERVATIONS FOR APRIL, 1840.

Chiswick.—April 1. Slight rain: cloudy. 2. Hazy: very fine. 3. Cold dry haze: frosty at night. 4—6. Very fine. 7. Fine: stormy showers at night. 8. Slight showers. 9. Cloudy and cold. 10—12. Very fine. 13—17. Fine but very dry. 18. Clear, hot and dry. 19. Hazy: very fine. 20. Very fine. 21—23. Cloudy and fine. 24. Very fine. 25. Very hot, nearly cloudless, and excessively dry. 26, 27. Hot and dry. 28. Excessively hot for the period of the season, thermometer 81° in the shade. 29, 30. Very fine: hot and dry. This month is remarkable for the limited quantity of rain and for a high temperature; the latter being the consequence chiefly of a powerful direct solar heat, which overcame likewise the counteracting effects of north and north-east winds, for they were in fact more prevalent than those from the opposite direction.

Boston.—April 1. Cloudy: rain P.M. 2. Rain. 3—5. Fine. 6. Cloudy. 7. Cloudy: stormy with rain P.M. 8. Cloudy: hail and rain P.M. 9—11. Fine. 12. Rain: rain early A.M. 13—19. Fine. 20—24. Cloudy. 25—29. Fine. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—April 1. Mild day with a shower. 2. Keen and cold but dry. 3, 4. Dry and more temperate. 5. Fine day after a very slight shower. 6. Stormy day with showers, though slight. 7. Keen cold day. 8. More moderate. 9. Fine mild day. 10. The same: slightly moist and cloudy. 11. Drizzling all day, but very lightly. 12. Fine though cold: slight rain P.M. 13. Fine soft slight rain. 14. Charming spring day. 15. The same: with frost rime A.M. 16, 17. Fine but coldish: frost rime again. 18. Very fine warm day. 19. The same: white rime A.M. 20. The same: slight showers P.M. 21. The same: gentle shower. 22. The same: moisture. 23. Dry but threatening. 24. The same: cleared up. 25—28. Beautiful day. 29. The same, but cloudy. 30. The same: very warm.

Sun shone out 29 days. Rain, very slight, fell 6 days. Frost, rime 4 days.

Wind north 1 day. North-east ½ day. East-north-east 2 days. East 3 days. East-south-east 1 day. South-east ½ day. South-south-east 2 days. South 7 days. South-south-west 1 day. South-west 7½ days. West-south-west 1 day. West 2½ days. North-west 1 day.

Calm 15 days. Moderate 8 days. Brisk 3 days. Strong breeze 3 days. Boisterous 1 day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; by Mr. THOMPSON at the Garden of the Horticultural Society at Chiswick, near London; by Mr. VEALL at Boston, and by Mr. DUNBAR at Applegarth Manse, Dumfries-shire.

Days of Month. 1840. April.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register.		Max.	Min.		Max.	Min.									
								Max.	Min.														
1.	29·664	29·685	29·638	29·27	29·50	29·55	44·4	47·0	43·9	54	53	42	48½	37	S.	S.	calm	E.	·072	·02	...	...	42
● 2.	29·720	29·922	29·714	29·35	29·73	29·93	46·3	47·2	39·3	60	38	46	46½	37½	E.	SE.	E.	E.	...	...	·14	...	40
3.	30·024	30·063	30·012	29·65	30·02	29·94	42·3	50·0	41·0	53	27	44	51	35	NNE.	E.	E.	SSE.	...	...	·02	...	42
4.	29·960	29·980	29·918	29·52	29·85	29·90	39·8	49·0	34·4	51	32	43·5	51½	34	N.	N.	N.	W.	...	...	...	...	37
5.	30·044	30·062	30·012	29·58	29·92	29·76	45·2	45·8	39·6	59	35	45	52½	39	NW.	NW.	N.	SW.	...	...	...	...	39
6.	29·798	29·814	29·480	29·22	29·43	29·49	49·3	55·2	38·8	58	30	49	51	40½	S.	W.	W.	SW.	...	...	...	0·05	40
7.	29·558	29·845	29·560	29·15	29·65	30·04	43·4	44·4	38·9	52	36	46	46	35	NNW.	N.	NW.	N.	·033	·02	...	...	41
8.	29·998	30·175	30·026	29·64	30·11	30·11	41·5	42·2	37·8	54	29	41·5	45	33	N.	N.	N.	NW.	·116	·01	·07	...	38
☽ 9.	30·264	30·416	30·277	29·83	30·22	30·25	42·3	43·3	35·3	50	25	46	53	37	NNW.	N.	N.	W.	...	·01	·05	...	37
10.	30·408	30·409	30·373	29·89	30·24	30·12	44·7	45·4	36·4	56	25	46	50½	44	NNW.	NE.	E.	S.	...	...	...	...	34
11.	30·258	30·283	30·126	29·79	29·90	29·90	46·8	47·7	35·4	62	33	47·5	48	44	S.	SW.	W.	S & W.	...	...	...	..	36
12.	30·058	30·075	29·994	29·55	29·95	29·88	47·2	48·0	43·8	60	26	49	48½	41	SSW.	SW.	calm	E.	...	...	·14	..	43
13.	29·918	29·950	29·860	29·49	29·80	29·78	45·0	46·0	39·7	68	26	51	55	42½	W.	E.	calm	S&SW.	...	...	·12	0·20	38
14.	29·808	29·868	29·808	29·33	29·75	29·78	46·9	48·7	41·6	69	30	50	57½	39	NNW.	NW.	calm	NE.	...	...	...	...	40
15.	29·942	30·050	29·936	29·40	29·83	29·94	55·7	56·3	44·7	70	29	52·5	62	40½	ENE.	S.	calm	SW.	...	...	...	...	40
○ 16.	30·108	30·185	30·095	29·48	30·06	30·23	55·7	56·4	44·6	70	37	54	54	40½	NW.	N.	calm	ENE.	...	...	...	...	42
17.	30·188	30·204	30·105	29·65	30·27	30·16	50·5	51·6	40·9	62	32	46	54½	36½	N.	E.	N.	E by N.	·033	...	...	...	44
18.	30·040	30·164	29·992	29·65	30·05	29·93	47·0	47·7	38·2	64	33	47	58	34	N.	E.	N.	SSE.	...	...	...	...	43
19.	29·922	29·927	29·884	29·45	29·89	29·82	42·8	43·5	39·0	64	29	43·5	60½	33	NW.	E.	calm	ESE.	...	...	...	...	40
20.	29·898	29·974	29·886	29·36	29·73	29·75	51·2	51·6	42·0	69	41	52	57½	39	ENE. V.	W.	calm	SSW.	...	...	...	0·03	41
21.	30·074	30·142	30·060	29·44	29·78	29·78	53·8	54·8	47·0	62	46	54	56	44	S.	W.	calm	SW.	...	...	...	...	44
22.	30·234	30·337	30·212	29·56	30·08	30·08	58·2	58·8	49·3	66	42	57	54	38	NW.	N.	calm	S.	...	...	...	...	49
23.	30·348	30·337	30·305	29·62	30·10	30·09	58·4	59·2	50·0	69	49	58	63	49	W. var.	W.	calm	S.	...	...	...	...	50
☾ 24.	30·298	30·277	30·126	29·58	30·08	30·05	58·3	58·8	50·4	75	41	57	58½	46½	S.	SW.	calm	W by S.	...	...	...	...	51
25.	30·174	30·223	30·112	29·51	30·03	30·00	61·8	66·4	50·7	80	40	62	67	40½	E.	S.	E.	S.	...	...	...	...	53
26.	30·312	30·369	30·280	29·64	30·14	30·25	60·5	66·2	51·5	77	42	61	64½	47½	NW.	SW.	calm	S.	...	...	...	...	54
27.	30·356	30·340	30·297	29·80	30·28	30·20	62·5	66·8	51·2	76	42	61	67½	41½	NE.	E.	E.	SE.	...	...	...	0·00	55
28.	30·300	30·292	30·257	29·75	30·11	30·04	59·7	67·0	48·7	81	40	62	70½	43	ENE.	SE.	calm	S.	...	...	...	..	52
29.	30·298	30·323	30·268	29·70	30·14	30·18	67·0	67·0	54·0	79	39	61	62½	45	NW.	NW.	NW.	SW.	...	...	...	...	52
30.	30·324	30·338	30·238	29·75	30·20	30·22	59·2	72·2	52·0	74	46	58·5	67½	50	NW.	NE.	calm	SW.	...	...	...	...	51
Mean.	30·077	30·133	30·028	29·55	29·094	29·071	50·9	53·5	43·3	64·80	34·86	51·2	56·	40·2					Sum. ·234	·06	·54	0·28	Mean. 49·6

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LXXXIII. *On Galvanic Circuits composed of two Fluids, and of two Metals not in contact.* *By* Professor J. C. POGGENDORFF.*

[Continued from p. 498, and concluded.]

WITH all combinations in which zinc, iron, or tin forms the positive metal, even with some in which amalgamated zinc occupies that place, the water has the ascendency over the (dilute) hydrochloric acid, as already observed by Fechner with the combination zinc-copper in a somewhat more complex experiment, his *experimentum crucis*[†].

But water also exhibits this ascendency, in the greater number of cases, over sulphuric acid[‡]; with the combinations zinc-platina, zinc-tin even more strongly than over hydrochloric acid, exceedingly strong, especially with amalgamated zinc and recently heated platina. And yet sulphuric acid is no electrolyte.

Nor is nitric acid an electrolyte, and nevertheless it so alters the electromotive force that with zinc-platina it has the ascendency over water, but in the other combinations with zinc and tin as positive member it succumbs to it.

Similarly circumstanced is ammonia, which is likewise no electrolyte. In all combinations with iron and tin as positive member, it always succumbs to water; in those with *zinc*, amalgamated or pure, it has on the contrary (copper excepted) generally the ascendency.

Solution of chlorine affords a similar example. The difference of its action from water and from hydrochloric acid is sufficiently evident from the table; and yet it can hardly be classed among the electrolytes.

The assertion of Vorsselman de Heer that only electrolytes are capable of changing the electromotive force, is therefore certainly incorrect.

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[*] From Poggendorff's *Annalen*, vol. xlix. January, 1840: translated by Mr. W. Francis.

[†] Lond. and Edinb. Phil. Mag., vol. xiii. p. 374.

[‡] As I have already shown, although less distinctly, in another way for the zinc-copper circuit. *Annalen*, vol. xlv. p. 405.

Just as little, in my opinion, do the facts speak for Faraday's theory. A stronger affinity must undoubtedly be attributed to the *chlorine* for *zinc*, *iron*, and *tin* than to the oxygen; and nevertheless, in circuits containing one of these metals as positive member, *hydrochloric* acid does not act more *strongly*, but more *weakly*, than water[*].

In the dilute sulphuric and nitric acids, according to Faraday, only the affinity of the oxygen of the water acts on the positive metal. Consequently these acids, when water acts opposed to them, must either produce no current, or, if the above-mentioned affinity is increased by their presence, develope, as is otherwise generally admitted, a *more powerful* electromotive force than pure water. Nevertheless the two acids act, in the greater number of cases examined, more *weakly* than water[†].

The same observation may be applied to *caustic potash* and *ammonia*. In Faraday's "*Experimental Researches*" we find it stated, § 919, "Thus, when zinc, platina, and dilute sulphuric acid are used, it is the union of the zinc with the oxygen of the water which determines the current;" further, § 932, "The similarity in the action of either dilute sulphuric acid or potassa goes indeed far beyond this, even to the proof of identity," and § 933, "But all the effects in these experiments prove, I think, that it is the oxidation of the metal necessarily dependent upon, and associated as it is with, the electrolyzation of the water, that produces the current; and that the acid or alkali merely act as solvents by removing the oxidized zinc."

But in truth the caustic alkalies always act differently from *water* and differently from acids[‡], and that immediately, in the first moment of immersion, when the zinc is still in possession of its full metallic lustre. There is, indeed, a certain relation to the attackability of the positive metal apparent, as is evident for instance from the comparison of the *iron* circuits with *zinc* and *tin* circuits; but why *both alkalies* with the iron circuits, and the *ammonia* with tin circuits have in general a *weaker* action than *water*,—why, even with the combinations *zinc-platina, zinc-silver*, the alkalies have the ascendency over the water, is according to that view not conceivable.

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[*] It also acts more weakly than sulphuric acid, at least with amalgamated zinc and platina or silver. (See Note, p. 423.)

[†] Becquerel observed years ago (*Ann. de Chim. et de Phys.*, vol. xli. p. 17) that when zinc and copper are placed in two cells separated by membrane and filled with a solution of the sulphate of zinc, and then some nitric acid is poured into one of them, this addition in the copper cell heightens the current, but in the zinc cell weakens it. This experiment, certainly, when correctly apprehended, as already observed by Berzelius (*Jahresbericht*, No. x. p. 23), is no argument in favour of the chemical theory. Similar facts, have moreover been noticed by Fechner (*Annalen*, vol. xliii. p. 433).

[‡] This indeed has not escaped Faraday. In § 941, he says, "The alkali, in fact, is superior to the acid in bringing a metal into what is called the positive state;"—but why? it may be asked; and how does this agree with § 921? in which it is stated, "Oxidation or other direct action upon the metal itself is the source of the current, but it is of the *utmost importance* to observe that the oxygen or other body must be in the state of combination, and indeed in such a state of combination as to constitute an electrolyte."

It is the same in all those cases in which two oxy-combinations are opposed to one another: *water* and *carbonate of soda, sulphuric acid* and *borax, sulphate of zinc* and *borax, sulphate of magnesia* and *borax*.

Perhaps here, from the carbonate of soda and borax being easily decomposable salts, and their solutions in the cases examined always succumbing to that of the second oxy-combination, a position from the *Experimental Researches* might be brought forward (§ 549) according to which substances are said to produce a more powerful current the more difficultly decomposable they are, and *vice versâ*. Thus might also be explained the general weaker action of dilute acids in comparison to water, since acidulated water is more easily decomposable than pure. However, this position appears hardly in unison with the fundamental principle of the chemical theory, that the affinity of the oxygen, chlorine, &c., for the zinc, is that which excites the current; for it might rather be supposed that this affinity can the less enter into activity the more strongly the oxygen, chlorine, &c., are retained by the positive element from which they are to be separated. Moreover, it cannot be applied to the alkalies, which in most cases increase the electromotive force and yet at the same time render water more decomposable than it is of itself. A further example, among many others, is afforded by *salt* and *sal-ammoniac*, two chlorides, the solution of the latter of which developes a stronger electromotive force than that of the first. It can hardly be admitted that sal-ammoniac is of more difficult decomposition than salt!

The solution of chlorine likewise, if I correctly understand Faraday's view, could not, according to it, act differently from pure water; for the chlorine is merely dissolved in the water, not combined with a body to form an electrolyte. Nevertheless it acts with zinc-platina, for instance, much more strongly than water. It acts, moreover, instantaneously on immersion, so that the action can scarcely proceed from formed chloride of zinc, which is also evident from the fact, that solution of chlorine, which has been frequently employed, containing therefore a considerable quantity of chloride of zinc, does not act in the least more powerfully than when pure.

If, on the other hand, the action is derived from the chlorine, then again, according to this view, it cannot be conceived why the *solution of chlorine* acts more powerfully than *hydrochloric acid*, in which, nevertheless, the chlorine is electrolytically combined with hydrogen, and without doubt is more strongly retained by the *hydrogen* than in the solution of chlorine by the *water*.

It might perhaps be replied, that the solution of chlorine is not a mere solution of the chlorine, but a mixture of hydrochloric acid and the oxide of chlorine; but setting aside that this supposition is not proved, is not even very probable, and is accompanied by a second supposition not more probable, that this mixture or the oxide of chlorine is of more difficult decomposition than hydrochloric acid, we need only call to mind the other cases where *non-electrolytes* exert a sensible influence on the development of the electromotive force,—the actions of *sulphuric acid*, of *nitric acid*, *ammonia*, *water* impregnated with *oxygen*, of the *peroxide of hydrogen*[*], of the *quinto*-sulphuret of potassium ($K\,S_5$)—to perceive that such a supposition is neither required nor can be generalized.

The position, that those bodies which, brought between the metallic plates of a voltaic pile, render it active, are all electrolytes (*Exp. Res.* § 858, 921), must therefore be thus altered, that the fluids between the metallic plates must, it is true, be electrolytes, *i. e.* decomposable bodies, since, at least with aqueous fluids and with a certain intensity of current, no conduction can take place without decomposition; but that the electromotive force which is developed on the contact of these fluids with the metals is not in any necessary connexion with the conductivity or decomposability, and can be increased or diminished by bodies which are not electrolytes, *i. e.* not directly decomposable[†]. I shall hereafter communicate another proof on this point.

Not less difficult for the disputed theory would be the cases in which *hydrochloric acid* and *sal-ammoniac*, *hydrochloric acid* and *salt*, or *salt* and *sal-ammoniac* are opposed to each other. In each of them, according to it, only the chlorine could act on both sides, and consequently no current arise. But I will pass over the discussion of these cases and turn to the *iodide of potassium*, from which Faraday has derived the new argument in favour of the chemical theory.

In the preceding tables I have enumerated the action of the iodide of potassium in 17 metallic combinations, both towards

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[*] Becquerel, *Ann. de Chim. et de Phys.*, vol. xxviii. p. 19.

[†] De la Rive and other supporters of the oxidation-theory, admit also, as is well known, that it is the oxidation of the zinc alone, no matter whether it be caused by the oxygen of the atmosphere absorbed by the water or any other action, which determines the current.

sulphuric acid and towards *hydrochloric acid*; in the whole, therefore, 34 cases. In 18 cases the acid had the preponderance; in 10 the iodide of potassium. In 6 the result was ambiguous, as during the experiment a decided reversion of the current ensued; and probably some of the first belong to this category. If we merely look at the *number* of favourable cases, the affinity-theory would indeed still retain some probability; but this must entirely disappear if at the same time we take into consideration the *weight* of the unfavourable cases.

Among the latter, viz. the unfavourable cases, those with platina more especially surprised me, and of these that with the combination *zinc-platina* and *hydrochloric acid.* It is in fact the same experiment which Faraday mentions in support of his view, which he himself had the kindness to show me in London (in this experiment *hydrochloric* acid was employed), and which I also had previously more than once performed with the same positive result. Whence now the opposite result? An error it could not be; I had repeated it so often and under the most varied circumstances! After some experiments I was so fortunate as to find the solution of this enigma; and with this, at the same time, the untenability of the explanation of the experiment, as also of the whole of this argument in favour of the chemical theory of Galvanism, is, in my opinion, clearly demonstrated.

The result of the experiment, in fact, depends solely on the *concentration* of the acid*. If we employ, as was the case in all the experiments enumerated in the table, a hydrochloric acid of 1·138 spec. gr., diluted with 6 times its volume of water, the iodide of potassium has the preponderance; but if, on the contrary, the same acid be employed but not diluted, the superiority is then on the side of the acid; and the current has such a direction, that on the decomposition of the iodide of potassium, the iodine passes to the platina. Between these two degrees of concentration of the acid, there will evidently be one with which not the least current will occur†.

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* Probably also on the concentration of the solution of the iodide of potassium; but I have not varied this.—On the other hand, I have assured myself, that the purity of this solution has little influence. A solution which from frequent use was mixed with sulphuric acid, had thus become brown and acid, and contained therefore free hydriodic acid with iodine, and dissolved zinc with effervescence,—acted just the same as pure iodide of potassium, and evinced with platina the superiority over hydrochloric acid more strongly than the latter.

† Likewise with *silver*, *copper*, and *tin*, combined with *amalgamated zinc*, the concentrated *hydrochloric acid* (of 1·138 spec. gr.) has instantaneously and strongly the ascendency over the iodide of potassium; even the dilute acid with *silver* and *copper* acts in the same way, as shown in the table; with *tin*, however, it succumbs to the iodide of potassium. The action of *tin* is therefore quite analogous to that of *platina.*

It is not to be supposed that the acid, thus diluted, is too weak to act on the zinc; on the contrary, it attacks it very violently, so energetically indeed that it becomes sensibly warm. The plea, that with the dilute acid the affinity of the chlorine for the zinc has become from the presence of the water weaker than that of the iodine for the same metal, is therefore inadmissible.

Moreover, the same acid has with *silver* and with *copper*, no matter whether combined with *zinc* (common or amalgamated) *iron* or *tin*, in a high degree the ascendency over the solution of the iodide of potassium,—a phænomenon which, compared with the opposite one on employing *zinc-platina*, at the same time clearly proves what essential part the *two* metals of the circuit take in the production of the current, and how contrary to nature, therefore, that view is, according to which, in reference to the voltaic current, the positive metal is termed the *generating* and the negative the *conducting* metal*.

Similar phænomena, not favourable to the theory, are evinced by the circuits of *sulphuric acid, iodide of potassium, platina* and a positive metal, by which, moreover, when the latter consists of *zinc*, especially non-amalgamated, those remarkable reversions in the direction of the current occur which have already been noticed in the table under No. 10.

An experiment performed specially for this purpose, and continued longer than usual, made me better acquainted with them. Bright filed plates of distilled zinc and non-heated plates of platina were employed for this experiment. The former remained during the whole time in the fluid; the latter were simultaneously immersed and taken out. During the first immersion, the current proceeded constantly in the direction $s > i$.

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* It is not very intelligible how this notion can have gained ground, since well-proved facts have long ago shown quite the contrary. If the negative metal in the circuit had merely to act as it were a passive part, to perform merely the function of *conducting*, then evidently the best conductor must produce the strongest current, or, rather, the greatest electromotive force. Copper conducts decidedly better than platina; but yet the latter, in combination with a positive metal, gives rise to a far greater electromotive force than the former. How essential the negative metal of the circuit is in *generating* the current, is most decidedly evident from the position first established by Fechner (Schweigger's *Journ*. vol. lx. 1830, p. 17, and Poggendorff's *Annalen*, vol. xliii. p. 433), that, as soon as the fluids do not act very alteringly on the metals, *the voltaic law of the tensions is also valid for the electromotive forces of the circuit*, that therefore, for instance, the electromotive force of a *zinc-platina* circuit is equal to the sum of the electromotive forces of a *zinc-copper* and of a *copper-platina* circuit. The law naturally cannot be applied to the intensity of the currents. The current from *copper-platina* is, as I have convinced myself, by far weaker than the difference of the currents from *zinc-platina* and *zinc-copper*,—easily imaginable from the inequality of the resistance of transition.

The first oscillation of the needle had the amplitude 80° − 45° (*i. e.* went from 80° on the one side of the meridian to 45° on the other). The deflexions, however, decreased rapidly, and when they had sunk to 12°−0° the platina was removed. This washed, dried, and again inserted, produced a current in the direction $s < i$. The first deflexions were 90° − 85; the following, 85° − 80°; 80° − 75°, &c., till at last 6° − 3°, when they were again taken out. The third, fourth, fifth, &c., immersion gave all of them currents in the direction $s < i$, only commencing with slighter deflexions.

At the same time two remarkable circumstances occurred here. At the commencement, namely, the (distilled) zinc, as is always the case when its surfaces are very bright, was but very slightly attacked by the dilute sulphuric acid, which also in this case consisted of 1 vol. concentrated acid, and 9 vol. water; but the longer it remained in it the stronger the action became, so that at last the disengagement of gas was very lively. At the commencement, the vibrations of the needle, although great, were nevertheless quite regular; but in proportion as the evolution of gas increased, sudden convulsions were evident in the vibrations, which became greater and greater, and at last passed into actual starts of 30, 40, 50, 60, 70, &c. degrees, and thus far exceeded the commencing deflexions which even on the fourth immersion did not amount to more than 10°. All these starts took place in the direction $s < i$. Their progressive increase evidently pointed to an increasing ascendency of the iodide of potassium over the acid.—The second notable circumstance was, that with each taking out of the platina, although both plates were removed at the same time, a strong deflexion (of 90°) likewise in the direction $s < i$ resulted. This phænomenon was especially surprising at the close of the first immersions, as the needle then made but very small and quite regular vibrations*.

I now repeated the same experiment (zinc filed bright, platina not heated) with a *stronger sulphuric acid* (1 vol. concen-

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[*] I frequently observed similar starts, and especially when, on employing *ordinary zinc*, I endeavoured to find out whether sulphuric acid or hydrochloric acid (both of the degrees of dilution mentioned at p. 489.) would develope the greater electromotive force. With silver, as negative metal, the convulsions and starts of the needle were so powerful that I could not decide as to the direction of the current. With *platina* these disturbances did not occur, because it was first examined, and the acids did not then act so violently on the zinc. With *copper* and *tin* they vanished in comparison to the force of the main effect; for with *copper*, the *sulphuric acid*, and with *tin*, the *hydrochloric acid*, had in the highest degree the superiority. Subsequent experiments, partly with bright filed distilled zinc, partly with ordinary, but amalgamated, showed me that also with *platina* and *silver* the sulphuric acid has the ascendency, although in a far less degree.

trated acid with 4 vol. water, or in weight 1 and 2 parts). In this case there was no indication of $s > i$; the first deflexion occurred even with the first immersion of the platina in favour of $s < i$, and indeed very violently = 90°. All the succeeding effects were likewise powerful, and in the same direction. The deflexion was, with slight vibrations, 80°, 70°, 60°, and so on, till at last about 20°, from whence, remarkably enough, it again increased. The amplitude of the last observed vibration went from $70° + 30°$.

The violent action of the acid on the zinc obliged me to terminate the experiment; but it was immediately recommenced with *amalgamated zinc* and the *same acid*. At present the current was nearly zero; only a slight deflexion of about 4° betrayed an inclination to $s < i$.

I now *heated* the *platina*, and indeed only that plate *which was to stand in the acid*. On contemporaneous immersion of both plates (of which the one heated had naturally, as in all similar experiments, perfectly cooled) a deflexion of 90° in the direction $s > i$ first resulted, immediately succeeded by one likewise of 90° in the direction $s < i$, and the needle now vibrated on the same side of the meridian, successively about the points 85°, 80°, 75°, 70°, 60°, 50°, 40°, till at last, after some minutes, it merely indicated a permanent deflexion of 2°, but still always in the direction $s < i$.

The heating of the platina was at present performed on the other plate, *which was to be placed in the iodide of potassium*. The result on immersion was of the same kind, but considerably less in strength than the previous one. The first deflexion in favour of $s > i$ amounted only to 5°, and the one immediately succeeding, in the direction $s < i$, to 22° only, upon which the needle then soon came to rest. A second heating of the plate to be placed in the acid, produced again the same result in its whole force. The first deflexion, in the direction $s > i$, was 90°; the second, in the direction $s < i$, likewise 90°, and the current now retained this direction with great energy, which diminished but very slowly*.

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* This phænomenon also appeared when dilute hydrochloric acid was employed, and to a far greater extent. If, after the effect has decreased to zero, the plate standing in the acid be taken out, washed, heated, and, after cooling, re-immersed, a movement of 90°, in the direction $s < i$, is immediately obtained, succeeded by a highly permanent deflexion in the same direction. If after the deflexion has again descended to nearly zero, we perform the same operation with the plate inserted in the iodide of potassium, this has but a very weak, in most cases, no result. Heating of both plates acted as when this was performed solely with the first. I likewise observed on this occasion that heated plates of platina almost wholly lose their remarkable effect by being suspended for some hours in the atmosphere.

I have communicated these details in order to show that the phænomena in circuits of the kind described are by no means always so simple as it seems they should be according to the affinity-theory. That, moreover, they do not speak for it is undoubtedly evident enough. Only at first and transitorily has the sulphuric acid the ascendency over the iodide of potassium; subsequently this acid, although it incontrovertibly attacks the zinc more violently than the iodide of potassium (even with energy increasing during the course of the action), is always overpowered by it; nay, what is remarkable, the less dilute acid (with 4 times its vol. of water) succumbs to the iodide of potassium to a greater extent than the acid diluted with 9 times its vol. of water. How is all this to be explained from the relative affinity of the oxygen and iodine to the zinc?

The above facts appear to stand in contradiction to the experiment of Faraday, mentioned at p. 487, in which a current was obtained with sulphuric acid, strongly overpowering the iodide of potassium. However, this contradiction is merely apparent; for, what was not there observed, the sulphuric acid was not pure, but purposely mixed with some nitric acid. A sulphuric acid, containing nitric acid, has in fact, (of which I have convinced myself,) in a high degree the superiority over the iodide of potassium; a powerful and permanent deflexion in favour of $s > i$ is immediately obtained, and at the same time there may be distinctly observed, from the yellow coloration of the solution of the iodide of potassium around the platina plate, the separation of the iodine*.

If, nevertheless, this fact is still employed, after so many proofs against the affinity-theory, as an argument in favour of it, it may then with justice be asked, Why then does the sulphuric acid, without this addition of nitric acid, afford in most cases the opposite result? Want of chemical action on the zinc it certainly is not! And then, how, even according to the affinity-theory, is the action of the nitric acid to be explained?

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* A dilute sulphuric acid, consisting of 1 vol. acid of 1·827 spec. gr., and 4 vol. water, to which was added an eighth of its volume of nitric acid of 1·321 spec. gr., was used for this experiment. A more dilute mixture, consisting of 12 parts in weight of dilute sulphuric acid (1 vol. concent. acid, and 9 vol. water), and one part in weight of the above nitric acid, gave by far weaker results. *Platina*, combined with *zinc*, produced, it is true, a current in the direction $s > i$; but even after it had been heated, it did not equal the current from *silver-zinc* or *copper-zinc*. With the combination *tin-zinc* the current had the direction $s < i$, as with pure sulphuric acid.

Dilute nitric acid alone (p. 489) likewise gives rise to similar effects. *Copper*, *silver*, *heated platina*, combined with *zinc*, immediately produced a powerful current in the direction $s > i$. With *non-heated platina* the direction of the deflexion was the same, but the intensity only slight, merely 5°; it increased, however, perceptibly, and without any oscillation the needle slowly rose to 45°, where it remained. With *tin* the direction of the current was the reverse, *i. e.* the iodide of potassium had the superiority, and indeed strongly. This is the more remarkable, as I convinced myself that the tin in the same acid is highly negative to the zinc.

A long discussion might here be opened; I will, however, merely touch upon one point. Faraday states that the addition of nitric acid to the sulphuric acid increases the *intensity of the chemical action*; and, after communicating some facts from which he draws the conclusion that this acid does not increase the *quantity* of the electricity, he adds: "This mode of increasing the intensity of the electric current, as it excludes the effect dependent upon many pairs of plates, or even the effect of making any one acid stronger or weaker, is at once referable to the conditions and force of the chemical affinities which are brought into action, and may, both in principle and practice, be considered as perfectly distinct from any other mode*."

Here we may with justice put the question, What measure then do we possess for the intensity of a chemical action? When the question is, as to the attack of an acid on a metal, we have, I believe, no other measure than the quantity of the metal which is dissolved from the unity of surface in the unity of time. But with this, certainly the most natural, view, there exists no reason why the nitric acid should enjoy any single advantage over the sulphuric acid, when these acids are taken of such a degree of concentration that they both dissolve just the same quantity of a like zinc surface in the same time. An advantage, according to Faraday's theory, is the less to be expected, as both acids are non-electrolytes, and their effect therefore could only be of like nature, and merely consist in increasing the affinity of the oxygen of the water for the zinc†. But

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* Faraday's Experimental Researches, § 908. It may be observed *en passant* that what the Faradayan theory terms the increase of the *quantity* of the electricity is the same as heightening the force of the current by diminishing the resistance; for instance, enlarging the surfaces, increasing the concentration of the fluids, therefore precisely the same as diminishing the denominator of Ohm's formula. By *electrolytic intensity*, or *intensity* of the electricity, this theory on the other hand understands, at least with the simple circuit, the electromotive force, or the numerator of this formula. But both expressions are sometimes used in a different sense, of which I have already given an example in the *Annalen*, vol. xlvii. p. 128*., and of which the explanation of the difference between the current of the pile, and that of the simple circuit (Exp. Res. § 994),—an explanation so perfectly simple, according to Ohm's theory, —gives a further proof.

† However, with the nitric acid, whether employed alone, or mixed with sulphuric acid, the process is not so simple, even with the above-mentioned

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[* A Translation of the Memoir of Ohm has just appeared in Part VII. of the Scientific Memoirs.—Edit.]

since, nevertheless, a specific distinction remains between the effects of the two acids, the one, added to the water, developing a slighter, and the other a greater electromotive force than the iodide of potassium, we should be forced to admit that the *quality* of the chemical action produces a specific difference in the excited electricity, and should thus again be brought back to the position maintained by De la Rive, but hitherto not proved, of the variety of electricities. I know not whether this is the opinion of the English philosopher; but the above-mentioned position, and another in which he expresses as a conjecture, "The same quantity of electricity may pass in the same time, in at the same surface, into the same decomposing body in the same state, and yet, differing in intensity, will decompose in one case, and in the other not,"*—would admit of such a construction.

But be this as it may, so much is certain, that there is no need of the hypothesis of an increase of the intensity of the chemical action, in order to explain the experiment in question. I have in fact convinced myself in the most positive manner *that the result of the addition of the nitric acid does decidedly not arise from the chemical attack of this acid on the* zinc, *but solely from an action of it on the* platina.

Instead of placing the *zinc* and *platina* in common in the stronger mixture of acids mentioned at p. 545, I separated the two acids by animal membrane (bladder), inserted the zinc (amalgamated) in the sulphuric acid (1 vol. concentrated acid, and 4 vol. water), and the platina in the nitric acid (1 vol. concent. acid, and 6 vol. water), while the two other plates, zinc and platina, stood in the solution of iodide of potassium. Now although in this case the zinc underwent no other attack in quantity and quality than in the experiment mentioned at p. 543, in which the iodide of potassium had the ascendency over the sulphuric acid, yet the direction of the current was the reverse; the iodide of potassium succumbed to the acid. The current also possessed a very considerable intensity, and, if not quite so powerful as in the case in which the zinc stood in the acid mixture, this, evidently, merely arose from collateral circumstances, partly from the separated acids having perhaps a weaker power of conduction than the mixed, partly, and without doubt chiefly, from the metals in the present arrangement being in a somewhat disadvantageous position, the communication between the two being made only by the membranous

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moderate degrees of concentration; for it is at least in part decomposed, which is indicated by the altered development of gas at the zinc, and more decidedly evident from the ammonia, the existence of which in the solution of zinc may be distinctly demonstrated by the addition of an excess of caustic potassa.

* Faraday, Experim. Res. § 988.

bottom of a cylinder which contained the platina and nitric acid, and was surrounded by a wider one which received the zinc and sulphuric acid.

To be perfectly certain that the mixed acids did not develope greater electromotive force than the separated, I caused them to oppose one another by substituting in the apparatus just described an acid mixture for the iodide of potassium. The experiment was in other respects similar to the previous one, only that the acids had a somewhat different degree of concentration. Both diluted acids consisted of 1 part by weight of concentrated acid, and 3 parts by weight of water, and equal parts by weight of them were mixed on the one side of the circuit with each other, and separated on the other side by bladder. The experiment was made both with amalgamated and non-amalgamated zinc, and previously heated platina.

In both cases the result was, *that the separated acids not only excite an electromotive force quite as great as the mixed, but have indeed a slight superiority over these!* The latter fact is the more remarkable, as the zinc plate (even amalgamated), immersed in the sulphuric acid containing nitric acid, is evidently more strongly attacked than that in the pure acid, and yet, after washing both in water, is in this fluid negative towards the latter plate.

I consider these facts, indeed, as more demonstrative than those already mentioned at p. 541, with the hydrochloric acid; nay, as so decisive, that I regard the proofs against the tenability of the argument, derived from Faraday's experiment, in favour of the chemical theory of galvanism, as perfectly destroyed by them*.

However I cannot refrain from drawing attention to the circuits composed of *acid, iodide of potassium, amalgamated* and *non-amalgamated zinc.*

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* The above fact is certainly decisive against the Faradayan theory, which merely admits the chemical attack on *metals* as the cause of the voltaic electricity. On the other hand, according to the theory of Becquerel or De la Rive, one might deduce this near equality in the action of the separated and mixed acid from an accidental compensation with the current originating from the contact of both acids. Now fluids excite, it is true, an electric current by their reciprocal contact, as was first *actually proved* by Fechner (Poggendorff's *Annalen*, xlviii. pp. 1. and 225.). A portion of the action may therefore in effect have originated from this cause; but since the currents, which truly originate from the reciprocal contact of the fluids, are always weak only, it is not probable that this portion was considerable, and exercised any great influence on the main result. As already mentioned, the separated acids have the superiority over the mixed when the *platina* is inserted in the nitric acid; the reverse happens when the *zinc* is immersed in the nitric acid. But in both cases the superiority is only slight. This appears to me to prove, that the current from the fluids which in both cases must possess opposite direction, has no considerable part in the main action.

As evident from the Table, the current has in these circuits, on employing *pure sulphuric acid* or *hydrochloric acid* in the diluted state, after a first deflexion in the direction $s < i$, the direction $s > i$ with great energy, or the acid the ascendency over the iodide of potassium. The same is the case, and indeed without the first deflexion $s < i$, when *sulphuric acid containing nitric acid* or *pure concentrated hydrochloric acid* (spec. gr. 1·138) is employed.—In all these cases, therefore, the *non-amalgamated zinc* acts as a negative metal, for instance like silver, towards the *amalgamated*; and yet it is always attacked far more energetically than the latter; by the concentrated hydrochloric acid, indeed, with a truly stormy violence. How can this be explained in a satisfactory manner according to the chemical theory?

I say in a satisfactory manner; for the explanation which Faraday has given of the cause of the positiveness or greater activity of the amalgamated zinc in comparison with the unamalgamated,—namely, that the latter, being directly attacked by the acids, neutralizes them by the oxide it produces, and thus retards the progress of oxidation, whilst at the surface of the amalgamated zinc the oxide formed is instantly removed by the free acid present, and the clean metallic surface is always ready to act with full energy upon the water*,—can scarcely be termed satisfactory, as it is in open contradiction to experience, which shows that under like circumstances by far more of the unamalgamated than of the amalgamated zinc is dissolved.

Just as little can the doctrine of *local* and *circulating* chemical forces, and the assumption that the latter are produced in greater energy or quantity by the amalgamated zinc than by the unamalgamated†, be admitted here, or generally, as valid. This doctrine possesses, it is true, such pliancy, that by it all the numerous cases, where, as in the experiment of Berzelius (p. 486.), the negative metal is more violently attacked than the positive, may be set aside, with the explanation that it is effected by a local action which adds nothing to the current; but, on a closer view, it is nothing more than a gratuitous hypothesis, which the chemical theory finds itself compelled to adopt in order not most palpably to fall into a contradiction with the fact, that the energy of the electromotive force no ways corresponds to the violence of the attack on the zinc or positive metal. Where is there any proof of this? It is as much in want of one, as the hypothesis imagined by De la Rive to get rid of the same difficulty, viz., that the electricities separated by the chemical process find a partial reunion at the very place where they are developed, and consequently the intensity of the current need not necessarily stand in direct proportion to the energy of this process,—an hypothesis which has already been dissected by Fechner*, and, in my opinion, founders even upon this ground alone, that, *cæteris paribus*, the current is the more intense the better the fluid conducts, *i.e.* the easier this reunion can take place in it.

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* Exp. Res. § 1005.

† Exp. Res. §§ 947, 996, 1120.

It is certainly an advantage of the contact-theory that it needs neither the one nor the other hypothesis, but is perfectly satisfied with the simple view, that the so-called local action, that which happens even previously to the closing of the circuit, is a pure chemical process not at all appertaining to the circuit: but the advantage were but slight, if it could merely enumerate in its favour the simplicity of this view; its real superiority over the chemical theories it acquires from its being a view well founded on facts. All cases more accurately examined, whether it be in the present Memoir or in previous ones by Fechner† and others, *prove* in the most evident manner that the energy of the direct chemical attack of the fluid on the positive metal does in no way stand in any connexion with the intensity of the excited electromotive force. And on the other hand it is *not proved* that the local action is ever converted into circulating, or weakened by it‡. What has been advanced as such, is evidently founded on error. The decrease of the hydrogen at the zinc which results on the closing of the circuit, does not happen from a *transfer* of *this* hydrogen to the negative metal, but simply from the oxygen being carried by the current to the zinc, and there combining with the hydrogen. I hope shortly to be able to confirm this by facts.

The following note has been communicated by M. Poggendorff to the Translator, with a request that it should be added:

As I did not foresee on penning this memoir that it would have the honour of being translated into English, it may be necessary to observe that I had not the intention of bringing forward a complete refutation of the chemical theory of galvanism, but merely to show that two facts recently brought forward in favour of this theory do not prove what it is intended they should. For the same reason many things have been passed over, or but briefly noticed, which appeared unnecessary for Germany, but which needed a more detailed exposition for the English readers, who in general are unacquainted with the researches of Ohm and Fechner. Among others, might here be enumerated the distinction between *electromotive force* and *intensity of current*, the not taking which into consideration has already given rise to so many errors; further, the resistance of *opposition*, an element hitherto little attended to in England[*], which is not merely in itself of importance, but to which especial regard must be had in the discussion of the two views respecting the origin of *voltaic* electricity, as it is considerably affected by the chemical action. Very generally the heightening of the intensity of the current by direct chemical attack on the one or other metal of the circuit is merely due to the diminution of the resistance of transition, and is not a result of the increase of the electromotive force. But it must be proved with respect to this force, that it is in direct ratio to the energy of the chemical action on one of the metals (or to the difference of the actions on both metals) of the circuit, if the chemical theory is to be regarded as founded. It would, however, then be requisite to continue to separate and quantitatively to determine the individual elements which have any influence on the intensity of the current, in the same way as it has been done by Fechner in his work, "*Maas bestimmungen über die Galvanische Kette* (Leipzic, 1831, 260 pages in quarto). I am convinced that the English physicists, to whose zeal and ability we are already indebted for so many interesting facts in the field of galvanism, would add considerably to the extension of our scientific knowledge of this branch of physics, and would consider Fechner's memoir on the Contact-theory[†] in a different point of view were they more intimately acquainted with that work, based on Ohm's theory, and illustrating and extending it.

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* Poggendorff's *Annalen*, vol. xlv. p. 232.

† For instance, Poggendorff's *Annalen*, vol. xliii. p. 433.

‡ Exp. Researches, § 996.

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[*] By which, among other things, the recent, important, and interesting discovery of Mr. Roberts (See L. & E. Phil. Mag. for Feb.) finds its explanation.

[†] Besides the article on the Contact-theory, translated in the Phil. Mag., might be mentioned two others in Poggendorff's *Annalen*, vol. xlv. p. 432, and vol. xlix. p. 433. What Schœnbein has brought forward against it does not appear to me to have any weight, and indicates his non-acquaintance with Ohm's theory.

LXXXIV. *Contributions to Electricity and Magnetism. No. III. On Electro-dynamic Induction.* *By* JOSEPH HENRY, *LL.D., Prof. of Natural Philosophy in the College of New Jersey, Princeton.*

[Continued from p. 265, and concluded.]

SECTION VI.—*The production of induced Currents of the different Orders from ordinary Electricity.*

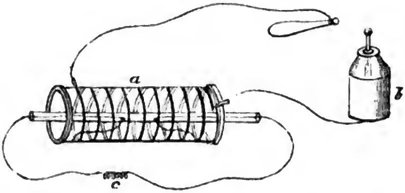
98. DR. FARADAY, in the Ninth Series of his Researches, remarks, that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact, that the secondary current, which exists but for a moment, could induce another current

of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this, a hollow glass cylinder, fig. 11, of about six inches in diameter, was prepared with a narrow riband of tinfoil, about thirty feet long, pasted spirally around the outside, and a similar riband of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner riband were joined by the magnetizing spiral (11.), containing a needle, and a discharge from a half-gallon jar sent through the outer riband, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner riband in the same direction as that of the current of the jar.* This experiment was repeated many times, and always with the same result.

Fig. 11.



a glass cylinder, *b* Leyden jar, *c* magnetizing spiral.

101. When the ends of one of the ribands were placed very nearly in contact, a small spark was perceived at the opening, the moment the discharge took place through the other riband.

102. When the ends of the same riband were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball, or the knuckle.

103. Also if the ends of the outer riband were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner riband.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps

necessary to recall the well-known fact, that when the knob of a jar is electrified positively, and the outer coating in connexion with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner riband became for an instant charged with this free electricity, and consequently threw off from the outer riband, by ordinary induction, the sparks described. It therefore became a question of importance to determine, whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connexion with the earth, and again with the knob in connexion with the same, so that the extra quantity might be in the one case *plus* and in the other *minus*; but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When, however, the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism appeared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the riband, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer riband of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phænomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribands of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer riband of the first cylinder, a tertiary current was induced in the inner riband of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last-mentioned riband.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in fig. 9. For these experiments, however, the coils were furnished with a double coating of silk, and the contiguous conductors separated by a large plate of glass.

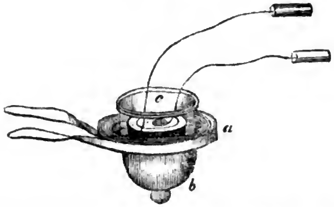
110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, figures 8 and 9. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed; but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in fig. 12. Helices No. 2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks, however, were very different in intensity with different discharges from the jar. In some cases no shock was received, when again, with a less charge, a severe one was obtained. But these irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a remarkable difference in the action of the two. I allude to

Fig. 12.



a coil No. 2, *b* an inverted bell glass, *c* helices No. 2 and 3.

the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or, in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (fig. 9) the discrepancy vanished. *Alternations were found the same as in the case of galvanism.* This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results, however, were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders[*]. In the former the distance between the ribands was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{20}$th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about $\frac{1}{30}$th of an inch. When a discharge from the half-gallon jar was passed through one of these, an induced current in the

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[*] This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

same direction was obtained from the other. The ribands were then separated, by plates of glass, to the distance of $\frac{1}{20}$th of an inch; the current was still in the same direction, or *plus*. When the distance was increased to about $\frac{1}{8}$th of an inch, no induced current could be obtained; and when they were still further separated the current again appeared, but was now *found to have a different direction, or to be minus*. No other change was observed in the direction of the current; the intensity of the induction decreased as the ribands were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribands.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point, a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were, however, in many cases, anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

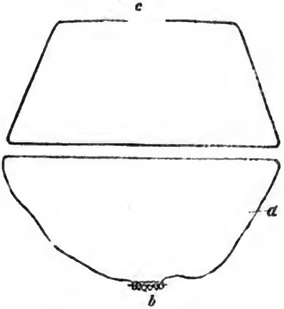
119. With a single half-gallon jar, and the conductors separated to a distance less than $\frac{1}{20}$th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity; and also on the length and thickness of the conductors. With a battery of eight half-gallon jars, and parallel wires of about ten feet long, the change in the direction did not take place at a less distance than from twelve to fifteen inches, and with a still

larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases, however, the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture-room of my respected friend Dr. Hare of Philadelphia, with the splendid electrical apparatus described in the fifth volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{10}$th of an inch in diameter and eighty feet in length, was stretched across the lecture-room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in fig. 13. The two wires were at first placed within the distance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*; but when the circuit was complete, the needle at each discharge

Fig. 13.



c place of the battery, *b* spiral.

indicated a current in the same direction as that of the battery. When the distance of the two wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to inclose it. The magnetism by this arrangement appeared stronger than with the last; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123. The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognised, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

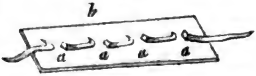
124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analogous effect in its own wire; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute in 1836. Two kites were attached, one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a con-

venient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.

126. It is well known that the discharge from an electrical battery possesses great divellent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampère, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass at the places so cut. Pass a discharge through the tinfoil from about nine half-gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force, between them. This will be understood by a reference to fig. 14; the ends are shown bent back at *a*, *a*, *a*, *a*. In the popular experiment of the pierced card, the bur on each side appears to be due to an action of the same kind.

Fig. 14.



b glass plate; *a*, *a*, *a*, *a*, openings in tinfoil.

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from

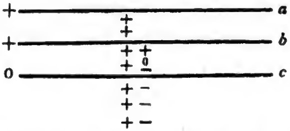
galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

128. To test this, the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer riband of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus.* With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents, as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV. may now be more definitely explained by saying, that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire) it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

Fig. 15.

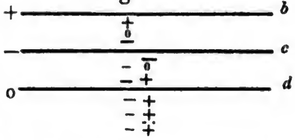


131. This will be rendered perfectly clear by fig. 15, in which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters + + +, &c., beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those + 0 — —, &c., beginning at the second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the

figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

132. Fig. 16 indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction of the influence, as before, is indicated by + 0 — —, &c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If, however, *d* is placed sufficiently near *c*, then neutralization will not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced, were the wire *c*, in fig. 15, placed sufficiently near the conductor *b*.

Fig. 16.



133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phænomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary, on the alternate magnetism of steel needles, placed at different distances from the line of a discharge of ordinary electricity*, and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris of Plymouth †. A comparative study of the phænomena observed by these distinguished *savants*, and those given in this paper, would probably lead to some new and important developments. Indeed every part of the subject of electro-dynamic induction

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* *Annales de Chimie et de Physique*, 1827.

† Philosophical Transactions, 1831, [noticed in Phil. Mag. and Annals, N. S. Vol. x. p. 297-298.]

appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

Note.—On the evening of the meeting at which my investigations were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the development of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above-named philosopher.

LXXXV.—*On the Heat of Vapours and on Astronomical Refractions. By* John William Lubbock, *Esq., Treas. R.S. F.R.A.S and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from p. 514.]

ON THE STEAM-ENGINE.

THE law which connects the pressure and the temperature of steam having been unknown, various empirical rules have been given. As, however, the expressions which arise are not in a convenient form for the calculations which are required in order to ascertain the *duty* which steam-engines are capable of performing, or to solve other problems of the same nature, M. de Pambour*, in his work on that subject, has employed another expression, viz.

$$\mu = \frac{1}{\varrho} = \frac{1}{n + q\,p},$$

in which ϱ is the density of steam, p the pressure, and n and q constants. According to my expression

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* *Théorie de la Machine à Vapeur*, p. 111.

$$\frac{1}{\varrho} = \frac{(1 - E)}{p^{\frac{1}{\gamma}} - E\,p}.$$

The pressure being reckoned in atmospheres, and the density of steam corresponding to the pressure of one atmosphere (or 14·706 lbs. per square inch) being unity. If we take the density of water for unity, then as the volume of steam at the pressure of one atmosphere is 1700 times greater than that of the same weight of water,

$$\mu = -\frac{K}{p^{\frac{1}{\gamma}} - E\,p} \qquad K = 1700\,(E - 1)$$

$$\log K = 2{\cdot}4765041 \qquad \frac{1}{\gamma} = 1{\cdot}0134 \qquad E = 1{\cdot}17602.$$

If we suppose that a certain volume of water represented by S be transformed into vapour at the pressure p, and that M is the absolute volume of vapour which results, we shall have

$$\frac{M}{S} = \mu = \frac{[0{\cdot}4109002]}{p}\left(\frac{1}{\alpha} + \theta\right).$$

If afterwards the same volume of water is transformed into vapour at the pressure p', and that the absolute volume which the resulting vapour occupies be called M', we shall have

$$\frac{M'}{S} = \mu'$$

$$\frac{M}{M'} = \frac{p'^{\frac{1}{\gamma}} - E\,p}{p^{\frac{1}{\gamma}} - E\,p}$$

" Soit* P la pression *totale* de la vapeur dans la chaudière, et p' la pression qu'aura cette vapeur à son arrivée dans le cylindre, pression qui sera toujours moindre que P, excepté dans un cas particulier que nous traiterons plus loin. La vapeur pénétrera donc dans le cylindre à la pression p', et elle continuera d'affluer avec cette pression et de produire un effet correspondant, jusqu'à ce que la communication entre la chaudière et le cylindre soit interceptée. Alors il cessera d'arriver de la vapeur nouvelle dans le cylindre, mais celle qui y est déjà parvenue, commencera à se dilater pendant le reste de la course du piston, en produisant par sa détente une certaine quantité de travail, qui s'ajoutera à celle déjà produite pendant la période d'admission de la vapeur.

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* The reasoning here is taken from M. de Pambour's work.

"*P* étant la pression de la vapeur dans la chaudière, et p' la pression qu'elle prendra à son arrivée dans le cylindre avant la détente, soit π la pression de cette vapeur en un point quelconque de la détente. Soit en même temps l la longueur totale de la course du piston, l' la portion parcourue au moment où a commencé la détente, et λ celle qui correspond au point où la vapeur a acquis la pression π. Enfin, soit encore a l'aire du piston, et c la liberté du cylindre, c'est-à-dire l'espace libre qui existe à chaque bout du cylindre, au-delà de la portion parcourue par le piston, et qui se remplit nécessairement de vapeur à chaque course; cet espace, y compris les passages aboutissants, étant représenté par une longueur équivalente du cylindre.

"Si l'on prend le piston au moment où la longueur de course parcourue est λ, et la pression π, on verra que si le piston parcourt, en outre, un espace élémentaire $d\,\lambda$, le travail élémentaire produit dans ce mouvement sera $\pi\, a\, \mathrm{d}\,\lambda$. Mais en même temps, le volume $a\,(l' + c)$ occupé par le vapeur avant la détente sera devenu $a\,(\lambda + c)$." Hence,

$$\frac{M}{M'} = \frac{p'^{\frac{1}{\gamma}} - E\,p'}{\pi^{\frac{1}{\gamma}} - E\,\pi} = \frac{\lambda + c}{l' + c}.$$

$$\lambda + c = (l' + c)\,\frac{(p'^{\frac{1}{\gamma}} - E\,p')}{(\pi^{\frac{1}{\gamma}} - E\,\pi)}$$

The elementary work produced $= \pi\, a\, \mathrm{d}\,\lambda$.

$$\int \pi\, a\, \mathrm{d}\,\lambda = \pi\, a\,(\lambda + c) - \int a\,(\lambda + c)\,\mathrm{d}\,\pi$$

$$= \pi\, a\,(\lambda + c) - \int a\,(l' + c)\,(p'^{\frac{1}{\gamma}} - E\,p')\,\frac{\mathrm{d}\,\varpi}{(\pi^{\frac{1}{\gamma}} - E\,\pi)}$$

$$= \pi\, a\,(\lambda + c) - a\,(l' + c)\,(p'^{\frac{1}{\gamma}} - E\,p')\int \frac{\mathrm{d}\,\varpi}{\pi^{\frac{1}{\gamma}} - E\,\pi}$$

$$= \pi\, a\,(\lambda + c)$$

$$+ a\,(l' + c)\,\frac{(\,p'^{\frac{1}{\gamma}} - E\,p')\,\gamma}{E\,(\gamma - 1)}\ \log\,(1 - E\,\pi^{\frac{\gamma - 1}{\gamma}}) + \text{const.}$$

This integral is to be taken from $\lambda = l'$ to $\lambda' = l$, let $\pi = p$ when $\lambda = l$, when $\lambda = l'$, $\pi = p'$.

$$\int \pi\, a\, \mathrm{d}\lambda = p\, a\, (l + c) - p'\, a\, (l' + c)$$

$$+ a\,(l' + c)\,\frac{\left(p'^{\frac{1}{\gamma}} - E\,p'\right)\gamma}{E\,(\gamma - 1)}\,\log\left\{\frac{1 - E\,p^{\frac{\gamma-1}{\gamma}}}{1 - E\,p'^{\frac{\gamma-1}{\gamma}}}\right\}$$

for the values of the constants E, γ. See p. 511.

To this must be added the work effected during the course of the piston through l', which is $p'\,a\,l$, and if R is the total pressure exerted upon unity of surface of the piston

$$p\,a\,(l + c) - p'\,a\,c$$

$$+ a\,(l' + c)\,\left(p'^{\frac{1}{\gamma}} - E\,p'\right)\,\frac{\gamma}{E\,(\gamma - 1)}\,\log\left\{\frac{1 - E\,p^{\frac{\gamma-1}{\gamma}}}{1 - E\,p'^{\frac{\gamma-1}{\gamma}}}\right\}$$

$$= a\,R\,l \qquad \text{(A)}^{*}$$

$R = (1 + \delta)\,r + p'' + f$, f is the friction of the machine not loaded, δ the increase of this friction due to unity of the charge r, p'' the pressure on the surface of the piston, representing the atmospheric pressure when the machine works without condensation, and otherwise the pressure of condensation in the cylinder.

If S denote the volume of water converted into vapour by the boiler in unity of time, this volume in the cylinder becomes

$$-\frac{S\,K}{p'^{\frac{1}{\gamma}} - E\,p'}$$

K being the same constant as in p. 563.

It is evident, according to the reasoning of M. de Pambour, in p. 125. of his work, that if v denote the velocity of the piston

$$\frac{S\,K}{p'^{\frac{1}{\gamma}} - E\,p'} = -\,v\,a\,\frac{l' + c}{l} \qquad \text{(B.)}$$

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[*] This equation is equivalent to the equation (A) of M. de Pambour, p. 123, which may be put into the form

$$p\,a\,(l + c) - p'\,a\,c + \frac{l\,S}{q\,v}\ \text{Nap. log}\ \left\{\frac{l + c}{l' + c}\right\} = a\,R\,l.$$

or $$p\,a\,(l + c) - p'\,a\,c + [6{\cdot}9505960]\,\frac{l\,S}{v}\ \log\ \left\{\frac{l + c}{l' + c}\right\} = a\,R\,l,$$

the pressure being reckoned in lbs. per square inch.

$$p'^{\frac{1}{\gamma}} - E p' = - \frac{l S K}{a v (l' + c)} = \frac{K}{\mu'},$$

Similarly,

$$p^{\frac{1}{\gamma}} - E p = - \frac{l S K}{a v (l + c)} = \frac{K}{\mu}$$

$$\frac{1}{\mu} = \frac{l S}{a v (l + c)} \qquad \frac{1}{\mu'} = \frac{l S}{a v (l' + c)}$$

$$p a (l + c) - p' a c - \frac{l S K \gamma}{v E (1 - \gamma)} \text{ Nap. log} \left\{ \frac{1 - E p'^{\frac{\gamma - 1}{\gamma}}}{1 - E p^{\frac{\gamma - 1}{\gamma}}} \right\} = a R l$$

$$p a (l + c) - p' a c - (p'' + f) a l - [4{\cdot}6411966] \frac{l S}{v} \log {}^{*} \left\{ \frac{1 - E p'^{\frac{\gamma - 1}{\gamma}}}{1 - E p^{\frac{\gamma - 1}{\gamma}}} \right\} = a l (1 + \delta) r$$

$$\log \left\{ \frac{1 - E p'^{\frac{\gamma - 1}{\gamma}}}{1 - E p^{\frac{\gamma - 1}{\gamma}}} \right\} = \log \left\{ \frac{l + c}{l' + c} \right\} - \frac{1}{\gamma} \log \left(\frac{p'}{p} \right).$$

If the machine work without expansion,

$$p = p', \qquad \log \left\{ \frac{1 - E p'^{\frac{\gamma - 1}{\gamma}}}{1 - E p^{\frac{\gamma - 1}{\gamma}}} \right\} = 0,$$

$$r = \frac{p - (p'' + f)}{1 + \delta}$$

The data upon questions relating to the steam-engine are the quantities a, l, l', S, and v, and it is evident that from these quantities the quantities μ and μ' may at once be found by an easy arithmetical operation; from these the following table will give the corresponding pressures p and p', and these pressures being introduced into equation A, the value of $a\ r$ may be easily found.

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* Log. of Briggs, the pressure being reckoned in atmospheres, the log. of the constant is [7·9670537], the pressure being reckoned in lbs. per square foot.

Table showing the volume (compared with that of water at 212°), and the temperature of steam.

Pressure in lbs. per square inch. 14·706 × p.	Temperature. Fahrenheit. τ		Volume. μ†	Pressure in lbs. per square inch. 14·706 × p.	Temperature. Fahrenheit. τ		Volume. μ
	Air*. Therm.	Merc. Therm.			Air. Therm.	Merc. Therm.	
1	101°·5	101°·5	20816	56	287°·6	289°·5	498
2	126·0	126·0	10871	57	288·7	290·7	490
3	141·4	141·4	7442	58	289·8	291·8	482
4	153·0	153·0	5691	59	290·9	293·0	474
5	162·2	162·2	4622	60	292·0	294·1	467
6	170·1	170·1	3902	61	293·0	295·1	460
7	176·8	176·8	3381	62	294·0	296·2	453
8	182·8	182·8	2986	63	295·0	297·2	447
9	188·3	188·3	2678	64	296·0	298·2	440
10	193·2	193·2	2429	65	297·0	299·2	434
11	197·8	197·8	2223	66	298·0	300·3	428
12	202·0	202·0	2051	67	299·0	301·3	422
13	205·9	205·9	1905	68	300·0	302·3	417
14	209·5	209·5	1778	69	301·0	303·3	411
14·706	212·0	212·0	1700	70	301·9	304·2	406
15	212·9	212·9	1669	71	302·8	305·2	401
16	216·3	216·4	1572	72	303·7	306·1	396
17	219·3	219·5	1487	73	304·6	307·0	391
18	222·3	222·6	1410	74	305·5	308·0	386
19	225·1	225·5	1342	75	306·4	308·9	381
20	227·9	228·3	1280	76	307·3	309·8	377
21	230·4	230·9	1224	77	308·2	310·7	372
22	232·9	233·5	1172	78	309·1	311·6	368
23	235·2	235·8	1125	79	310·0	312·6	364
24	237·6	238·3	1082	80	310·9	313·5	359
25	239·8	240·6	1042	81	311·8	314·5	355
26	242·0	242·8	1005	82	312·7	315·4	351
27	244·0	244·9	971	83	313·5	316·3	348
28	246·1	247·0	939	84	314·3	317·1	344
29	248·0	249·0	909	85	315·1	317·9	340
30	250·0	251·0	881	86	315·9	318·7	337
31	251·8	252·8	855	87	316·7	319·6	333
32	253·7	254·8	831	88	317·5	320·4	330
33	255·4	256·5	808	89	318·3	321·2	326
34	257·2	258·3	786	90	319·1	322·0	323
35	258·9	260·1	765	91	319·9	322·9	320
36	260·6	261·8	746	92	320·7	323·7	317
37	262·2	263·5	727	93	321·5	324·5	313
38	263·8	265·1	709	94	322·3	325·3	310
39	265·3	266·6	693	95	323·0	326·0	307
40	266·8	268·2	677	96	323·7	326·8	305
41	268·2	269·6	662	97	324·4	327·5	302
42	269·7	271·2	647	98	325·1	328·3	299
43	271·1	272·6	633	99	325·8	329·0	296
44	272·5	274·1	620	100	326·5	329·7	293
45	273·8	275·4	608	105	330·0	333·3	281
46	275·2	276·8	596	120	339·7	343·4	249
47	276·5	278·2	584	135	348·4	352·4	223
48	277·8	279·5	573	150	356·5	360·8	203
49	279·1	280·8	562	165	363·9	368·4	186
50	280·4	282·1	552	180	370·7	375·5	172
51	281·7	283·5	542	195	377·0	382·0	160
52	282·9	284·7	532	210	383·3	388·4	150
53	284·1	286·0	523	225	389·0	394·4	141
54	285·2	287·1	514	240	394·5	400·1	133
55	286·4	288·3	506				

* $\tau = -\frac{[2\cdot0651059]}{p^{\cdot0184}-1\cdot17602} - 448^\circ$

† $\mu = \frac{[0\cdot4109002]\left(\frac{1}{a}+\theta\right)}{}.$

The following example will serve to show in what manner the table was calculated.

Ex.—Calculation of the temperature and volume of steam for the pressure of 180 lbs. per square inch.

$$\begin{array}{rl}
\log 180 & = 2{\cdot}2552725 \\
\log 14{\cdot}706 & = 1{\cdot}1674946 \\
\hline
\log p & = 1{\cdot}0877779 \times {\cdot}0134 = {\cdot}01457622386 = \log 1{\cdot}03413 \\
 & 1{\cdot}17602 \\
 & 1{\cdot}03413 \qquad 2{\cdot}0651059 \\
\log\ {\cdot}14189 & = 9{\cdot}1519518 \\
\hline
 & 2{\cdot}9131541 = \log 818{\cdot}7 \\
 & 448{\cdot}0 \\
\hline
\text{Temp. Fahr. Air Therm.} & = 370{\cdot}7 \\
 & 4{\cdot}8 \\
\hline
\text{Temp. Fahr. Merc. Therm.} & = 375{\cdot}5 \\
 & 0{\cdot}4109002 \\
\log \frac{1}{p} & = 8{\cdot}9122221 \\
 & 2{\cdot}9131541 \\
\hline
 & 2{\cdot}2362764 = \log 172 \qquad \mu = 172
\end{array}$$

The following data are taken from M. de Pambour's work on the Steam Engine, p. 238.

$l' = {\cdot}25\, l$ $v = 250$ f $= {\cdot}5$ (lb. per square inch) $\delta = {\cdot}14$

$l = 10$ ft. $a = 12{\cdot}566$ sq. ft. $S = {\cdot}927$ cub. ft.

$p'' = 4$ lbs. per square inch. $c = {\cdot}05\, l$

$$\begin{array}{rlcrl}
\log\ v & = 2{\cdot}3979400 & \qquad & \log\ v & = 2{\cdot}3979400 \\
\log\ a & = 1{\cdot}0991971 & & \log\ a & = 1{\cdot}0991971 \\
\log {\cdot}30 & = 9{\cdot}4771213 & & \log 1{\cdot}05 & = 0{\cdot}0211893 \\
\hline
 & 2{\cdot}9742584 & & & 3{\cdot}5183264 \\
\log\ S & = 9{\cdot}9670797 & & \log\ S & = 9{\cdot}9670797 \\
\hline
 & 3{\cdot}0071787 = \log 1016{\cdot}6 & & & 3{\cdot}5512467 = \log 3558{\cdot}3 \\
 & \mu' = 1016{\cdot}6 & & & \mu = 3558{\cdot}3
\end{array}$$

Hence by the table p' = 25·686, p = 6·627 reckoned in lbs. per square inch.

p' = 1·7466, p = ·4506, $p'' + f$ = ·3060 *in atmospheres.*
log 1·7466 = ·2422019, log ·4506 = 9·6538224
·2422019 × ·0134 = ·00324550546

9·9967545
log E = 0·0704184

0·0671729 = log 1·16727

0·3461776 × ·0134 = ·00463877984
·0704184

·0750571 = log 1·18865

log ·18865 = 9·2756568
log ·16727 = 9·2234181

0·0522387

log p = 9·6538224	log p' = 0·2422019	log $(p'' + f)$ = 9·4857179
log a = 1·0991971	log a = 1·0991971	log a = 1·0991971
log $(l + c)$ = 1·0211893	log c = 9·6989700	log l = 1·0000000
1·7742088	1·0403690	1·5849150
59·457	10·974	38·451
	38·451	
	49·425	

4·6411966
log l = 1·0000000
log S = 9·9671554
8·7179923

4·3263443
log v = 2·3979400

1·9284043

84·802
59·457

144·259
49·425

94·834

log 94·834 = 1·9769641
log 14·706 = 1·1674946
log 144 = 2·1583625

5·3028212
log $l(1 + \delta)$ = 1·0569049

4·2459163
$a r$ = 17616

$a r$ = 17616 expressed in lbs., M. de Pambour finds $a r$ = 17337.

[To be continued.]

LXXXVI. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron.* *By* Dr. C. SCHAFHAEUTL, *of Munich.*

[Continued from p. 523, and concluded.]

IN scarcely any analytical proceedings has the presence of electro-negative metals been more overlooked, than in the analyses of cast iron, steel, and wrought iron: the best irons of Sweden contain, as we shall soon show, a considerable quantity of arsenic, and the celebrated English *Low-Moor* iron contains still more. By forging the best English cast steel, arsenic is volatilized and may be very easily detected by the smell; and the blacksmiths who forge *Low-Moor* iron, frequently complain of the unpleasant smell which escapes (by them termed a sulphureous smell), causing them often swelled lips. It is for this reason that the *Low-Moor* iron surpasses in hardness and tenacity all other English iron. The same iron is known for its capability of being converted into bar-steel for *coach-springs*, although not bearing a higher degree of conversion.

It is known that Wootz, or Indian steel, as well as cast steel made from Dannemora iron, is particularly adapted for cutting instruments which require an extremely sharp fine edge; but for purposes in which great tenacity is required, without a particularly fine grain, where the steel is required to be welded at an elevated degree of heat and in large masses, the celebrated Russian CCND iron is far preferable, containing besides a large quantity of silicon and manganese, also a large quantity of phosphorus.

To the presence of sulphur as well as arsenic is generally ascribed the property of the iron being red-short, and the sulphur particularly has in this respect a bad reputation. Karsten declares that even the presence of 0·03375 part of sulphur is capable of making the iron totally unfit for use in a red heat, because he caused sulphate of lime or gypsum to be melted down with iron ore in a blast furnace, and found the iron prepared from it perfectly red-short, containing only the small quantity of sulphur just mentioned. But he did not investigate the other contents of the red-short cast iron, which in such case contains always calcium or sulphuret of calcium in its composition as well as sulphuret of silicon. If such a small quantity of sulphur as Karsten mentions, would make iron red-short, no malleable iron at all could be produced by means of pit-coal, as even the softest and best English iron invariably contains more sulphur than the quantity mentioned by him. Charcoal itself imparts a portion of sulphur to the iron.

As a further proof of what has just been asserted, we now refer to some specimens of *French cast iron*, the produce of the furnaces near *Alais*, département du Gard, at the foot of the Cevennes.

These specimens were produced from hydrated oxides of iron, which cover the summits of several hills of limestone and carboniferous sandstone extending over a long district, and have undoubtedly been deposited in this state by water. The greatest part of this ore has a perfectly ochry appearance, intermixed with masses of red oxide of iron, which is so similar to oxide of iron, precipitated from its solution in acids by caustic ammonia, and dried on a filter, that it is impossible to distinguish between the two specimens when placed together, which is the artificial and which the natural. As it is the mode in France to assay the iron ores only in the *dry way* in a small crucible, lined with charcoal, nothing more is obtained than the quantity of metallic iron which is contained in the ore; and as those ores were found to be very rich and in great abundance, a large iron-work has in consequence been erected; but actual trial of the ore in the blast furnaces very soon convinced the proprietors, that in the selection of ore for *practical* purposes more research is required than the mere melting down of three grammes of iron ore in the crucible in the laboratory.

The iron obtained from this ore had invariably the bad quality of emitting a great quantity of fumes during heating, and not welding except in a state of half-fusion. But when this welding was accomplished, the iron naturally had lost its quality, and was found to be burnt. This ore is invariably mixed with a subarseniate of iron, which contains in 100 parts 7 of arsenic acid and 13·68 of water, and is mechanically mixed with galena, bournonite, and similar minerals. The small quantity of arsenic in this case is easily overlooked even with the blow-pipe, and generally no other means will detect the presence of arsenic except a current of sulphuretted hydrogen.

The manner of conducting the blast furnaces with pit coal in France is, notwithstanding the great difference in the ores, exactly the same as in England; and English workmen are down to the present time generally employed, not only in working at the puddling furnaces, but also at the blast furnaces.

It will be apparent, that the above-mentioned ore is extremely fusible, and requires therefore a very careful arrangement of the fluxes, in order to obtain a tolerably good quality of cast iron, particularly as in this hot southern climate the rarefied and dry air has a peculiar influence on the production of the blast furnaces under some circumstances, which I shall further explain; so that I witnessed for months a different working of the blast furnaces, even at different periods of the day.

Notwithstanding the profusion of rich ores throughout France, it is infinitely more difficult to produce iron of good quality from them, than from the clay ironstone in England; and excepting where iron is melted down with charcoal, the iron produced is of very inferior quality compared with that of England.

As I think it will be very instructive to examine the chemical properties of several specimens of such cast iron, obtained from the same ore and in the same furnace, I shall briefly describe five specimens of iron from the furnace of Alais.

I call the first (*a*); it has a dead gray appearance, but is intersected by somewhat whitish shining rays having a distant resemblance to the lamellar crystallization of white crystallized charcoal iron. It was rather hard and brittle, and its specific gravity was 7·442. The second specimen (*b*) was obtained under peculiar circumstances. During one cast, in particular, the iron ran from the hearth into the moulds in the sand, and the rapid contraction of the exterior of those pigs forced out the still liquid interior through the face of the pigs like a fountain. The iron thus forced out is the specimen (*b*); it had a silvery white appearance, broke with large crystalline planes, approaching somewhat to a cubical fracture, and had a specific gravity of 7·33.

The specimen (*c*) was also perfectly silver-white, consisting of an extremely large pearl-like granulation, easily to be broken, and its specific gravity was 7·582.

Specimen (*d*) was extremely difficult of fusion, scarcely to be melted down in the fineries, and not at all available in the puddling furnaces; its specific gravity was 7·61.

Specimen (*e*) is a malleable iron produced from gray cast iron, obtained only from a few casts during the beginning of the working of the blast furnace. Its qualities we shall afterwards describe.

On treating the specimen (*a*) with hydrochloric acid in the way before described, I observed, that during the last washing of the sulphuret of lead with boiling-hot distilled water, acidulated with hydrochloric acid, as soon as this water dropt beneath into the solution of nitrate of lead, the surface of the liquid assumed a beautiful bright vermilion-red colour during the formation of chloride of lead. As soon as a considerable quantity of the coloured fluid was collected, I decanted it into another glass and found next day that the colouring matter was deposited, traversed by beautiful red needle-like crystals; these crystals washed with water during solution lost their colour, were not soluble in alcohol, and heated in a glass tube emitted a very pungent smell somewhat resembling cyanogen.

The liquid solution of nitrate of lead smelt very strongly of hydrogen developed from cast iron, and I am convinced, that this compound of hydrogen, carbon, azote, and sulphur forms a salt with the oxide of lead, which is more distinct, when the acid, in which the cast iron is dissolved, is so weak, that little or no sulphuretted hydrogen is disengaged. When acetate of lead is used, and the lead carefully precipitated by sulphuric acid, this compound is separated from the lead and will be found to swim on the surface of the liquid. It is unfortunately too little to be collected, and the only way to glean some idea as to its composition is, to detonate the hydrogen with oxygen *.

Another remarkable circumstance is, that in determining the quantity of arsenic contained in the iron by boiling it in aqua-regia, dropping the neutralized filtered liquid into hydrosulphuret of ammonia, and decomposing it carefully with acid, the precipitated sulphur, during its solution in aqua-regia, deposited invariably a black scaly heavy substance, darker than graphite. From 12·89 grains of this sulphur, I separated 0·36 grains of these black scales. By heating them in a glass tube, sulphur was disengaged, and black dull scales remained equal to 0·1438 grains. These black scales were not soluble in any acid, and appeared through the microscope mixed with white transparent grains. A part of these scales heated on a platinum foil began to sparkle, glowed in a very lively manner, which after some time ceased, and a white powder remained, mixed with those transparent grains. The powder before the blowpipe appeared like silica, and the scales consisted therefore probably of sulphuret of silicon, or a mixture of sulphuret of silicon with metallic silicon; or it might perhaps be considered as a compound of sulphur, carbon, and silicon. If we consider it as a simple sulphuret of silicon, we might obtain the not very probable formula $\mathrm{Si}\,\overset{6}{\mathrm{S}}$.

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* A series of experiments, which I have made recently, and since the writing of the above paper (which only contains the experiments made while travelling through England and France), confirm entirely this opinion. I shall elucidate this subject further in a paper on the gases developed by treating iron with acids, and describe at the same time a new *portable* apparatus for analysing the compound radicals (of organic chemistry), by means of which the oxygen, hydrogen, carbon, and azote of the compound are easily determined in one operation only.

The remainder of the solution of this iron in hydrochloric acid, heated in the crucible, *showed no disposition to glow* like the specimens before-mentioned; it lay still dark on the bottom, after the crucible had become for a considerable time red-hot, and after four ignitions its appearance was but very slightly altered. It increased after the first ignition = 0·064

,, second ,, = 0·209
,, third ,, = 0·096
,, fourth ,, = 0·016

0·385

35 grains of the specimen (*a*) boiled in aqua-regia for five minutes, left a residuum only = 3·7625. It was this solution in which the before-mentioned black scales were generated.

The same quantity of iron treated in a retort with diluted nitric acid, left only 2·30 grains of residuum, without the development of any carbonic acid, which shows that during the solution a new compound is formed, of which azote and hydrogen form a prominent part.

By treating the same quantity of iron with still more dilute nitric acid, carbonic acid gas was evolved, and the residuum was found to be 3·144 grains.

From this residuum, boiled in a platinum dish with nitric acid, sulphur was very soon separated, which was removed after renewing the acid; and boiling it again till decomposition was entirely completed, perfectly white silica was separated, which in the course of drying on the filter, became interspersed with beautiful blue spots, having a resemblance to phosphate of iron.

The 2·3 grains residuum of the iron, treated in the second experiment in the retort, was, after the action of the acid had ceased, poured out with the acid into a china dish, and remained untouched for six weeks.

After the lapse of this time, a brown sediment was as usual found on the bottom, and in the middle of it a white mass, composed of gelatinous granulations, interspersed with brick-red or vermilion points, like vanadic acid. A great part of this gelatinous residuum retained the perfect form of the fragments of cast iron, the *skeleton* of which they formed.

These gelatinous fragments, when divided, showed the interior to consist of gelatinous layers, which from the second layer down to the centre were of a beautiful blue colour. They imparted to diluted hydrochloric acid when poured over them a green colour, which was destroyed by ammonia, and again revived by acids. Reagents as well as the blowpipe discovered nothing in the solution except protoxide of iron, carbon and azote, and the blue tints appeared to me therefore to form a cyanuret of iron (?).

The gelatinous mass, viewed through the microscope, consisted of an aggregation of gelatinous grains of silica, interspersed with grains at least ten times smaller of a vermilion colour. The blue layers had the same composition, with the exception, that the silica was penetrated by the before-mentioned blue colour.

From these experiments we can perceive very plainly the mechanical structure of the white cast iron, in which the close connexion of the silicon with carbon and azote is beautifully developed.

The disappearance of the blue colour from the layers on the outside during the contact of the acid and the air, shows the progressive formation and decomposition—as well as the interspersed brick-red grains resembling vanadic acid, and explains the mechanical arrangement of the different constituents of cast iron.

The specimen (*b*) shows other remarkable qualities. Treated with concentrated hydrochloric or nitric acid, the yellow powder, consisting of sulphur and silicon, as already mentioned in a foregoing part of this treatise, was invariably separated: 1·80 grains of this yellowish-gray powder, which swam as a viscid mass on the surface of the liquid, heated on platinum foil, lost 0·80 grains of sulphur. The remaining 1·00 of this yellowish-gray powder, ignited with carbonate of soda, was found to be contracted into a yellowish-brown cake, adhering very strongly to the crucible. Two grass-green drops of manganate of soda adhered to the lid; silica was separated amounting to 0·1611; oxide of iron = 0·855; alumina = 0·028.

If we consider the sulphur united with the iron, the formula resembles a bisulphuret of iron, and we have

Silicon	0·077
Sulphuret of iron . . .	0·945
Sulphur	0·800
	1·822

or, if we take the ingredients as they appear,

Silicon	0·07739
Aluminum	0·01342
Iron and manganese . .	0·85500
Sulphur	0·80000
	1·75081

This yellow powder separated was soluble in aqua-regia, as well as in caustic ammonia.

Further, I dissolved 20 grains of the present specimen of cast iron in aqua-regia; it was quickly and violently attacked, leaving a black coaly residuum. Nevertheless 1·11 grains of the iron remained undissolved, which I carefully washed and separated. The solution was then evaporated to dryness, mixed with five times its weight of soda, and exposed to a white heat in a charcoal fire. The mass was found afterwards to be of a yellowish-brown colour, intermixed with black lumps, and several white drops adhered to the lid. Acidulated water was poured over it, and the whole digested for some time. A light gray muddy liquid was the result; but the mass on the bottom was not much attacked. After changing the acid, its action was very soon stopped again. By adding more acid the action began again very violently; the whole mass in the crucible was converted into a black viscid mass, filling up the whole crucible, while disengaging carbonic acid with a particularly sharp hissing noise. I poured the whole into a china dish. In the green liquid I found swimming a black flocculent mass in considerable quantity. After some time hydrogen gas was developed, and the black mass began to diminish gradually in bulk till all traces of it were lost, and the colour of the liquid, green before, became changed into yellow.

The evolution of hydrogen proved that the black mass must have been a reduced metallic body, either silicon or iron. It is very curious that the alkali shewed such a reducing power, which seems to be derived from the carbonaceous matter combined with the oxide of iron; a case which never occurred to me at any other time.

The silica separated weighed = 0·5148
The oxide of iron ,, 8·9041

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9·4189

The chemical constituents of these examples, determined in the usual manner, are shown in the following table, to which I have added the preceding analysis for comparison:

Ingredients.	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	Iron (*a*).	Iron (*b*).	Iron (*c*).	Specimen (*d*).	Steel.
Silicon ...	4·86430	1·00867	1·0090	1·860	2·006	0·4828	2·9784	0·5204
Aluminum	1·00738	0·08571	0·0606	0·108	0·098	0·0134	0·0876	0·000
Carbon ...	3·38000	4·30000	1·9100	5·800	4·750	2·7500	4·269	1·42800
Azote......	0·00000	0·76371	0·7205	0·874	0·565	1·0360	0·6386	0·18310
Sulphur ...	0·17740	0·32018	1·1050	0·645	0·800	0·3800	0·433	1·00200
Arsenic ...	0·00000	0·00000	0·0000	4·050	2·560	4·0800	3·840	0·93400
Antimony .	0·00000	1·59710	0·0000	0·000	0·000	0·000	0·000	0·12100
Chrome ...	0·00000	0·00000	1·38200	0·000	0·000	0·000	0·000	0·00000

For 35 grains of those irons.

Ingredients.	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	Iron (*a*).	Iron (*b*).	Iron (*c*).	Specimen (*d*).	Steel.
Residuum in acids. .	5·53	6·7700	12·005	10·890	16·625	9·45	10·045	1·995
Increase of weight after ignition	0·000 (lost.)	0·9084	2·697	0·385	1·880	1·82	0·880	0·059
Flocculent powder after boiling with hydrochloric acid.....	4·762 gray.	0·6270 gray.	1·603 gray.	2·316 gray.	2·036 black.	0·485 black.	2·044 black.	0·381 gray.

Table inclusive of the increase of weight in the different states of ignition.

	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	(*a*).	(*b*).	(*c*). Loss.	Steel.
1st	0·000	0·173	0·000	0·064	0·1454	1·14	0·059
2nd......	0·000	0·584	0·450	0·209	0·8000	1·16	0·000
3rd	0·000	0·043	0·846	0·096	0·560	0·80	0·000
4th	0·000	0·000	0·810	0·016	0·218	0·00	0·000
5th	0·000	0·000	0·210	0·000	0·000	0·00	0·000
6th	0·000	0·000	0·162	0·000	0·000	0·00	0·000
7th	0·000	0·000	0·144	0·000	0·000	0·00	0·000
8th	0·000	0·000	0·018	0·000	0·000	0·00	0·000
9th	0·000	0·000	0·054	0·000	0·000	0·00	0·000
10th	0·000	0·000	0·000	0·000	0·000	0·00	0·000

If we treat powdered iron with a current of dry chlorine, a *quantity of silicon* is always retained by the remaining carbon and azote; and this is likewise a proof that a certain quantity of silicon is chemically combined with the carbon, as no alkali has the power to extract it*.

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* In specimens in which the silicon is combined with the iron, the silicon is left after the solution of the iron in acids, in the form of a white and somewhat gelatinous granulation, as we saw in the gray iron from Vienne. On the contrary, where the carbon is combined with the silicon in not too large proportions, even a white heat is insufficient to burn this carbon, as we had examples in the black second remainders of the iron, (*b*), (*c*), and (*d*).

Ingredients.	Gray French Iron.	White Welsh Iron.	Iron from Creuzot.	Iron (*a*).	*b.*	*c.*	*d.*	Steel.
Carbon and Azote	3·412	5·3920	2·086	6·5000	5·2696	4·02	3·5190	1·7265
Silicon	0·138	*1·008	0·702	0·4804	0·4804	0·23	0·4804	0·2740
Last residuums after ignition and extraction by acids.								
Carbon, Hydrogen and Oxygen.....	3·700	3·276	(3·580)	4·7600	3·811	0·912	2·9754	0·3230
Silicon	4·812	1·009	1·000	1·8500	2·006	0·473	2·8671	0·5191
	gray.	gray.	gray.					

We here see the combination of carbon with silicon, the quantity of which is almost always constant. If we further consider the different relations of the numbers in the above tables, we find that the white irons (*a*) (*b*) (*c*) (*d*) are characterized very distinctly by the different proportions of carbon and silicon. Specimen (*c*) was very difficult to treat in the refining furnace, as well as in the puddling oven, and was of a large roundish grain, silver white, and contracting very much during the process of cooling. Specimen (*d*) was almost infusible in the refining as well as the puddling fires, and producing an iron red-short as well as cold-short, which would not weld under any circumstances. The quantities of sulphur and arsenic cannot be the cause of these singularities, as both ingredients are found in a less proportion in (*c*) and (*d*) than in the specimens (*a*) and (*b*). The silicon alone is predominant in the last specimen; but if we compare its quantity of silicon with that contained in the first analysed gray iron from Vienne, we find it there in twice as great a proportion. Nevertheless the iron ran as liquid as water. The real cause of the difference in quality of those two last specimens must therefore be sought for in the different proportions of carbon and silicon combined, and, in reality, the tables show us, that the quantities of silicon in relation to the carbon increase in proportion as the different specimens become less and less fusible. It therefore naturally follows, that the more carbon becomes combined with silicon, the less carbon will be combined with the iron, and consequently our residuums will show the property of becoming ignited by a low degree of heat, and, in fact, the residuum of the last specimen lost after the first ignition instead of gaining as all the other specimens did, and the gain after the first ignition was extremely small in comparison with that of all the other specimens mentioned in the beginning.

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* If we compare the silicon left by chlorine in the white Welsh iron, we find it equal to the actual determined quantity of silicon. In all other specimens the chlorine had carried away all or a great part of the silicon, viz. where it was combined with the iron, as in the cast iron from Vienne. But in this specimen only almost the whole of the silicon seemed to be combined *with carbon*, on which combination chlorine never acts.

If we look back to the specimen of iron first analysed at the Maesteg iron-works, we shall find, that we came to the conclusion, that the second part of this residuum, which during the first ignition neither gained nor lost weight, was combined as follows, in three grains:

Metallic carburet of iron . .	1·040728
Metallic iron	1·12268
Carbon	0·44138
Silicon	0·159500
Carbon	0·270500

In fact, this property of thus becoming ignited depends entirely on the carburet of iron. The residuum from acids loses this facility of being burnt or oxidised, as well as the corresponding iron in the puddling and boiling furnace, and the difficulty of *converting cast iron* into *malleable iron* increases in the ratio of the diminution of the carburet of iron in the cast irons.

The specimen (*e*) was made from gray cast iron, produced in the same furnace from the same ore. It melted in the puddling furnace into a very liquid state, and filled the oven during its ebullition with innumerable brilliant sparks, emitting a hissing sound as if a great mass of white hot iron was burning and oxidizing. I made excellent soft malleable iron of it, but the bars had the peculiarity of not welding under any circumstances whatever, notwithstanding the iron was not in the slightest degree red-short.

I kept a pile of this iron, consisting of six single broad puddled bars in a balling or reheating furnace for six hours, during which time the furnace had been several times recharged with similar piles of other iron; yet this pile showed not the slightest inclination to weld: the pile looked as dry as possible in the flames, and seemed to become harder and drier every moment at a white heat, when all the other irons welded easily; and after being taken out of the furnace it was found to be covered with large blisters, exactly like those of blistered steel from the converting furnace. Its outside was silver-white, showing very little traces of oxidation. The inside of the broken bar was very similar in appearance to blistered steel, showing the cubical crystalline form and the large blisters in the inside, covered with the usual colours of blue and yellow. It forged very well, yet hardened but very little. By actual analysis, a considerable quantity of carbon was found in it, as well as arsenic, but *no trace of silicon.* In the retort, treated with hydrochloric acid as usual, the evolution of gas lasted upwards of three weeks: the evolved gas had *no smell*, which proves that carbon, at *least alone*, cannot be the cause of the bad smell of this description of hydrogen. The residuum of the solution was black, smoking very much during ignition, and leaving a small quantity of dirty red residuum, which was entirely soluble in hydrochloric acid, leaving only a few small black scales behind. The solution contained iron, but no traces of silica.

I must here mention a fact but little known, that all piles of iron which are to be welded in a reverberating-furnace, must rest on a bottom which contains a large quantity of free silica. When the pile of iron is heated in such a reverberating-furnace, the silicon and the iron on the surface of the pile become oxidized, forming a very tough half-melted slag, which does not at all prevent the access of air, and the iron would burn into cinders did not the silica of the bottom combine with the slag next to it, forming a liquid silicate and giving an equal quantity of silica to the uppermost bar of iron in the pile, until this liquid slag is spread over the whole pile and its interstices. Iron piles heated upon a slag bottom will not weld but burn, a circumstance which I found always overlooked.

When iron is heated on a bottom composed of siliceous matter, for a long time and at the highest degree of heat, the silicon of the bottom is reduced by the carbon of the iron as well as of that of the flames, and combining with the iron renders its texture loose, makes it finally melt, and produces that which is usually termed *burnt iron.*

The silicon is, in fact, the cause of the welding property of the iron. Thus silica is sometimes the cause of malleable iron melting in our common fires. The general idea, that malleable iron can be melted even in Sefstrœm's or Knight's blast-furnace, is quite erroneous. An accurate analysis of the iron before and after fusion will soon convince us of the truth of this assertion, and we find invariably that the iron during fusion had combined either with carbon, or with silicon, or with both. We have seen above, that iron, even though it contains a large quantity of carbon, sometimes developes a perfectly inodorous hydrogen, and an inodorous hydrogen is therefore no proof of chemically pure iron. The process of welding iron consists in heating the skeleton grains of iron, contained in the mass, in order to excite all their attractive forces, but at the same time to prevent their combining with any other body, especially carbon, in which case only the skeleton grains will attract each other and become united. The attractive forces of these grains for carbon are developed only when they are at a white heat, and it is an error, which has remained even in the last edition of Turner's Chemistry, that iron will weld at a red heat. When the grains of iron at a white heat really come into contact with carbon, or when a sufficient quantity of carbon has not been burnt away during the process of puddling, the skeleton grains, instead of adhering to each other, come into a state of fusion during the compressing force of the strokes of the hammer, assume a crystallized form, and produce a sort of cold-short iron.

Chemically-pure iron I could never make to weld. I procured chemically-pure iron by reducing oxide of iron (from which all traces of silica were carefully separated by means of repeated solutions and evaporations) in a current of hydrogen, and inclosed it hermetically in a platinum tube previously filled with dried hydrogen. Several of these tubes were exposed to different degrees of heat, from the dark red-heat to the white-heat, and hammered, in order to give consistency to the inclosed powder of iron; but it never showed any signs of continuity or welding afterwards, and under the microscope itself appeared unaltered.

We have just shown, that silicon principally imparts to the iron the property of welding; but we see likewise in the last specimen of iron examined, that even a considerable quantity of carbon contained in the iron does not impart to it the property of *hardening* after being heated and cooled in water. Besides this last specimen of iron, I melted pure iron with 3 per cent. of charcoal prepared from sugar, in a clay crucible, and, watching the heat very carefully until it had become quite liquid, I poured it into a common ingot mould for cast steel. This iron when broken presented a large round granulation of a bluish-white colour, resembling the specimen *c* before mentioned; under the hammer it forged extremely soft and tough, like Taberg iron, but would not harden at all when dipped red-hot into water, notwithstanding it contained 2·5 per cent. of carbon. But it contained scarcely any traces of silicon; and I found by keeping the liquid mixture for a longer time at a higher degree of heat, the silicon increased, and with it the property of hardening.

We have mentioned in a former paragraph, that iron when heated in a reverberating-furnace in contact with siliceous matter, imbibes a portion of silicon; but the question now is, why the specimen *e*, before-mentioned, did not imbibe silicon from the bottom of the furnace, but combined instead with the carbon of the decomposed flame? The answer is obvious. In the cast iron from which the specimen *e* was prepared, silicon, iron, and arsenic were combined. Arsenic in combination with silicon has the property of rendering the latter more easily oxidized, so that the greater part of the silicon is consumed before the arsenic, which occasioned the extraordinary hissing noise, already mentioned, during the process of ebullition of the specimen *e* in the puddling furnace. The malleable iron thus prepared had entirely lost its silicon, without which no peroxide of iron could be formed sufficiently liquid to resist the reducing power of the flames. The protoxide, wherever it was formed, consisted of a dry powder, which was speedily reduced by the action of the flame into its former metallic state, combining at the same time with carbon, and gradually changing the whole mass into a carburet. When, on the contrary, the protoxide contains sufficient silica, a very liquid silicate of iron is generated, which, not capable of being reduced by the flames, spreads itself over the entire surface, and likewise prevents the action of the flame upon the iron. The state in which the molecules of malleable iron as well as cast steel exist, seems never to have been taken into consideration, and both malleable iron as well as malleable steel were considered to differ from cast iron and cast steel only so far as regarded their chemical properties. But this is altogether an error. Malleable iron and malleable steel owe their properties to the mechanical force of the hammer; and as soon as they lose the peculiar arrangement of their molecules, produced by the hammer, these properties are entirely changed[*]. The preparation of malleable iron from cast iron shows this very distinctly. The iron is brought into a half-melted state, in which state the larger crystals of the iron, called grains, lose their attractive power in respect to position; but the smaller crystalline compounds of the molecules of iron never lose their form or structure, but retain them during the whole process of puddling, and the slag rising only keeps the small crystals of the iron separate, and by enveloping them prevents their acting directly one upon the other. The softened, but not liquid, grains of the iron begin now to abstract oxygen from the surrounding slag, which is immediately replaced by the *oxygen of the air*[†]; and thus generating with the carbon of the iron, carbonic oxide gas, as well as carbonic acid; and the evolution of these gases causes the well-known ebullition of the whole mass. In this separated state of the molecules, it is very difficult for heat to be brought to act on them; and it is, in fact, well known to all workmen, that several metals are the more difficult to re-melt the more minute their state of division.

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[*] That peculiar sort of steel from which in steel pen manufactories the extremely fine chisels are made to cut the slit into the pen, is entirely produced by long-continued and judiciously-applied hammering.

[†] By putting a cap of sheet iron, resembling the head of a still, into the boiling iron, the tube of which extending through the door of the furnace dips into water or quicksilver—we soon perceive that air is absorbed, and the water begins to rise in the tube. By blowing an uninterrupted current of air into this apparatus, the boiling is soon re-established;

Each grain of the iron, not being in a fluid state, by losing the carbon, silicon, &c., is converted into the base of malleable iron, leaving behind only a skeleton of the grains of cast iron. All the crystalline planes are destroyed; and therefore, instead of the crystals adhering to each other with their planes of crystallization by a force easily to be overcome, an innumerable quantity of points of the skeleton of the crystals adhere together in all directions of the adhesive force; and as we have before observed, that the skeleton grains are naturally not in a liquid state, the spaces formerly occupied by the carbon, silicon, and portions of iron, which have been consumed, are still vacant, consequently the iron in this state exerts a very great decomposing power upon all chemical bodies, as is the case with oxide of iron reduced at a low degree of heat by hydrogen. This fact likewise explains the circumstance, which I have often observed, that by using bad coals, or when the draught of the furnace is not well regulated, the iron which during the first half of the process of puddling had lost all its sulphur by chemical means, exhibited at the end of the process more sulphur than the cast iron actually contained before the puddling commenced.

The vacant spaces in the skeleton grains which constitute the lumps of iron made in the puddling furnaces, are immediately closed when brought under the forge hammer, and consequently the decomposing power ceases; but where the malleable iron thus obtained is again kept at a white heat for a considerable space of time, the closed pores in some measure re-open, and their attractive force towards chemical agents begins to re-appear, and thus, to mention one instance, the iron combines in the cementing furnace with carbon, forming *cemented steel*, without altering the juxta-position of the silicon and iron molecules in the cemented bar. Such a cemented bar, when exposed to a proper degree of heat, will weld to another similar bar, as the mechanical texture of the iron had not been altered. But it is different when the cemented steel, instead of being softened only, is reduced to a perfect liquid state by melting. In this case the iron and si-

and I used a similar apparatus, for ascertaining the nature of the escaping gases, after the boiling mass had been mixed with different chemical agents, licon combine more closely with the carbon which they imbibe during cementation. The carburet of silicon partially separates from the carburet of iron, and crystallizes during a slow process of evolving carbonic oxide gas, particularly when no more than one definite carburet of iron is contained in the bar of steel. By these means the peculiar appearance by which the Damascus steel is distinguished is produced *. As Wootz, or Indian steel, is only a cast iron, and has therefore not lost any portion of its silicon or aluminum, which a malleable iron bar always has to a greater or less extent lost, and which it never recovers during cementation, those crystals of silicon or aluminum are more developed in Indian steel, than in that made in the common way from malleable iron. As, therefore, iron during cementation is only capable of imbibing carbon, it is evident that the silicon and other electro-negative metals must be already contained in the iron in order to produce good steel, and for this reason certain descriptions of iron only are capable of producing good steel. All iron, and particularly English iron, has during the process of puddling lost a much greater part of its silicon than of its carbon, and its grains are intermixed with an extremely thin layer of a supersilicate of iron, which during cementation is only partially reduced, and leaves a different silicate between the grains of the bar, which makes the steel thus produced invariably red-short. Iron prepared from pure oxide of iron in so-called German fires, or even by a different process in a puddling furnace, is at large intervals only interwoven with almost pure protoxide and peroxide of iron, which during cementation is entirely reduced. The carbonic oxide produced occasions those large blisters from which

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* As a proof that a real separation takes place in melted steel between the carburets of silicon and iron, I refer to an experiment made in Mr. Wilson's steel manufactory at Sheffield, when I melted English iron with lamp black and charcoal from sugar. The molten mixture was poured into an ingot mould used for common cast steel, one side of which was hotter than the other. After cooling the metallic parallelepipedon, one half of its cross-fracture was observed to be crystallized, and the other granulated. When drawn out under the tilting hammer, a bar was obtained; one half of its short axis, corresponding to the crystallized side of the ingot, was perfect steel, and the other half the softest iron, just as if a steel bar had been welded to a soft iron bar. The soft part of the bar contained a great quantity of carbon, but merely traces of silicon, too small to be weighed.

In another similar experiment, after the liquid steel had been poured into the ingot mould, a lump of metal the size of a hen's egg remained at the bottom of the crucible. On perceiving this, I put the steel back into the crucible, and re-melted it at the strongest heat I could obtain; and on again pouring it out, I found the lump, still remaining at the bottom of the crucible quite unaltered; 35 grains of this lump, dissolved in hydrochloric acid, left only 0·198 grains of a grayish residuum.

cemented steel has obtained the name of blistered steel, and the oxygen of the part of this gas which is in contact with the sides of the blister occasions those fine tints of yellow and blue with which the inside of the blisters is generally covered.

It is generally asserted that the outside of the cemented bar of steel contains more carbon than the inside, and that therefore the fusion of the cemented bar served to spread the carbon more equally throughout the mass. This is contrary to fact. When the iron bar which is to be cemented is of a suitable thickness, the process of carbonization commences, like the process of the reduction of iron ores, almost simultaneously at the centre and at the outside of the bar; and I have often found that the inside of the cemented bar contained a greater quantity of carbon than the outside. The quantity of carbon imbibed by each portion of the bar depends entirely on the quantity of carbon first contained in the different parts of the iron bar, as well as on the carburet of silicon contained in it; and this different degree of carbonization may even be detected by the eye, from the various forms of crystallization contained in the bar.

This, as well as other chemical phænomena which have not received sufficient attention, may be explained by the law which causes one atom of water in the voltaic circuit to evolve hydrogen and oxygen at the same moment at two distant points.

In conclusion, I annex an analysis of the best English cast steel, to controvert the modern assertion that the best cast-steel bars consist only of a pure compound of carbon and iron. The analysed specimen was a fragment of an excellent razor, forged in my presence, in the workshop of Mr. Rodgers, of Sheffield, of the specific gravity of 7·92.

Silicon	0·52043
Aluminum	0·00000
Manganese	1·92000
Arsenic	0·93400
Antimony	0·12100
Tin	Traces.
Phosphorus	0·00000
Sulphur	1·00200
Azote	0·18310
Carbon	1·42800
Iron	93·79765
Loss	0·09382
	100·00000

35 grains of this steel dissolved in hydro-chloric acid of sp. gr. = 1·104, deposited on the vault of the retort, soon after the acid began to act upon the iron, a dark ring of carbonaceous matter twice the diameter of the space occupied by the steel filings at the bottom of the retort. The inside of this ring was gradually filled up by the black residuum, whilst at the same time it collected in rays around the filings at the bottom of the retort, which filings in the same ratio disappeared, till the space formerly occupied by them was filled up entirely by the black powder, gradually increasing towards the centre, each grain of this black powder retaining the form of the particle of steel filing from which it had its origin. The action of the acid and the evolution of gas had not ceased in three weeks. The residuum, of a dark brown greenish colour, was equal to 1·995 grains. Heated in a platinum crucible, a single bright spark appeared towards the centre, which immediately disappeared. Long after this the mass ignited within the crucible, and had increased in weight = 0·959. After being treated by hydrochloric acid, 0·381 of silica remained, contaminated with a little iron. The greatest quantity of arsenic was found to be contained in the acid from the retort.

Arsenic, sulphur, silicon, and azote are constituent parts of all the best English steel which I have analysed, made from the Dannemora iron, called Hoop L. and double Bullet. Their relative proportions in all specimens are nearly the same. In some specimens of very hard steel I found the carbon increased to 1·69 per cent. It is scarcely necessary to add, that the arsenic, antimony, and tin were precipitated together by a current of sulphuretted hydrogen, and sometimes by means of hydro-sulphuret of ammonia. The precipitate was divided into two equal parts; one part dissolved in aqua regia, the diluted solution mixed with tartaric acid and the quantity of sulphuric acid ascertained in the usual way. The other portion of the precipitate obtained by sulphuretted hydrogen was heated carefully in a glass capsule in a current of dry hydrogen, till the arsenic was driven off. The antimony and tin were of course left behind. I endeavoured to separate both these, as proposed by Gay Lussac, by dissolving the residuum in aqua regia, and precipitating one half of the solution by means of metallic zinc; the other by metallic tin, which only separates the antimony. But to gain an exact result, a larger quantity of the material for analysis is necessary than can be conveniently obtained by analysing iron.

If a current of sulphuretted hydrogen is driven through an acid solution of peroxide of iron, with other metals and silica, the silica invariably falls down with the sulphurets, perhaps in the state of a sulphuret, and remaining, after the treatment of the sulphurets with aqua regia, in such a state as to be insoluble in all acids except hydrofluoric acid.

We cannot enough recommend the utmost care in examining the precipitate obtained by sulphuretted hydrogen from solutions of iron, and *all* the contents of this precipitate ought to be always separated and tried to be procured in their isolated state.

By trying to separate phosphoric acid from iron, by means of alkalies, the phosphoric acid can only be separated entirely from the iron when the mixture shall be kept in a perfect white heat for some time.

Since writing this article several months ago, some remarks have occurred to me, which may serve to elucidate it.

According to the experiments in the previous paper, we consider, that the toughness of the black and gray sorts of cast iron is owing to the siliciuret of iron, while their qualities of strength and fusibility are attributable to the carburet of aluminum, silicon and iron. I must here observe, that really gray cast iron, used for foundry purposes, never changes its appearance from gray into white without changing its chemical composition; while the white crystallized cast iron, produced on the continent from spathose iron ores, at a comparatively low degree of heat, changes its appearance from white into apparently gray cast iron, according to the degree of slowness with which it is cooled. But this ready conversion of white iron into gray iron is only apparent, and the abovementioned crystallized iron bears, in either of its states, the chemical character of white iron, in which a part of the silicon is replaced by manganese. When apparently converted into gray iron by slow cooling, it has only changed the state of aggregation of its component molecules, and consequently its density; or, in other words, the molecules of this sort of iron have had time to arrange themselves during its cooling into a more developed crystalline form. This crystalline form may be easily distinguished from the regular foliated form of crystallization of real gray cast iron, when viewed under the microscope, by the irregularity, smallness, and thickness of its component leaves or scales, and a stroke of the hammer will invariably restore to the part struck the original white silvery colour peculiar to it. The residuum of both varieties of this iron, after its treatment with hydrochloric acid, have all the characters of the residuum of white iron; they are brown, instead of white or gray, become ignited at a very low degree of heat, and never effervesce with caustic ammonia.

The crystalline form of cast iron generally depends on the relative atomic combination of carbon with silicon. The hardness and whiteness of the compound decreases with the increase of carbon, and has reached its utmost degree of friability in that sort of supercarburet of silicon, which is called *graphite*, or by the iron-smelters, *kish*.

I procured a beautiful specimen of this sort of graphite or kish a few weeks ago from one of the blast-furnaces at Merthyr-Tydvil. A rather porous piece of slag, of a yellowish green colour, like impure sulphur, was found to be interwoven with a graphitic formation, consisting of large irregular layers of different sizes, and of a dusty grayish graphitic hue. These large layers or laminæ were found to be composed of smaller rhombic scales, lying one over another, similar to the tiles of a roof, and giving to the surface the appearance of a regular rhombic network *.

The composition of the large laminæ was found to be different in different parts of their thickness. The scales of graphite on the outside were soft, light, and so easily divisible as to soil the fingers. They increase in thickness and become more dark-coloured towards the middle, and the central layer had the appearance and hardness of black cast iron, and its somewhat conchoidal fracture had a lustre betwixt those of glass and pitch. The exterior and thinnest scales were not attracted by the magnet at all; but the interior ones were affected by the magnet almost in the ratio of their increasing thickness.

In hydrochloric acid, the central layer evolved hydrogen rapidly, first a white, and afterwards a yellowish scum of silica were separated, and it showed in fact all the properties of the blackest cast iron.

The scales adjoining were strongly attracted by the magnet, and appeared under the microscope to be covered with small flattened crystals, forming an irregular six-sided prism, of which only four sides were developed; in a similar manner only two opposite sides of the rhombic faces of each end of the crystal were left, corresponding to the smaller sides of the prism.

Those small crystals seemed to constitute a central point, from the sides of which the small leaves of graphite forming the surface of the laminæ appeared to radiate.

I succeeded in separating one of those largest crystals, and in covering it under the microscope with a drop of concentrated hydrochloric acid. The acid did not attack the crystal until heat was applied, and then quickly formed around the crystal a framework of white tough silica, apparently consisting of leaves or parallel threads, corresponding to the sides of the nucleus, which after having been separated from the silica with a fine needle, was finally converted entirely into a spot of silica.

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* [In Phil. Mag. First Series, vol. xl. p. 41, will be found an examination, by Mr. (now Prof.) E. Davy, of a native graphite considered by him strongly to resemble *kish*.—Edit.]

By repeatedly treating one of the large graphite layers or laminæ with boiling hot hydrochloric acid and alkalies, it increased in blackness and brilliancy; the single leaves appeared thinner, their mutual connection was loosened, and the magnet had no further action on them.

With the exception of hydrofluoric acid no single chemical liquid seemed to have any action on those scales, and only the most concentrated hydrofluoric acid slowly attacked them, when in a state of most minute division.

After several fruitless efforts, I finally discovered a new method of decomposing them by means of acids, which gave rise to new and interesting phænomena.

I poured about three fluid-drachms of concentrated sulphuric acid over two grains of these purified graphite scales in a deep platinum crucible, and made the acid boil briskly over a spirit lamp. After this, I removed the crucible from the fire, till the dense fumes which arose began somewhat to cease. I then drew up about one fluid-drachm of strong fuming nitric acid into a long small glass tube, and dropped one half of the acid rather slowly, the other half quickly, into the hot sulphuric acid, which caused the latter to boil again, during a rapid evolution of binoxide of nitrogen. As soon as the boiling began to cease, I placed the crucible again over the lamp, and boiled the liquid till all the nitric acid was decomposed. I found the scales of graphite so much swollen as to fill up the whole lower part of the crucible, so that the liquid was no longer visible. On nearer inspection, I perceived that every single leaf of those scales was converted into a spongy body, of the lustre of coke, and of the same breadth and thickness, about the size of a pea.

Washed with distilled water, and dried at 212° Fahr., those spongy masses weighed 2·18 grains, and lost after ignition 0·39 grains. No degree of heat to be obtained by a large spirit lamp caused any further alteration. Their appearance in this state nearly resembled in lustre and texture pieces of hard coke and foliated charcoal; they were composed of four to five easily separable layers, which were again intersected by several cracks or fissures vertical to them, similar somewhat to the structure of charred wood, and their edges only appeared of the peculiar metallic lustre of coke. Even the smallest pieces being placed on a platinum foil resisted for a long time all the effects of the flame, but at length began to ignite and to be consumed quickly, always leaving a grayish or brownish residuum, which consisted of silica with a little iron.

As I treated those spongy ignited masses again with sulphuric and nitric acid, in the manner above mentioned, I found their bulk considerably diminished; and after a repetition of the same operation for the fourth time, the last trace of graphite had disappeared, and the acid remained perfectly clear. Diluted and saturated with caustic ammonia, a white, light flocculent precipitate fell, and the whole liquid, evaporated to dryness, and ignited, left a brownish residuum consisting of silica with a little alumina and iron.

The graphite obviously had here been converted into carbonic acid by means of nitric acid; but it is a very curious fact, that this conversion takes place only under the abovementioned circumstances.

Concentrated nitric acid, dropped on the red-hot graphite, has not the slightest action on it; neither has sulphuric acid dropped into boiling nitric acid. To obtain the expected results, the above precept must be followed strictly, and the crucible must be spacious, as with every drop of nitric acid falling into the sulphuric acid a slight explosion takes place, which might occasion the loss of some of the liquid.

A somewhat probable explanation of the singular action of both acids combined in this manner, seems to be, that the boiling sulphuric acid absorbs the water from the nitric acid, the oxygen of which is only able to combine with the carbon of the supercarburet of silicon at the moment when the sulphuric acid combines with the water of the nitric acid. If the residuum of gray cast iron dissolved in hydrochloric acid be treated in the same way, all graphite scales disappear, and only white silica remains.

LXXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 5, 1839. NO paper read.

Dec. 12.—"On the Nerves of the Gravid Uterus." By Robert Lee, M.D., F.R.S.

The author, while dissecting a gravid uterus of seven months, on the 8th of April, 1838, observed the trunk of a large nerve proceeding upwards from the cervix to the body of that organ along with the right uterine vein, and sending off branches to the posterior surface of the uterus; some of which accompanied the vein, and others appeared to be inserted into the peritoneum. A broad band, resembling a plexus of nerves, was seen extending across the posterior surface of the uterus, and covering the nerve about midway from the fundus to the cervix. On the left side, a large plexus of nerves was seen, surrounding the uterine veins at the place where they were about to enter the hypogastric vein. From this plexus three large trunks of nerves were seen accompanying the uterine vein, which increased in size as they ascended to the fundus uteri. From the nerve situated on the posterior surface of the vein, numerous filaments passed off towards the mesial line, as on the right side; some following the smaller veins on the posterior surface of the uterus, and others becoming intimately adherent to the peritoneum. The largest of the nerves which accompanied the uterine vein was traced as high as the part where the Fallopian tube enters the uterus; and there it divided into numerous filaments, which plunged deep into the muscular coat of the uterus along with the vein. A large fasciculated band, like a plexus of nerves, was also seen on the left side under the peritoneum, crossing the body of the uterus; and several branches, apparently nervous, proceeding from this band, were distinctly continuous with some of the smaller branches of nerves accompanying the uterine veins. The preparation of the parts was placed in the Museum of St. George's Hospital, on the 1st of October, 1838; and several anatomists who examined it were of opinion that they were absorbents accompanying the uterine veins, and tendinous fibres spread across the posterior surface.

Dr. Lee availed himself of another opportunity which presented itself, on the 18th of December of the same year, of examining a gravid uterus in the sixth month of pregnancy, which had the spermatic, hypogastric and sacral nerves remaining connected with it; and during the last ten months, he has been diligently occupied in tracing the nerves of this uterus. He believes that he has ascertained that the principal trunks of the hypogastric nerves accompany, not the arteries of the uterus, as all anatomists have represented, but the veins; that these nerves become greatly enlarged during pregnancy; and that their branches are actually incorporated, or coalesce with the branches of the four great fasciculated bands on the anterior and posterior surface of the uterus, bearing a striking resemblance to ganglionic plexuses of nerves, and sending numerous branches to the muscular coat of the uterus.

The author gives the following description of the nerves of the gravid uterus in the sixth month, and of these fasciculated bands as displayed in the dissection.

Behind the uterus, the aortic plexus divides into two portions, to form the right and left hypogastric plexuses. These plexuses, after an intimate union with the nerves accompanying the ureters, descend to the neck of the uterus, upper part of the vagina, and contiguous parts of the bladder and rectum, where they are joined by branches from the third and fourth sacral nerves. The left hypogastric plexus, about two inches below the aortic plexus, sends off a large branch, which passes on the inside of the ureter to the superior uterine vein, where it is about to terminate in the hypogastric vein. Here the nerve suddenly expands, becomes broad and thin, and passes into a great plexus of nerves, which completely encircles the vein. This plexus, surrounding the uterine vein, is joined below by two large branches, which proceed from the hypogastric plexus nearer the vagina, and lower down, and from which branches pass on the outside of the ureter. From the upper part of this plexus, surrounding the uterine vein near its termination, three large trunks of nerves proceed upwards with the vein to the superior part of the uterus, and enlarge as they ascend. The posterior branch of these hypogastric nerves sends off in its course smaller branches, which accompany the ramifications of the uterine vein on the posterior surface of the uterus. Passing upwards beyond the junction of the spermatic with the uterine vein, and running between the peritoneum and the left posterior fasciculated band, it spreads out into a web of thin broad branches and slender nervous filaments, some of which are inserted into the peritoneum, and others follow the vein to the fundus uteri, which they completely surround as the vein passes down into the muscular coat of the uterus.

Some of the branches of this nerve, near the fundus uteri, are distributed to the muscular coat, but these are small and few in number.

The middle and anterior branches of the hypogastric nerves adhere closely to the uterine vein as they ascend, and form around it several plexuses, which completely invest the vessel. From these plexuses branches are sent off to the anterior surface of the uterus, some of which, in an arborescent form, follow the trunk and branches of the uterine artery. These two hypogastric nerves ascend, and closely unite with the left posterior fasciculated band.

On the left side of the uterus this band arises near the mesial line, on the back of the uterus, midway between the fundus and cervix, from a mass of fibres, which adhere so firmly both to the peritoneum and muscular coat that it is difficult precisely to determine their arrangement. From these fibres the band proceeds across the uterus, in the form of a thin web, to the point where the spermatic vein is leaving the uterus. After closely uniting with the hypogastric nerves, this band proceeds outwards to the round ligament, becoming less firmly adherent to the peritoneum, where it unites with the left anterior band, and spreads out into a great web, under the peritoneum. The left posterior band is loosely attached, through its whole course, to the subjacent muscular coat by soft cellular membrane.

The spermatic nerves on the left side pass down to the ovarium with the spermatic artery, and first give off several branches to the corpus fimbriatum. A few small branches are then sent into the outer end of the ovary. The spermatic nerves afterwards leave the artery, and proceed with the veins to the uterus, where they firmly unite to the outer extremity of the left posterior band; and after the junction of this band with the prolongations of the anterior band under the round ligament, numerous small, delicate filaments, apparently nervous, are sent to the base of the ovarium.

On the right side of the uterus, the author finds that the distribution of the hypogastric and spermatic nerves does not essentially differ from that now described as seen on the left side. The form and situation of the right posterior band is, he states, much more clearly seen than on the left side, and presents the appearance of a white pearly fasciculated membrane about a quarter of an inch in breadth, proceeding from the mesial line at right angles to the hypogastric nerves, across the body of the uterus, to the round ligament, where it unites with the anterior band. Numerous branches, strikingly resembling the branches of nerves, are sent off from the upper and lower edges of this band, and from its posterior surface to the muscular coat of the uterus. An extensive and intimate union at various points is distinctly perceptible between these branches sent off from the band and the branches of the hypogastric nerves. On the anterior and upper part of the neck of the uterus, there is a great mass of reddish-coloured fibres, firmly interlaced together, resembling a ganglion of nerves, into which numerous large branches of the hypogastric nerves on both sides enter, and to which they firmly adhere. From the upper part of this fibrous substance there passes up, over the whole anterior surface of the uterus, a thin band of firm white fasciculated fibres, prolongations of which extend to the round ligaments,—into which, and into the posterior band, they are continued by numerous filaments, like those of nerves. From the posterior surface of this great band, numerous branches, also apparently nervous, can be traced to a considerable depth through the muscular coat of the uterus.

The author concludes his paper with the following remark, and a short historical account of the progress of discovery on the subject of the nerves of the uterus:—

"From the form, colour and general appearance of these fasciculated bands, and the resemblance they bear to ganglionic plexuses of nerves, and from their branches actually coalescing with the hypogastric and spermatic nerves, I was induced to conclude, on first discovering them, that they were nervous plexuses, and constituted the special nervous system of the uterus. The recent examination, however, of the gravid uterus of some of the lower animals, in which I have found a structure similar to those bands in large quantity under the peritoneum, has left me in considerable doubt as to the nature of these bands, and until further investigations have been made, I shall not venture to pronounce a positive opinion respecting them."

The description of the nerves of the uterus contained in Professor Tiedemann's splendid work, the author adds, is usually referred to by anatomical writers as the most accurate and complete which has ever been given. Professor Tiedemann has represented the spermatic nerves as being distributed chiefly to the ovarium; and the hypogastric as invariably accompanying the trunk and branches of the uterine arteries, along the sides of the uterus,—dividing into smaller branches, and quickly disappearing in the muscular coat of the uterus. He has made no mention of the large nervous trunks on both sides of the uterus, which accompany the uterine veins; nor has he noticed fasciculated transverse bands on the anterior and posterior surfaces of the uterus, connected with the hypogastric and spermatic nerves.

"Observations made at the Cape of Good Hope, in the year 1838, with Bradley's Zenith Sector, for the verification of the amplitude of the Abbé de la Caille's Arc of the Meridian; by order of the Lords Commissioners of the Admiralty." By Thomas Maclear, Esq., M.A., F.R.S., &c. Communicated by Sir John Barrow, Bart., V.P.R.S., &c.

The author gives an account of the precautions taken in putting together the different parts of the zenith sector, which he received on the 9th of December, 1837, in erecting it in the central room of the Royal Observatory at the Cape of Good Hope, and in afterwards transferring it to the southern station of La Caille, in Cape Town. He then proceeds to describe La Caille's observatory, and the particular circumstances of its locality, with relation to the object in view, namely to determine the influence of Table Mountain on the direction of the plumb-line*. He next relates his progress to Klyp Fonteyn, where he arrived on the 24th of March, 1838, and describes the operations resorted to for erecting the sector at that place. He then enters into the details of observations made at different stations, and especially with comparative observations at the summit and foot of the mountain of Pequet Berg. The instrument was lastly conveyed back to Cape Town, and again examined, and the observations made with it repeated. The reduction of the observations occupies the remainder of the paper; and in conclusion, the author remarks, that although these labours have not altogether cleared up the anomaly of La Caille's arc, yet they show that great credit is due to that distinguished astronomer, who with imperfect means, and at the period in which he lived, arrived at a result, derived from sixteen stars, almost identical with that from 1139 observations on forty stars, made with a celebrated and powerful instrument.

Dec. 19, 1839.—A paper was read, entitled, "An account of experiments made with the view of ascertaining the possibility of obtaining a spark before the circuit of the Voltaic Battery is completed." By J. P. Gassiot, Esq.

The author of this paper adverts to the fact, of a spark invariably appearing when the circuit of the Voltaic Battery is completed; an effect which Dr. Faraday has shown can be easily produced, even with a single series. He then refers to the experiments of Mr. Children, Sir Humphry Davy, and Professor Daniell, recorded in the Philosophical Transactions; in which experiments, when more powerful and extended series were used, the spark was obtained before contact took place.

In order to ascertain, not only the fact of a spark being obtained, but also the distance through which it may be passed, the author had an instrument prepared, which he denominates a *Micrometer*

[* See L. and E. Phil. Mag., vol. xiv., p. 522.—Edit.]

Electrometer, and by which an appreciable space of one five-thousandth of an inch could be measured with great accuracy. He describes this instrument; and relates several experiments which he made with a view to test the correctness of its action. He first prepared 160, and then 320 series of the constant battery, in half-pint porcelain cells, excited with solutions of sulphate of copper and muriate of soda; but although the effects, after the contact had been completed, were exceedingly brilliant, not the slightest spark could be obtained. He was equally unsuccessful with a water battery of 150 series, each series being placed in a quart glass vessel; and also with a water battery belonging to Professor Daniell, consisting of 1020 series; but when a Leyden battery of nine jars was introduced into the circuit of the latter, sparks passed to the extent, in one instance, of six five-thousandths of an inch.

The author mentions his having been present at the experiment of Professor Daniell, on the 16th of February, 1839, when that gentleman had 70 series of his large constant battery in action; and having been witness of the powerful effects obtained by this apparatus, he was induced to prepare 100 series of precisely the same dimensions, and similarly excited: but although this powerful apparatus was used under every advantage, and the other effects produced were in every respect in accordance with the extent of the elements employed, still no spark could be obtained until the circuit was completed; *even a single fold* of a silk handkerchief, or a piece of dry tissue paper, was sufficient to insulate the power of a battery, which, after the circuit had been once completed, fused titanium, and heated 16 feet 4 inches of No. 20 platinum wire.

The author then describes a series of experiments made with induced currents. Twelve hundred and twenty iron wires, each insulated by resin, were bent into the form of a horse-shoe. A primary wire of 115 feet and a secondary of 2268 feet, were wound round the iron wires. With this arrangement he obtained a direct spark (through the secondary current), sufficient to pierce paper, to charge a Leyden jar, &c. Several forms of apparatus employed by the author are next described, and also a series of 10,000 of Zamboni's piles. With this arrangement he charged a Leyden battery to a considerable degree of intensity, and obtained direct sparks of three-fiftieths of an inch in length. He ultimately succeeded in obtaining chemical decompositions of a solution of iodide of potassium, the iodine appearing at the end composed of the black oxide of manganese.

The Society then adjourned over the Christmas Vacation, to meet again on the 9th of January, 1840.

Jan. 9, 1840.—A paper was read, entitled, "On the construction and use of Single Achromatic Eye-Pieces, and their superiority to the double eye-piece of Huyghens." By the Rev. J. B. Reade, M.A., F.R.S.

The author observes, that experience has shown it to be impracticable to make a telescope even approach to achromatism, by employing the same object-glass with an astronomical, as with a terrestrial eye-piece: for if the focus of the blue rays from the object-glass be thrown forwards, as it must be in order to make it impinge upon the focus of the blue rays of the terrestrial eye-glass, then there will be produced a great *over-correction* for the astronomical eye-glass; and *vice versa*. Hence it appears that the application of Huyghenian eye-pieces to refracting telescopes, is incompatible with the conditions of achromatism, throughout the entire range of magnifying power; and that in reflecting telescopes they unavoidably introduce dispersion, because they are not in themselves achromatic. These defects the author proposes wholly to obviate, by substituting for the Huyghenian eye-pieces, single achromatic lenses of corresponding magnifying power; consisting of the well-known combination of the crown-lens, and its correcting flint-lens, having their adjacent surfaces cemented together; thus avoiding internal reflections, and enabling them to act as a single lens. The achromatic eye-pieces which he uses were made by Messrs. Tulley and Ross, and are of the description usually termed *single cemented triples*.

A paper was also read, entitled, "Meteorological Observations made between October, 1837, and April, 1839, at Alten in Finmarken." By Mr. S. H. Thomas, chief mining agent at the Alten Copper Works. Presented to the Royal Society by John R. Crowe, Esq., Her Britannic Majesty's Consul at Finmarken. Communicated by Major Edward Sabine, R.A., V.P.R.S.

This memoir consists of tables of daily observations of the barometer and thermometer, taken at 9 A.M., 2 P.M., and 9 P.M., with remarks on the state of the weather, at Kaafjord, in latitude 69° 58′ 3″ north, and longitude 23° 43′ 10″ east of Paris.

Jan. 16.—A paper was read, entitled, "On Nobili's Plate of Colours; in a Letter from J. P. Gassiot, Esq., addressed to J. W. Lubbock, Esq., V.P. and Treasurer R.S." Communicated by J. W. Lubbock, Esq.

The effect produced by the late Signor Nobili, of inducing colours on a steel plate, excited the curiosity of the author, and led him to the invention of the following method of producing similar effects. Two of Professor Daniell's large constant cells were excited with the usual solutions of sulphate of copper and sulphuric acid. A highly-polished steel plate was placed in a porcelain soup-plate, and a filtered solution of acetate of lead poured upon it. A piece of card-board, out of which the required figures had been previously cut with a sharp knife, was then placed upon the steel-plate. Over the card, and resting on it, there was fixed a ring of wood, a quarter of an inch thick, and the inner circumference of which was of the same size as the figure. A convex copper-plate was made so that its outer edge might rest on the inner part of the wooden ring; and its centre placed near, but not in actual contact with the card-board. Connexion was then made by the positive electrode of the battery with the steel-plate; the negative being placed in the centre of the copper convex plate. The figure was generally obtained in from 15 to 35 seconds. If a concave, instead of a convex plate be used, the same colours are obtained as in the former experiment, but in an inverse order.

"Geographical position of the principal points of the Triangulations of the Californias and of the Mexican coasts of the Pacific, with the heights of the principal points of that part of the Cordilleras." By the Comte Vincent Piccolomini; in a letter addressed to Sir John F. W. Herschel, Bart., V.P.R.S. Communicated by Sir John Herschel.

Hauteurs des principaux points des Cordilleres, des côtes de l'Océan Pacifique du Mexique, et de la haute et basse Californie.

	Elévation en pieds français sur le niveau de la mer pacifique.		Elévation en pieds français sur le niveau de la mer pacifique.
Volcano di Orizaba	18728	Tasco	5971
Volcano di Popocatepetl..	17812	Temascaltepec	5760
Volcano di Tztlacihuatl...	15698	Guernavaia..................	5447
Rio frio	10948	Tchuacan	5398
Real del Monte	10570	Xantetelco	5030
El oro........................	8995	Cuicatlan	5028
Tlalpuhahua	8435	Oajaca	5024
Ameia........................	8247	Cuautla	4587
Naupalucan	8194	Talapa	4542
Las Vigas	7918	Acayucan	4485
Perote........................	7911	Coscomatepec	4451
Ozumba (Etat de Puebla)	7874	Huatusco	4424
Tepeyahualco..............	7702	Talostoc	4421
Ozumba (Etat de Mexico)	7620	Lautepec.....................	4019
S. Rosa	7565	Orizaba (ville)	3998
Lagunas de Chalco	7510	Real de Christo	3851
Mexico	7450	Huaitla	3336
Tepeaca	7444	Cordova	2769
Huehuetaca	7121	Dominguillo	2274
Puebla	7078	Villalé........................	1578
Tula	6613	Petapa	617
Tlacotepec	6479	Tehuantepec (Océan Pacifique.)..................	132
Zacualpan	6181		

Position géographique des principaux points de la Triangulation des Californics et des côtes de l'Océan Pacifique du Mexique par le Comte Vincent Piccolomini.

	Latitude.			Longitude calculée de Greenwich.		
	°	′	″	°	′	″
Volcanos de las Virgenes..............................	29	91	14·35	121	46	30·82
Cap S. Lucas (Basse Californie)......................	23	08	35·27	109	82	15·04
Monterrey (Haute Californie).........................	36	58	17·85	121	46	53·09
Guaymas (Département de Sonora)	27	55	00·48	111	45	37·42
Matamoros (Texas)..	25	59	22·07	97	54	18·76
Id. Id. Barra grande de S. Yayo	26	30	27·15	...	...	......
Id. Id. Barra del Rio	25	53	03·11	...	...	......
Bejar (Texas)	29	74	88·93	98	85	17·74
Mine d'or de S. Yago de los Cabálleros	25	13	77·04	106	67	15·87
Vulcans de Tuxtla	18	47	25·91	94	07	43·11
Namampateptl (Province de Vera Cruz)	19	21	48·71			
Bahia de San Francesco (au cap los Reyex dans la Haute Californie)	37	59	17·29	122	37	13·04
Port de San Blas, tour de l'église (Guadalajara) .	21	67	05·54	105	43	17·28
Volcanos de Colima......................................	19	03	45·17	103	21	47·04

N.B. L'instrument employé pour déterminer les Longitudes était un Chronomètre de O. H. Bestor; pour la mesure des triangles de prémier ordre je me servis d'un théodolite de dix pouces de diamètre sortant des ateliers de Munich, pourvu de quatre verniers et donnant 10″. Les élévations du sol furent déterminées par des observations barométriques faites avec soin, souvent répetées et déduites par le moyen d'observations correspondants; elles furent calculées d'après la méthode d'Oltmanns et vérifieés par celles du Baron Zach. —V.P.

"Report on the co-operation of the Russian and German observers, in a system of simultaneous Magnetical Observations." By the Rev. H. Lloyd, F.R.S., in a letter addressed to Sir John F. W. Herschel, Bart., V.P.R.S. Communicated by Sir John Herschel.

"On Magnetical Observations in Germany, Norway, and Russia." By Major Sabine, R.A., V.P.R.S., in a letter to Baron von Humboldt, For. Mem. R.S., dated Oct. 24th, 1839.

These letters relate to communications which Professor Lloyd and Major Sabine have had, conformably to a resolution of the Council of the Royal Society, with the scientific authorities at Göttingen, Berlin, and St. Petersburg, respecting the organization of a simultaneous system of magnetical observations. It appears, from these letters, that the system proposed by the Royal Society is viewed with general interest and approbation; and nineteen stations are enumerated at which there is reason to expect that magnetical observatories, acting in concert, on that system, will be established.

Jan. 23.—A paper was read, entitled "On the structure of Normal and Adventitious Bone." By Alfred Smee, Esq., communicated by P. M. Roget, M.D. Sec. R.S.

On examining, by means of a microscope, very thin sections of bone, prepared in a peculiar manner, the author observed a number of small, irregularly-shaped, oblong corpuscles, arranged in circular layers round the canals of Havers, and also rows of similar bodies distributed around both the external and the internal margins of the bone. Each corpuscle is connected by numerous filaments, passing in all directions, with the Haversian canals and the margins of the bone, and also with the adjacent corpuscles. He finds that the canals of Havers are vascular tubes containing blood. The corpuscles themselves are hollow, and their cavities occasionally communicate with those of the canals; their length is equal to about two or three diameters of the globules of the blood. They exist in cartilaginous as well as osseous structures, and are found in every instance of adventitious bone, such as callus after fracture, morbid ossific growths either from bone or from other tissues; and the author has also ascertained their presence in the bony and cartilaginous structures of inferior animals, such as birds and fishes. Measurements relating to these corpuscles, by Mr. Bowerbank, are subjoined, from which it appears that their diameters vary from about the 10,000th to the 4000th, and their lengths from the 2300th to the 1400th part of an inch.

"An attempt to establish a new and general Notation, applicable to the doctrine of Life Contingencies." By Peter Hardy, Esq., F.R.S.

After premising a short account of the labours of preceding writers, with reference to a system of notation in the mathematical consideration of life contingencies, the author enters at length into an exposition of the system of symbols which he has himself devised, together with the applications which they admit of in a variety of cases.

Jan. 30.—A paper was read, entitled "Observations on Single Vision with two Eyes." By T. Wharton Jones, Esq. Communicated by Richard Owen, Esq., F.R.S.

The author animadverts on the doctrine which Mr. Wheatstone, in his paper on the Physiology of Binocular vision, published in the Philosophical Transactions for 1838, p. 371*, has advanced, in opposition to the received theory of single vision being dependent on the images of objects falling on corresponding points of the two retinæ. He maintains that, under these circumstances, the two impressions are not perceived by the mind at the same instant of time, but sometimes the one and sometimes the other. If one impression be much stronger than the other, the former predominates over, or even excludes the other; but still the appearance resulting from the predominating image is nevertheless in some manner influenced by that which is not perceived. He supposes that there are compartments of the two retinæ, having certain limits, of which any one point or papilla of the one corresponds with any one point of the other, so that impressions on them are not perceived separately; and considers that this hypothesis, combined with the principle above stated, is required, in order to explain the phenomena in question.

Feb. 6.—A paper was read, entitled "Observations on the Blood-corpuscles of certain species of the Genus Cervus." By George Gulliver, Esq., F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards†.

The author has found that the blood of the Muntjac‡, the Porcine§, and the Mexican Deer‖, contains, together with corpuscles of the ordinary circular form, a still larger number of particles of less regular shape; some curved and gibbous in the middle, and acutely pointed at the ends, with a concave and convex margin, like a crescent; others approaching more nearly to segments of a circle; some shaped like a comma, being obtuse at one end and terminated by a pointed curve at the other; others having an acute projection of the convex part, so as to constitute a triangular, or even quadrangular outline; some having the figure of the head of a lance; while a few presented a double or sigmoid flexure, as if they had been twisted half round at the middle. Like the ordinary blood-discs, these peculiar corpuscles are deprived of their colouring matter by water; but with only a small quantity of water they quickly swell out, and assume an oval or circular figure, forming long bead-like strings by the approximation of their edges. In saline solutions they become rather smaller, but preserve their figure tolerably well.

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* [Noticed in L. and E. Phil. Mag., vol. xiii., p. 461.]

† [Papers by Mr. Gulliver, on the blood-corpuscles of various animals, will be found in the present volume, p. 23, 105, and 195.—Edit.]

‡ *Cervus Reevesii.* § *C. Porcinus.* ‖ *C. Mexicanus.*

In an appendix, the author gives an account of his observations of the blood-corpuscles of a new species of Deer inhabiting the mountains of Persia, of which a specimen has been lately received by the Zoological Society. Many of these corpuscles presented the singular forms above described.

A paper was also read, entitled "Meteorological Register kept at Port Arthur, Van Diemen's Land, during the year 1838." By Deputy-Assistant Commissary-General Lempriere, in south latitude 43° 9′ 6″, and east longitude 147° 51′ 33″. Communicated by Captain Beaufort, R.N., F.R.S.

The height of the instrument above the level of the sea till the 21st of August was 57 feet, 7 inches; and during the remainder of the year 3 feet.

A paper was also in part read, entitled "Experimental Researches in Electricity, 16th Series." By Michael Faraday, Esq., D.C.L., F.R.S., &c.

Feb. 13.—The reading of a paper, entitled "Experimental Researches in Electricity, 16th Series." By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded. On the source of power in the Voltaic pile. An abstract of this paper has already appeared in our Number for April, p. 329 of the present volume.

Feb. 20.—A paper was read, entitled "On the Wet Summer of 1839." By Luke Howard, Esq. F.R.S. &c.

The observations of the author were made at Ackworth, in Yorkshire; and the following are his results with regard to the mean temperature and the depth of rain, in each month, during 1839.

Mean Temp.		Rain. in inches.	Mean Temp.		Rain. in inches.
	°			°	
Jan.	37·04	1·13	July	59·30	5·13
Feb.	39·64	2·14	Aug.	58·09	2·94
March	39·08	3·21	Sept.	54·49	3·43
April	44·09	0·58	Oct.	48·39	3·40
May	49·94	0·38	Nov.	43·14	4·54
June	56·35	4·89	Dec.	37·29	1·85

Mean temperature of the year 47·24°.

Total depth of rain in 1839, 33·62 inches.

He states that the climatic mean temperature of the place is about 47°, and the mean annual depth of rain about 26 inches. The excess of rain during the year 1839 was therefore very great.

The author describes the effect of the hurricane of the 7th of January, and follows the changes of the weather during the remainder of the year.

A paper was also in part read, entitled "On the chemical Action of the Rays of the Solar Spectrum on preparations of Silver and other substances, both metallic and non-metallic, and on some photographic processes." By Sir John F. W. Herschel, Bart. V.P.R.S. &c.

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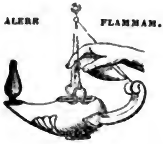
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JOURNAL OF SCIENCE.

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AND
ROBERT KANE, M.D. M.R.I.A.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XVII.

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I. Illustrative of Mr. Griffith's Paper on the Mineral Structure of the South of Ireland.
II. Illustrative of Dr. Faraday's Letter to M. Gay-Lussac, on Magneto-electric Induction *.

* This Plate will be given in one of the Number of the next Volume.

ERRATA, &c.

P. 145, line 17 from the bottom: the following reference ought to have been *inserted* in this place: "An abstract of Prof. Faraday's Seventeenth Series of Researches in Electricity appeared in vol. xvi. p. 336."

P. 370, line 12; p. 372 line 15; and p. 374, line 8 from the bottom, *for* H. G. ARMSTRONG, *read* W. G. ARMSTRONG.

THE
LONDON AND EDINBURGH
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[THIRD SERIES.]

JULY 1840.

I. *On the Iodide of a new Carbo-hydrogen.* By JAMES F. W. JOHNSTON, *F.R.S.**

IF coal gas be made to pass slowly and for a length of time over pure iodine, the latter substance moistens, and is partly changed into a dark-brown liquid, which effervesces with alkaline carbonates, showing the presence of hydriodic acid. After some hours, colourless prismatic crystals shoot out from the iodine, and clothe the interior of the vessel, and ultimately the whole is changed into a mixture of different compounds forming an olive-coloured substance, partly coating thickly the sides of the vessel, and partly constituting an unctuous mass, with the dark fluid at the bottom.

The liquid contains free iodine and hydriodic acid. When washed out from the solid portion by alcohol and neutralized by caustic potash, the solution gives a yellow precipitate, consisting of a mixture of Faraday's iodide of carbo-hydrogen ($H_2\,C_2\,I$)† and of iodide of formyle (iodoform $H\,C_2\,I_3$).

The solid product being exposed to the air loses its unctuosity. If broken up and examined by the microscope it is seen to consist of a congeries of colourless prisms ($H_2\,C_2\,I$) mixed with another substance, which is amorphous, and of a dark green almost black colour. Alcohol separates the former, or if the mixture be exposed to the air they volatilize, leaving the dark green substance nearly pure.

The production of the hydriodic acid and of the iodides of formyle and of carbo-hydrogen is easily understood. Coal gas contains probably more than two equiatomic compounds of carbon and hydrogen; at least two, $C\,H_2$ and $C_2\,H_2$, the light carburetted and olefiant gases, are present. The latter would furnish the three compounds obtained in this experiment, as shown by the following formula:

---

* Communicated by the Author.

[† Mr. Faraday's account of this substance will be found in Phil. Mag. First Series, vol. lix. p. 352. EDIT.]

$$2\,(C_2\,H_2) + 5\,I = H\,I + C_2\,H_2\,I + C_2\,H\,I_3$$

that is to say, one atom of olefiant gas decomposes to form hydriodic acid and formyle, while another unites with iodine* directly. Still this does not represent the action *quantitively*, since the proportion of the $C_2\,H_2\,I$ is much greater in actual experiment, and appears also to be variable.

Such is the action in close vessels provided only with a small aperture to allow the current of gas to pass out very slowly; but since these three compounds are all volatile, it is easy to understand how only the solid dark green fixed substance should be obtained when the iodine is placed in an open vessel and a current of coal gas is made to stream upon it. In this way it was first obtained by Mr. Kemp of Edinburgh, to whom the discovery of this substance is due, and who several years ago presented me with a specimen prepared by exposing iodine for several days to the action of an open jet of coal gas. I am not aware how far Mr. Kemp has since studied the action in close vessels.

I. This substance is of a dark olive green colour, is without taste, emits a slight odour of naphtha, is brittle, and has a density of about 0·95. It is insoluble in water, and in boiling alcohol or æther. Treated with hot nitric acid it becomes yellow and dissolves. With muriatic acid either in the form of gas or of liquid acid, it undergoes no change. Sulphuric acid aided by heat decomposes it. It blackens and gives off iodine vapour and sulphurous acid, leaving undissolved a very bulky charcoal. Dry chlorine slowly changes its colour to a dark brown. If previously moistened with alcohol it becomes yellow by the action of chlorine. In hot solutions of carbonated alkalies it is partly decomposed, iodine being separated, but its colour remains unchanged. Heated to 212° Fahr., it slowly but sensibly loses weight, evolving the odour of naphtha and a little iodine. At a higher temperature it gives off a volatile combustible liquid resembling naphtha, which burns with much smoke, and if the heat be still increased, iodine vapour appears in large quantity colouring the naphtha (?) dark brown, and a bulky shining charcoal remains behind. It is not unlikely that hydriodic acid may also be among the products.

---

* During the combination of chlorine with olefiant gas a portion of muriatic acid is formed, a fact inconsistent with the idea of a *direct* union of the two substances to form $H_2\,C_2\,Cl$: may not an equivalent proportion of the volatile chloroform be produced, as in the above formula, substituting Cl for I? Felix d'Arcet (*Ann. de Chim. et de Phys.*, lxvi. p. 108.) has stated, that during this action of chlorine on olefiant gas, a second oily liquid is formed, represented by $C_4\,H_4\,Cl\,O$, to which he gives the name of *chloretheral*, but which Berzelius with great probability I think, considers to be a compound of the chloride with the oxide of *elayle* ($C_2\,H_2\,Cl + C_2\,H_2\,O$). This explanation of the production of muriatic acid, however, implies that the gases employed are always more or less moist. Regnault accounts for the presence of the acid by representing the oily compound by the rational formula ($C_2\,H_3\,Cl + H\,Cl$) part of which is decomposed during the process, and H Cl evolved. But Löwig and Wiedman have shown that $C_2\,H_3$ (acetyle) does not preexist in the oil, though it may possibly be formed by its decomposition. See Poggendorff's *Annalen*, xlix. p. 133.

Burned with oxide of copper this substance gave the following results:

1. 8·77 grs. gave $\ddot{C}$ = 18·56 and $\dot{H}$ = 4·94 grs.
2. 8·77 grs. gave $\ddot{C}$ = 18·15 and $\dot{H}$ = 4·855 grs.
3. 6·137 grs. gave $\ddot{C}$ = 12·316 and $\dot{H}$ = 3·505 grs.

These are equivalent, per cent., to

	1.	2.	3.
Carbon....	= 58·203	57·225	55·490
Hydrogen	= 6·258	6·151	6·346
Iodine......	= 35·539	36·624	38·164
	100	100	100

It was not till I observed the discrepancy between the first and second analyses that I studied the action of a temperature of 212° Fahr. on this compound, and found that it was slowly decomposed, and iodine expelled from it by this degree of heat. The third analysis therefore was made with more precaution, and care was taken to avoid decomposition by the application of heat while pumping out the moisture from the oxide of copper. In this analysis therefore the chances of error were least, and the result agrees very closely with the formula $C_{30} H_{20} I_1$ since

		Calculated.	Experiment.
30 carbon...	= 2293·110	55·667	55·490
20 hydrogen	= 249·592	6·059	6·346
1 iodine....	= 1578·290	38·274	38·164
	4120·992	100	100

The excess of hydrogen is due to the more imperfect manner in which it was necessary to pump out the water in order to avoid the separation of iodine, as had probably been the case in the previous analysis*. The experimental result however may be reconciled to the formula ($C_{30}\ H_{20} + H\ I$) which gives 6·340 of hydrogen per cent., a quantity so near to that found as to leave nothing for the ordinary error of analysis. The action of chlorine hereafter described, if the results are to be depended upon, gives a probability to this formula, in addition to that which is derived from the rational formula, adopted by Liebig to represent the constitution of what by other chemists are still regarded as chlorides and iodides of olefiant gas (elayle) and some other carbo-hydrogens.

It has been already stated, that when boiled in carbonated alkalies the colour of this compound remains unchanged, and that it undergoes decomposition. The decomposition however is only partial. 4·51 grs. boiled in a concentrated solution of carbonate of soda, and afterwards washed and dried at a gentle heat, still weighed 3·51 grs. having lost 22·17 per cent. The whole of the iodine therefore is not separated by this process; it may however be completely separated by mixture with pure carbonate of soda, and gradually heating over the lamp to a temperature below redness.

In this way I obtained by means of nitrate of silver an approximation to the quantity of iodine, which however was too rude to be worthy of insertion in the present paper. I was, at the time of making the experiment, unacquainted with the more perfect method of estimating the quantity of iodine since published by Lassaigne.

II. Diffused through water and subjected to the action of chlorine, the green colour of this compound is slowly changed to brown, but the action is much more rapid and complete when the iodide is reduced to fine powder, diffused through alcohol, and submitted to a current of chlorine. Thus treated it speedily acquires a bright yellow colour, combining with chlorine and yielding the iodine to the supernatant liquid, in which it is readily recognised.

I subjected to analysis a portion of the substance thus prepared, after washing with alcohol and drying at 212° Fahr. When heated in a close tube it gave off no iodine.

a. 8·45 grs. gave $\ddot{C} = 18{\cdot}81$ grs. and $\dot{H} = 4{\cdot}545$ grs.

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* 2·795 grs. heated to 212° for some time, lost 0·295 gr., and at a subsequent weighing the loss amounted to 0·485 gr. Still it seems possible to preserve it for a long time at ordinary temperatures and in close vessels without sensible decomposition. One of the specimens employed in the above analysis was prepared by Mr. Kemp, and had been in my possession several years.

The water in the chloride of calcium tube reddened litmus, indicating the presence of muriatic acid, by which the weight of water would be in some measure increased.

b. 7·61 grs. heated with dry carbonate of soda, gave 7·033 grs. of chloride of silver or 22·8 per cent. of chlorine.

c. 4·462 grs. heated in like manner, but with more care, gave 4·517 grs. of chloride of silver or 24·12 per cent. of chlorine.

These results give for the composition of the yellow matter

	A	B	
Carbon...	= 61·55		= 30 atoms.
Hydrogen	5·98		17·8 —
Chlorine...	24·12	22·8	2·02 —
Oxygen ...	8·35		3·11 —
	100		

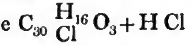
This result indicates the irrational formula $C_{30}\ H_{17}\ Cl_2\ O_3$, which gives

30 carbon	= 2293·11	=	62·13	per cent.
20 hydrogen	212·15		5·75	—
2 chlorine	885·30		23·99	—
3 oxygen	300·00		8·13	—
	3690·56		100·	

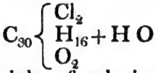
The elements contained in the above irrational formula are capable of being arranged in several rational positions.

The green iodide being $C_{30}\ H_{20} + I$,

The yellow substance may be $(C_{30} \substack{H_{17} \\ O_3} + Cl) + Cl$ (1), in which three of hydrogen are replaced by three of oxygen, the atom of iodine by one of chlorine, and the whole combined with another atom of chlorine—the oxygen being derived from the alcohol, which was undergoing a simultaneous decomposition by the action of chlorine.

Or, it may be  . . . (2.)

$$C_{30} \substack{H_{16} \\ Cl} O_3 + H\,Cl$$

Or —  . . (3.)

$$C_{30}\left\{\begin{array}{l} Cl_2 \\ H_{16} \\ O_2 \end{array}\right. + H\,O$$

in both of which the principle of substitution is equally evident. Were we to represent the green compound by $C_{30}\ H_{20} + H\ I$, there would even be a conservation of the original *type* of composition *. This type may also be supposed to be sufficiently preserved in the radical, and that it is owing

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[* See the memoir of M. Dumas, p. 442 of the last volume.—EDIT.]

to the presence of much muriatic acid in the solution, derived from the action of the chlorine on the alcohol, that this radical $\frac{30}{20}$ R unites with H Cl (2°) instead of simply with Cl, when it might have been represented by a formula * the exact counterpart of that which indicates the substance from which it is derived.

There is, however, still another mode of representing the rational constitution of this substance, which while it is accordant with the *facts* on which Dumas's views are founded, is inconsistent with the *principle* of the conservation of types. The yellow compound may be

$$(C_{15}\,H_8\,O_2 + H\,O) + (C_{15}\,H_8\,Cl_2) \qquad . \quad . \quad . \qquad (4.)$$

consisting of equal atoms of an analogous oxide and chloride of the radical $C_{15}\,H_8$ or $C_{30}\,H_{16}$, with the former of which is combined also an atom of water. This mode of representing it is accordant with the views of Berzelius, and is supported by many interesting and striking analogies.

We ought indeed to distinguish carefully between the *fact* of the mutual substitution of hydrogen and chlorine, and the *theory* of the persistence of types, or the opinion that the element which replaces performs the same function in the organic compound as that which is replaced. Of the former there can be no doubt, while the adoption of the latter as a principle is as yet attended with many difficulties and apparent anomalies, which do not present themselves when we regard these altered compounds after the manner in which our yellow substance is represented in formula 4.

New views all tend to hasten forward science, but a new view is not in itself necessarily an advance. It may often serve as a useful guide-post, when it does not directly help us on our way. Such good results are sure to follow from the discussion of the theory of substitutions, though all the views of its eminent author should not find a permanent place in the science.

I speak with the less confidence in regard to the above formulæ, because I am sensible that the examination of the two compounds described in this paper is by no means complete: a more careful research into the properties and chemical relations, especially of the first of them, would be likely I think to lead to interesting results.

The analyses above given were made as far back as 1838, and in February 1839, and the investigation was left unfinished till I should obtain a fresh supply of the compound. My attention has recently been recalled to the subject by a paper on *Hellenine*, by M. Gerhardt (*Ann. de Chim. et de Phys.* vol. lxxii. p. 163), in which he gives for this substance the formula $C_{15}\ H_{10}\ O_2$ or $C_{30}\ H_{20}\ O_4$, containing apparently a radical isomeric with the carbo-hydrogen $C_{30}\ H_{20}$, which exists in the iodide above described. By the action of chlorine, hellenine becomes $(C_{15}\ H_{10}\ O_4 + C_{15}\ H_{10}\ Cl_4)$ according to Berzelius, or $(C_{15}\ H_9\ Cl\ O_2 + H\ Cl)$ according to Dumas, in which we see a considerable analogy with the formulæ for the oxichloride above described. By the action of anhydrous phosphoric acid on hellenine, a yellow liquid carbo-hydrogen is produced, to which M. Gerhardt gives the name of hellenene, and which is represented by the formula $C_{15}\ H_8$, — the hypothetic radical which enters into the constitution of our oxichloride as it is represented in the formula (4).

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* $C_{30}\,\genfrac{}{}{0pt}{}{H_{16}}{Cl}\,O_3 + Cl.$

These interesting approximations indicate a series of comparative experiments, to which the iodide described in the present paper might be subjected, with the hope of throwing new light on the nature of the ever-varying isomeric modifications, of which the compounds of carbon and hydrogen are susceptible. I hope to be able soon to return to the subject with a view to this investigation.

In regard to the presence of a carbo-hydrogen represented by the formula $C_{30}\ H_{20}$ in coal gas, it need not excite our surprise if many other such compounds should hereafter be met with among the volatile and gaseous products obtained from the distillation of coal. When we consider how many less volatile substances of this class have been extracted by Pelletier and Walter* from the products of the distillation of resin for the manufacture of gas, and how many more volatile ones have been separated by Couerbe† from the gas thus produced when subjected to pressure, we shall be prepared to expect in coal gas also, the vapours of many other volatile substances in addition to those which have hitherto been detected.

I have not as yet proposed any name for the supposed radical $C_{30}\ H_{20}$. It belongs to the same group as mesitylene (Enyl of Berzelius) $= C_6\ H_4$ and retinyle $C_{18}\ H_{12}$, in both of which the elements are in the ratio of 3 to 2. It would be exceedingly desirable to adopt the system of nomenclature proposed by Berzelius for these compounds, in which the name is compounded of the Greek numerals expressive of the number of atoms of each element which are contained in the compound. But that such names may be univerally adopted, it is necessary that the same atomic weights should also be generally received. In the present case, for example, our green compound would be represented

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* Poggendorff's *Annalen*, vol. xliv. p. 81.

† *Ann. de Chim. et de Phys.*, vol. lxix. p. 148.

By $C_{30}H_{40} + I_2$ according to Berzelius.
By $C_{60}H_{40} + I_2$ according to Dumas.
And by $C_{30}H_{20} + I$ according to British chemists.

The compounds of carbon and hydrogen therefore, on the principle of Berzelius, which abstractedly is very valuable as a guide for general nomenclature, would receive in the works of different chemists at least two, and sometimes three different names of foreign origin. Trivial names therefore derived as heretofore from different and various sources, will be likely in the present state of the science to create much less confusion in our rapidly extending, already exceedingly difficult and almost Protean nomenclature.

Durham, May 16, 1840.

II. *On the Optical Characters of Greenockite (Sulphuret of Cadmium). By* JAMES D. FORBES, *Esq., F.R.SS. L. and Ed., Professor of Natural Philosophy in the University of Edinburgh.*[*]

IT appears from a late number of Professor Jameson's Journal, that the crystallographic characters of this new mineral, as examined by Mr. Brooke, remain ambiguous, and that the crystals sent to him for examination do not enable him positively to say whether it belongs to the rhombohedral or to the prismatic system; it may therefore be interesting to state that I have discovered that it possesses only *one* axis of double refraction in the direction of the axis of the pyramid or prism in which it usually crystallizes, and consequently there can be no doubt that greenockite is a rhombohedral cadmium blende.

Edinburgh, May 18, 1840.

III. *On a new Species of Biliary Calculus. By* THOMAS TAYLOR, *M.R.C.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WHILST engaged in examining the extensive collection of calculi in the museum of the Royal College of Surgeons, which had been entrusted to me for that purpose by the Board of Curators, I remarked, among those in the Hunterian collection, one, which from its extreme lightness and peculiar appearance, I was led to suspect had been incorrectly described in the manuscript catalogue as consisting of the mixed phosphates.

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[*] Communicated by the Author. We were favoured with this article in the middle of May, but from an oversight which we regret its insertion was omitted.

This calculus was externally of a dirty white colour, and had the greasy feel of cholesterine calculi; it floated on water, and when applied to the tongue, left an impression of bitterness. It was of an oval figure slightly flattened, one inch and a half in length, rather better than an inch in thickness, and about one inch and a quarter in breadth, but being broken in this direction its exact measurement could not be ascertained. It readily yielded to the knife, and the cut surface presented a polished appearance: its structure was lamellar, being composed of white and reddish-yellow layers arranged concentrically and alternating with each other. The layers were easily separable: at its centre there was a small vacuity.

When heated before the blowpipe it readily fused, then caught fire, burning with a clear flame and giving out the smell of animal matter, but nothing of a urinous character. It left a carbonaceous residue, which by raising the heat was converted into a white ash. This ash was alkaline, dissolved in water and dilute acetic acid, and the solutions gave a white precipitate with oxalate of ammonia; it was therefore lime.

When digested in boiling water, the water became slightly brown, but no apparent solution took place: the water on evaporation left a transparent yellowish-brown residue, which had a bitter taste and resembled inspissated bile.

Boiling alcohol extracted from it only a minute quantity of white fatty matter, which was deposited on cooling.

A solution of caustic potass removed the whole of the colouring matter, but the rest of the calculus was unacted on: the potass solution was dirty green, and when neutralized with muriatic acid deposited a scanty precipitate of the same tint.

When digested in nitric acid, effervescence took place, with the escape of a little nitrous acid; it then melted into a transparent oil, which on cooling concreted into a white fatty matter. This substance, when washed with distilled water, melted at a temperature much below that of boiling water.

When, instead of nitric acid, muriatic or acetic acid was employed, the portion of calculus did not melt until it had been removed from the acid; it then presented similar appearances to that obtained by the action of nitric acid; consequently this white fatty matter was not formed by the action of the nitric acid.

In all these cases the acids retained lime in solution. The fatty matter separated by the action of acids was partially soluble in boiling alcohol, and the solution on cooling deposited shining crystalline scales. With caustic potass it formed a ropy almost gelatinous solution, and was precipitated in white flakes on the addition of an acid. A small piece being placed upon the ball of a thermometer previously heated, began to solidify when the temperature had sunk to about 135° Fahr.

From these experiments I concluded that this calculus consisted of margarate, or stearate of lime, mixed probably with the oleate of the same base and some of the other constituents of the bile. That the lime was in combination with the fatty acid, was indicated by the insolubility of the calculus either in alcohol or caustic alkaline solutions, until it had been previously digested in some acid.

The minute quantities on which I had hitherto operated prevented me from determining whether only one or more of the fatty acids were present. The following analysis was therefore made.

Analysis.—12·80 grs. of the calculus previously dried *in vacuo* over sulphuric acid were boiled in distilled water: a peculiar odour was given off, and the water acquired a yellowish-brown colour: being evaporated to dryness it left a transparent resinous-looking residue, which weighed 0·84. This residue when digested in alcohol left 0·24 in the form of dirty yellow flakes, which in distilled water swelled up and ultimately dissolved, forming a solution which in its chemical characters exactly resembled that of the mucus of the gall bladder.

The alcoholic solution being evaporated to dryness, the residue was redissolved in water; the solution was intensely bitter; with muriatic acid it gave a copious viscid precipitate: acetate of lead produced likewise a viscid precipitate, and the supernatant liquor when clear was again troubled by a solution of subacetate of lead.

The 0·84 consisted therefore of mucus of the gall bladder 0·24; inspissated bile 0·60.

After water had extracted from the calculus all that it was capable of dissolving, it was treated with successive portions of boiling alcohol sp. gr. ·830.

The first alcoholic solution on cooling deposited a white matter, which did not readily redissolve in hot alcohol or æther, but was acted upon by acetic acid. It appeared to be part of the calculus that had been dissolved unchanged; the quantity was however too minute to be estimated. The alcoholic solutions were filtered, and being mixed together, the whole was gently evaporated; as the liquid became concentrated, it deposited some white fatty matter and acquired a yellow tinge; a residuum was ultimately left, which had the appearance of a mixture of a fluid and concrete oleaginous substance. On the application of heat it became a yellow oil, which on cooling only partially solidified: it weighed 0·47.

It strongly reddened litmus paper; dissolved readily in a cold solution of caustic potass; and was precipitated in soft flakes on the addition of an acid.

This substance consisted therefore of oleic acid, mixed with margaric or stearic acid.

Strong acetic acid diluted with twice its bulk of water was now poured over the calculus, and the action of the acid aided by a gentle heat. The insoluble residue was collected on double filters, washed, and dried.

The acetic solution with its washings was reduced to a small bulk, and a solution of ammonia added; after the lapse of several hours no precipitate appearing, the excess of ammonia was nearly neutralized by a solution of oxalic acid: a white precipitate fell, which when washed, dried, and heated to dull redness in a platina crucible, left 2·09 carbonate of lime = 1·17 lime.

The remaining liquid being evaporated to dryness and the ammoniacal salts expelled, a residuum was left which weighed 0·10: water dissolved a portion of this; the solution was alkaline, and when evaporated minute crystals were formed, which slightly effervesced in acetic acid: their solution not precipitating chloride of platina, leaves little doubt of their being carbonate of soda: the small portion which remained undissolved proved to be carbonate of lime.

The matter left upon the filter after the action of the acetic acid was again digested in boiling alcohol, a considerable portion dissolved, and the remainder had acquired a much deeper colour: it was collected on the same filters, which were repeatedly washed with boiling spirit; when dried and weighed against the outer filter it amounted to 0·86.

This substance possessed a brownish-yellow colour. It dissolved in solutions of caustic and carbonate of potass, forming solutions having nearly the same colour.

Muriatic acid rendered it green, and when added to its alkaline solution threw down green flocks.

With nitric acid it formed a red solution.

This substance was therefore identical with the colouring matter of the bile, and which forms the principal constituent of the biliary calculi of oxen.

The alcoholic solutions were concentrated by careful distillation in a small retort: the liquid remaining in the retort, when cold, formed a soft crystalline mass, composed of brilliant plates and having a pearly lustre, very much resembling margaric acid.

This substance, when fused and kept for some time *in vacuo* over sulphuric acid, weighed 8·88. It melted at 136° Fahr., and on cooling became a crystalline solid, reddened litmus paper, and was easily soluble in a cold solution of caustic potass; the solution when concentrated was ropy and gelatinous; when dilute it formed a slightly milky mixture with minute glistening particles floating in it; on the addition of an acid, the substance was thrown down in the form of white flakes, which possessed the same properties as before solution. When boiled with the alkaline carbonates, it was dissolved, with the escape of carbonic acid. By redissolving it in hot alcohol, crystalline plates were deposited on cooling, which after washing with cold spirit fused at about 140°. The low fusing point of this substance evidently indicates the presence of oleic acid. In order to ascertain whether the crystals fusible at 140° were pure margaric acid or stearic acid rendered more fusible by an admixture of oleic acid, they were again dissolved in warm spirit, and the crystals as soon as formed dried by compression between folds of blotting paper; by repeating this process two or three times, the fusing point was raised to nearly 160°. This must therefore be regarded as pure stearic acid; and as I find that both stearic and margaric acids require to be several times recrystallized from their alcoholic solutions to free them from even small quantities of oleic acid, and as no decided indication of the presence of margaric acid could be detected in the mother liquors, I am inclined to believe that oleic acid had only been separated by the above treatment, and that consequently margaric acid did not enter into the composition of the calculus. It would however be impossible to speak decidedly on this point.

The result of the analysis is as follows:

Stearic acid mixed with a small proportion of oleic acid	9·35
Lime	1·17
Soda with a trace of lime	0·05
Yellow colouring matter of the bile	0·86
Inspissated bile	0·60
Mucus of the gall-bladder	0·24
	12·27
Loss	53
	12·80

The greater part of the loss in this analysis should be added to the stearic acid, as owing to the sudden extrication of vapour while under the receiver of the air-pump a small part of the acid was thrown out; but I prefer giving the quantities actually obtained to making any allowance for known sources of error.

The composition of the calculus clearly points out its biliary origin, but whether taken from man or the brute must remain doubtful, as there is no history to guide us.

Stearic, margaric, and oleic acids exist in the bile of oxen in combination with soda; and according to Lecanu and Casaseca*, stearic and oleic acid in that of man; the latter I have frequently detected, but I cannot find that these acids have ever been noticed as entering into the composition of biliary concretions, much less forming the prominent constituent. I was unable to detect the slightest trace of these acids in five specimens selected from about 200.

From cholesterine calculi it is readily distinguished by the absence of any crystalline structure when broken, which unless the quantity of colouring matter be very large is always more or less apparent in that variety; also by its insolubility in alcohol or æther and by readily dissolving in these menstrua, and in a cold solution of caustic potass after it has been acted upon by an acid.

Before I conclude, I am anxious to rectify an error which I inadvertently committed in my paper on the calculi in the museum of St. Bartholomew's Hospital†. I there stated that urate of ammonia had always been confounded with the uric acid variety in the tables that had been published on the relative frequency of the different species of calculus‡. This was not, however, the case in a paper by Dr. Yelloly, in the Philosophical Transactions for 1829–30, containing the analysis of the Norwich collection, and I regret that I have not had an opportunity of making this acknowlegement sooner.

April 8th, 1840,
New Bridge Street, London.

THOMAS TAYLOR,
M.R.C.S.

N.B. A notice of this calculus was sent to the Board of Curators in March 1839, but a variety of circumstances have delayed its publication.

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* Gmelin's *Handbuch der Chemie*: *Journal de Pharmacie*, 12.

† L. & E. Phil. Mag. vol. xii. p. 412.

‡ *Ib.* p. 414.

IV. *On the Anatomy of the Brain.* By Dr. Foville, *M.D.Par.*[*]

It is more than twelve years since Dr. Foville laid before the Royal Academy of Sciences of Paris some highly interesting discoveries respecting the anatomical structure of the brain. They were the results of laborious researches in what at that period he had already been long engaged. At the time they attracted the careful attention of the anatomical members of the Institute, and more especially of the late Baron Cuvier, and of Professor Blainville, and an able and highly favourable report on the subject was presented to the Academy by the latter *savant*. Translations of this Report and of Dr. Foville's Memoir were published soon after in the Annals of Philosophy[†]. From that time to the present the Doctor has been pursuing his investigations, and has made new discoveries as well as confirmed his old ones, and has been engaged in preparing a detailed and extended work on the anatomy, physiology, and pathology of the brain and spinal cord. He laid a condensed view of the anatomical facts before the Medical Section of the British Association at its meeting in Birmingham, and has presented a further memoir to the Academy of Sciences of Paris. The Doctor has thrown the substance of both papers into the following description.

Medical men in general are so well aware of the importance, and at the same time of the difficulty of the study of the nervous system, that the physician who attempts to communicate to his professional brethren the result of his researches, may flatter himself that he is speaking on a subject which cannot fail to be interesting. Hence in desiring to communicate some results of my anatomical researches concerning the brain, I need not solicit the indulgence of the meeting for the subject itself, which has for many years engaged my attention, but I must apologise for the imperfect style of the present sketch. I arrived in London only a few days ago in order to visit my friend Dr. Hodgkin, and had not anticipated his inviting me to join him at the meeting of the British Association for the Advancement of Science, and present to this meeting an abstract of my researches into the structure of the encephalon. Having left my manuscript, now nearly ready for publication, in France, and not having time and opportunity to procure new preparations from which I might write the present essay, I have been obliged to depend on my memory for the description of some structural arrangements which have arrested my attention.

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[*] Communicated by Dr. Hodgkin.

[†] Phil. Mag. and Annals, N.S. vol. v. p. 278.—Edit.

When we look at the works which treat of the anatomy of the brain, we find that they may be distinguished into two classes. Some being designed to illustrate the form of the brain in general, and to indicate the particulars of its external surface as well as of its ventricular cavities, only exhibit its substance by sections, which destroy the arrangement of the parts without showing their structure. Others, being composed by authors aware of the imperfection of those processes by which nothing but superficial appearances and casual sections are exhibited, aim at elucidating the mysteries of the cerebral organization by studying the composition and arrangement of the substances constituting the encephalic masses.

It is needless in speaking to so enlightened an assembly as this to draw any comparison between the advantages of these two different methods. Centuries have passed since the superiority of that which is designed to ascertain the intimate composition and structure of the parts was first recognised by some anatomists. The celebrated Willis has insisted as strongly as any modern writer on the advantages of the process alluded to; and Malpighi proved its excellence when he so accurately described the granular disposition of the gray matter and the fibrous structure of the white. Nevertheless in spite of these superior men, their improved views did not generally prevail in the schools; and when at the end of the last century, Reil, and at the beginning of the present, Gall, undertook to prove the fibrous structure of the white substance of the brain, they thought they were announcing an anatomical discovery.

Reil by his writings, and Gall by his lectures and publications in which he was associated with Spurzheim, have propagated their ardour for sound anatomical inquiries. But notwithstanding their efforts, the practice of cutting through the brain is not yet abandoned, though it is generally understood that this old and imperfect method is insufficient of itself, and that it is by combining with it more appropriate means of investigation that facts which twenty years ago were controverted are now incontestably demonstrated. Thus, for example, all anatomists of the present day concur in the view that the white substance is fibrous, and the gray granular; and at the same time, contrary to the views of Gall, a great many physiologists agree in thinking that the gray matter is the active part in the functions attributed to the nervous system, and that the white fibres are in the brain as in the nerves, inert conductors. It still remains to determine which of these substances is pre-existent to the other; in what order the development of the different parts which they compose takes place, and what are the most essential differences in the nervous systems in different animals. It is sufficient to mention the name of Tiedemann to remind you of the light which has been thrown on these interesting and important questions.

I will not undertake to enumerate the many points which are yet to be elucidated. I will merely, before exposing my own views on the structure of the encephalon, allude to those of the most eminent anatomists who have attempted to determine the disposition of the two substances composing the encephalic masses.

Willis, Malpighi, Reil, Gall, and Spurzheim are of opinion that the white fibres of the crura cerebri, after emerging from the medulla oblongata, penetrate into the hemispheres, diverging in various directions till they meet the gray matter of the convolutions in which they terminate. They believe that the corpus callosum reaching from one hemisphere to the other is formed by the union on the median line of a new order of fibres, originating in that same gray matter of the convolutions, as that in which the fibres proceeding from the crura cerebri terminate. Gall clearly and distinctly expresses this opinion, when he says that the brain is formed of two different orders of fibres, the one of diverging, the other of converging fibres.

Tiedemann does not admit these two orders of fibres. According to his views, the fibres meeting in the corpus callosum are a continuation of those which proceed from the crura cerebri after they have gone through the entire circumference of the hemispheres.

Which of these different views is consistent with the truth? Is there no other difficulty than the choice between them?

It is pretty generally admitted that the encephalic masses are, according to the poetic language of Reil, "an efflorescence of the spinal marrow," the cerebrum being developed at the summit of its anterior, the cerebellum, at the summit of its posterior columns. The decussation between the two anterior pyramids belonging to the anterior columns is held to account for the effects of cerebral complaints being manifested on the side of the body opposed to their seat in the brain.

For this last fact to be true, it is necessary that the decussation between the two pyramids should be at once in communication with all the muscular nerves in the body and with all the parts of the brain, which being altered produce symptoms on the opposite side. Hence in our survey the necessity of tracing accurately the connexions between the pyramids and the other parts of the cerebro-spinal system.

The spinal marrow situated below the decussation of the pyramids is composed of two symmetrical portions, which are united by a commissure of white matter. Without entering into the consideration of how many distinct columns are to be found in each half, I will now point out the different appearances belonging to the anterior and posterior surfaces of its commissure.

Throughout its whole extent the anterior commissure is divided longitudinally by numerous small holes, which are never to be found absolutely on the median line, where there exists a kind of very small raphe. At its superior extremity this anterior surface of the commissure becomes more superficial, and gradually assumes something of the aspect belonging to the decussation of the pyramids; so much so, that it is not always easy to determine where the commissure ends and the decussation begins.

The posterior surface of this commissure, which is visible at the bottom of the posterior fissure, does not present the same appearance as the anterior surface. The two surfaces are so near each other that they seem at first to belong to the same fibrous layer; but a close examination demonstrates that this is not the case, for there is constantly a sensible interval between the two surfaces. On the median line, two small very fine parallel white layers pass from the one to the other. If this double commissure be compared to the corpus callosum and fornix, these white layers going from the one to the other bear some analogy to the septum lucidum in the brain.

I think this cavity has already been noticed by Malpighi; and Dr. Hodgkin believes that he has more than once observed it. Gall described a longitudinal cavity on each side of the spinal cord, but it has been ascertained that they were produced by blowing into the central longitudinal masses on each side of the marrow. Thus we find in the commissure two transverse bands of white matter uniting the two halves of the spinal marrow, which bands are connected together on the median line by a delicate double band of white matter between the layers, of which a natural cavity seems to exist.

Let us now inquire what becomes of the two commissures when they penetrate on each side into the substance of the marrow. It seems to me that they soon become united together so as to constitute a kind of axis, around which are attached the lateral fibrous columns by means of fibres radiating transversely from the former into the latter. It appears also that a greater number of the fibres going to the roots of the spinal nerves proceed directly from the axis itself. This distribution is more evident with respect to the posterior than to the anterior roots.

Without attempting to describe the lateral columns of the spinal marrow, I must not omit to mention a remarkable difference between the lateral parts of these columns and the anterior and posterior portions. If a portion of the spinal marrow be cut out transversely, freed from its membranes, and left to macerate in plain water for a few hours, the lateral parts of each half will assume, in consequence of the swelling of their fibrous fasciculi, an appearance something like that of a large nerve, whilst the anterior and posterior parts adjoining these lateral columns are uniformly swelled without presenting any such appearance. Let us now see how the axes of the superficial columns of the spinal marrow are connected with the parts developed at their upper extremity, that is to say, in the medulla oblongata.

The anterior part of the axis, in other words, the anterior part of the commissure, appears to go almost entirely into the decussation of the anterior pyramids, and into the pyramids themselves. A very fine commissure between the pyramids seems to be a continuation of the anterior commissure which exists throughout the whole length of the spinal marrow.

The posterior commissure ascending along the medulla oblongata comes very near to the surface, and when examined between the two lateral portions of the medulla, it seems to constitute a very delicate bond of union between the two sides of the calamus scriptorius. This union seems to be established by means of two slight fibrous columns, which are prolongations of the posterior commissure of the spinal marrow.

If these statements are correct, we see that when the prolongations of the commissures or naked surfaces of the axis of the spinal marrow are examined in the medulla oblongata, instead of being very near each other on the median line, they are separated by all the interval existing between the anterior and posterior surfaces of the medulla oblongata. But so slight are the remains of the commissures when examined in the medulla oblongata, that it is very easy to overlook them. When the two halves of the medulla oblongata are separated, and this separation is easily produced by introducing and gently pressing a probe between the two anterior pyramids or the two halves of the calamus scriptorius, they present to our view fibres going from before backwards, and so disposed, that those on the right side correspond to the intervals of those on the left. On each side they seem to pass from the pyramid before to some small columns which proceed from the posterior commissure behind. These two opposed layers of fibres passing from before to behind, correspond in the medulla oblongata to the double band having the same disposition in the medulla spinalis.

With the posterior bundles of the medulla oblongata, continuous with the posterior commissure of the spinal marrow, are connected those white fibres seen on the surface of the calamus scriptorius, which are said to go to the auditory nerve.

I must not forget to say, that from these same posterior ascending bundles proceed radiating fibres, going to the superficial columns of the medulla oblongata, in like manner as there are found radiating fibres proceeding from the axis of the medulla spinalis and going to its superficial columns.

These remarkable anatomical arrangements are not to be found in the spinal marrow and medulla oblongata alone; they exist still higher, in the crura cerebri. I must not now attempt to describe these connexions further, as I have several other points to notice, and shall therefore be glad if I have succeeded in conveying to you some idea of their character. Nor will I here undertake to describe the multifarious and highly complicated parts which are to be found in and above the medulla oblongata going to the cerebellum and cerebrum, into which many of them may be followed. I will merely remark, that among all these fibrous bundles those in communication with the anterior pyramids are the most simple and the most direct in their progress to the brain through the crus cerebri, for they constitute the inferior part.

This inferior part of the crus cerebri, flattened a little transversely at its entrance into the brain, is received into a transverse cavity.

The superior part of the same crus cerebri when entering the brain unites directly with the thalamus nervi optici, which seems to be an enlargement and modification of this part. When attentively examined, the thalamus nervi optici is seen forming with its appendages, of which the nervus opticus emerging from it is the most remarkable, a complete circle, or at least a rounded mass, about the inferior part of the crus at its entrance into the brain.

It seems highly important to distinguish with exactness the further disposition of these two distinct prolongations of the crus cerebri.

Internal to the fissure of Sylvius there is an elongated four-sided surface, of whitish colour, which is the only fibrous part of the external surface of the brain. This quadrangle is perforated by a great number of vascular holes, and its greatest dimension is from within outwards and backwards.

Its anterior margin is curved, and bears the external root of the olfactory nerve.

Its posterior margin, equally curved, bears a part of the optic nerve, which is prolonged outwards and backwards from the commissure.

At the anterior border of the same quadrangle is seen the base of the convolution which is in relation with the olfactory nerve.

To the extreme limit of its posterior margin is attached in some sort by its base the great tuberosity of the most internal convolution of the temporal lobe of the brain. This convolution is not less remarkable for its form and situation than for its surface, which appears invested by a whitish layer of matter perforated with holes like lace.

Internally this quadrangle is inclined towards the inferior part of the septum lucidum, and presents a swelling corresponding to the commissure of the optic nerves.

With the fibres composing the surface of this quadrangle are united all the roots of the olfactory nerve, except the anterior one, which is continued into the substance of the convolution on which this nerve rests.

With the fibres of the same quadrangle are also combined a layer of nervous matter and some radicular prolongations given off from the optic nerve.

The most internal portion of the quadrangle goes up under the septum lucidum to the base of the anterior part of the lateral ventricle. Its external portion, combined with the great tuberosity of the convolution of the cornu ammonis, or the convolution displaying the hippocampus major on its ventricular surface, corresponds with the deepest part of the descending cornu of the same ventricle in the temporal lobe.

By its adherent surface this quadrangle covers a part of the inferior extra-ventricular portion of the corpus striatum and thalamus nervi optici, and also receives a delicate layer of fibres which come from the corpus striatum and optic thalamus, in a line as distinct from that by which the fibrous planes proceed from the same parts to the internal surface of the convolutions, as that in which the anterior roots of the spinal nerves originate is distinct from that in which the posterior roots of the same nerves arise. Several radicular prolongations of the olfactory nerve go into the slight fibrous layer which is observable in the substance of the corpus striatum.

The anterior boundary of the fibrous quadrangle whose relations I have just now sketched, sends off a fibrous band, which connected at its origin with the base of the convolution of the olfactory nerve, ascends in front of the corpus callosum, to the side of which it is applied, winds round its anterior extremity, then follows its superior aspect, and descending behind its posterior border into the convolution which bounds the fissure of Bichat, returns to the tuberosity situated on the external edge of the quadrangle so often mentioned.

This fibrous band therefore, with the quadrangle in which it ceases at its two opposite extremities, describes a large circle in which the corpus callosum is inscribed. The circumference of this fibrous band forms the base of a convolution whose remarkable disposition has engaged the attention of anatomists. It forms with the latter, on the edge of the corpus callosum contiguous to the hemispheres and the fissure of Bichat, a sort of listing, or border, in which the gray substance of the convolutions ends. I have thought it proper in an extensive work on the anatomy of the brain to name this band cerebral border, and the corresponding convolution, convolution of the border.

Thus, without multiplying details, we find reaching all along the circumference, or if the term which I have proposed be approved of, along the contracted border forming the limit of the extensive gray membrane folded into convolutions on the surface of the brain, a fibrous cord, which forms along with the superficial quadrangle whence it is derived, a complete circle round the corpus callosum, and this quadrangle and its immediate dependence communicate on the other side with the sensorial nerves of the brain.

Let us now take a rapid view of the fibrous parts of the hemispheres. They originate externally in a fasciculated fibrous layer interposed between the gray masses of the corpus striatum and optic thalamus, and go to the internal surface of the cerebral convolutions, whilst the fibrous layer whence they issue is continued into the inferior part of the crus cerebri, and consequently into the anterior pyramids. On the outside of the corpus striatum and optic thalamus, these fibrous planes of the hemisphere separate into two strata, upon which the cerebral convolutions are raised; and each of these strata or secondary planes becomes united to the external margin of the circular band forming the border of the hemispheres. One of these unions takes place in the hollow of the fissure of Sylvius; the other in all the rest of the internal circumference or border of the gray substance of the convolutions.

Of these two orders of fibres thus differently disposed, the one then communicates with the posterior part of the crus cerebri and the nerves of sensation; the other with the inferior part of the crus cerebri and the anterior pyramids, which decussate like the effects of cerebral lesions bearing upon the powers of motion. The first order of fibres are applied to the circumference of the convolutions very near the median line, and the circle which they form is on a vertical plane, directed antero-posteriorly; the second, radiating from within outwards towards the middle of the convex surface of the hemispheres, spread themselves out from thence towards the termination of the convolutions round the corpus callosum, and become connected here with the external margin of the circular band, which extends to the same point of the convolutions.

This remark on the relation of the convolutions to the two orders of fibrous parts, of which the one proceeds from the anterior pyramids and the anterior parts of the spinal marrow, whilst the other is intimately connected with the sensorial nerve of the brain, and the posterior parts of the spinal marrow, points out the manner in which the cerebral convolutions should be studied.

If our description is confined to their forms, nothing of importance will be revealed. But if on the contrary we endeavour accurately to determine their relation to the other parts of the system by means of the fibrous parts entering into their composition, the mind will be prepared to apprehend the different offices they are intended to fulfil. This is what I have endeavoured to accomplish in the first place in the human brain, and secondarily in that of a considerable number of mammiferous animals. I shall subjoin here an abstract of my researches on this subject.

Considered in reference to the different connexions of their fibrous parts, the convolutions may be distinguished into two principal classes: first, those clothing the prolongations in the brain of the fibrous bands connected with the olfactory lobes, optic nerves, and posterior parts of the medulla spinalis; and second, those enveloping the cerebral terminations of the pyramidal fasciculus of the crus.

The convolutions connected with the prolongations in the brain of the fibrous parts which proceed from the basilary quadrangle, or the common meeting-point of the sensorial nerves and the posterior parts of the medulla, constitute all the plain internal part of the hemisphere, the surface of the basilary cerebello-temporal zone, and the lobule of the insula. Those developed on the terminations of the pyramidal fasciculus of the crus, constitute all the external portion of the hemisphere and the concave surface of the orbital region of its base. The respective limits of these two classes of convolutions are indicated on the one hand by a grand line of convolutions which courses along in its whole extent the large circumference of the hemisphere, commencing in front at the anterior margin of the perforated quadrangle, and terminating behind at the posterior margin of the same quadrangle; and on the other hand by another line of convolutions, which forms the inclosure of the fissure of Sylvius, arising before and ending behind like the preceding grand line at the opposite margins of the perforated quadrangle.

Thus contiguous and united at their extremities, these two lines diverge from each other throughout the rest of their extent, the grand line traversing successively the internal border of the orbital region of the base of the brain, the great convex border of the hemisphere, and lastly, the external border of the basilary cerebello-temporal zone; whilst the convolutions of the inclosure of the fissure of Sylvius travels in succession along the anterior border, the superior border, and lastly, the inferior border of the fissure which it bounds.

All the principal divisions found by each of these two lines of convolutions correspond to those formed by the other. That part of the large circumference which forms the internal border of the orbital region corresponds to the convolutionary inclosure of the fissure of Sylvius, forming the external limit of the same orbital region. The great convex boundary of the hemisphere corresponds to the superior border of the fissure of Sylvius; and lastly, the external border of the cerebello-temporal zone answers to the inferior border of the fissure of Sylvius. The anterior angle formed by the union of the first and second parts of the inclosure of the fissure of Sylvius, answers to the angle formed at the anterior extremity of the brain by the union of the great convex border of the hemisphere with the external border of the orbital region. The posterior angle of the fissure of Sylvius, subtended by the line which forms its superior border, meeting with that which forms its inferior border, answers to the angle formed by the meeting of the great convex border of the hemisphere with the external border of the cerebello-temporal zone at the posterior extremity of the brain. Lastly, as these two lines meet at their extremities, in contact with the perforated quadrangle, it is seen that the convolutions included in the interval between them occupying all the convex external aspect of the hemisphere and the orbital region of its inferior aspect, are quite distinct from those situated at the internal aspect and at the basilary surface; and lastly, from those which in the fissure of Sylvius itself constitute the lobule of the insula. Of the convolutions of the regions which I have just pointed out, those of the internal aspect of the cerebello-temporal zone are included between the line of convolutions which tracks the large circumference of the hemisphere, and that part of the convolution of the border which extends from the anterior to the posterior margin of the perforated quadrangle, following the curve from before to behind of the corpus callosum, and the fissure of Bichat. Lastly, the convolutions of the insula are included between the line of convolution forming the inclosure of the fissure of Sylvius, and that part of the convolution of the border, very short in man, on which is prolonged the external root of the olfactory nerve.

If we dissect the convolutions of the plain internal surface of the hemisphere, those of the cerebello-temporal zone, and lastly, those of the insula, we can easily show that their fibrous parts converge from the great circumference of the hemisphere, and of the convolutionary inclosure of the fissure of Sylvius towards the corresponding regions of the convolution of the border, and terminate at last in the fibrous band of the border itself, whose connexions with the perforated quadrangle, the sensorial nerves of the brain, and the posterior parts of the spinal marrow we are already acquainted with. On the contrary, dissection of the convolutions on the external convex surface of the hemisphere included between the two grand lines of convolution which bound this aspect, one of these lines traversing the great circumference of the brain, the other following and forming the boundary of the fissure of Sylvius, shows that the fibrous twig of these convolutions terminates in the plane of the hemisphere which emanates from the pyramidal portion of the crus cerebri.

As to the two lines of convolution situated on the limit of those convolutions which are united with the dependences of the border and of those which envelope the terminations of the plane of the hemisphere, they both pertain by one of their margins, to the productions of the border, and by the other to the productions of the plane of the hemisphere. They form then a means of union between the two orders of convolutions, of which the one is connected to the sensorial nerves and the posterior parts of the spinal marrow, and the other to the anterior pyramids and the anterior parts of the spinal marrow.

Thus all the convolutions developed in the interval of the two spaces between the external surface of the hemisphere and the internal and external margins of the orbital region, are in exclusive relation with the prolongations of the pyramidal fasciculus of the crus.

All the convolutions developed between the border, properly speaking, and the great circumference of the hemisphere, pertain to the fibrous emanations from the perforated quadrangle.

All the convolutions of the insula situated between the short region of the border which is traversed by the external root of the olfactory nerve, and the convolutionary inclosure of the fissure of Sylvius, belong peculiarly to the fibrous parts emanating from that region of the border to which belongs the external root of the olfactory nerve. Lastly, the great circumference in which are found the external surface of the convolutions belonging in particular to the terminations of the pyramidal fasciculus of the crus, and the internal surface of those which belong exclusively to the fibrous emanations from the perforated quadrangle, is destined by its double relations to a mixed purpose. It contains the anastomosis of the fibrous extremities of the pyramids and of the fibrous extremities issuing from the perforated quadrangle.

The convolutionary inclosure of the fissure of Sylvius, to the extra fissural margin of which are connected the convolutions developed upon the pyramidal termination of the crus, and with the fissural edge of which is combined the fibrous duplicature of the convolutions of the insula proceeding from that part of the border to which belongs the external root of the olfactory nerve, is destined, like the line of convolution which courses along the great circumference of the hemisphere, to contain the anastomosis of the fibrous parts belonging to the pyramidal convolutions with those which proceed from the border. They are thus in relation with the sensorial nerves of the brain, with the posterior parts of the crura, and with the medulla oblongata.

We might proceed to describe the various groups formed by the two orders of convolutions which we have now established; but it must not be forgotten that our present object is merely to give a succinct idea of the principal results of our anatomical investigations.

But I have as yet only pointed out a very trifling portion of the fibrous layers intermediate between the nerves of sensation of the brain, and I shall immediately proceed to mention the other parts which unite with the fibrous band and the superficial quadrangle previously described. Each of these parts forms, like the preceding, a circle which traverses the inferior part of the crus cerebri, some embracing this part of the crus cerebri like a kind of bracelet, others separating from it in the greater part of their course, but always coming back to unite at their termination with the white quadrangle of the fissure of Sylvius.

Setting off from the crus, the first of these parts is the white superficial covering of the optic thalamus, the fibres of which are circular.

The second is the tænia semicircularis, incomplete as a ring when considered alone, but completed by the quadrangle with which it directly unites in front, whilst behind it joins the great tuberosity of the convolution of the cornu ammonis, attached to the external part of the superficial quadrangle.

Thirdly, the corpus striatum itself forms by its gray matter alone a complete circle, or rather an ellipse, the inferior extra-ventricular part of which is covered by the quadrangle so often mentioned.

Fourthly, to the outer side of the corpus striatum there is a fibrous circle, which surrounds it as the tænia semicircularis compasses the optic thalamus. So far as I know, this fibrous ring has not hitherto been described.

Fifthly, the corpus fimbriatum and the corresponding half of the fornix form likewise a complete circle with the fibrous layer of the fissure of Sylvius. In the sixth place would come, if we had not before described it, the white band of the border.

In the seventh and last place, the two little bands situated upon the corpus callosum close to the median line, terminate, like all the preceding parts, at the anterior and posterior limits of the white perforated surface internal to the fissure of Sylvius.

The corpus callosum itself is in intimate relation with all these concentric circles. Its particular disposition was described in a memoir which I had the honour of reading before the Academy of Medicine at Paris in 1825; but the death of Beclard, who was appointed to report on my paper, deprived me of the judgement which that illustrious professor would have formed.

There is not, I believe, any communication between the fibrous expansions of the inferior part of the crura of the opposite sides of the brain.

It appears to me that in man the anterior commissure only unites those parts of the opposite sides which are connected with the nerves of sensation. The well-known fact that in many mammifera this commissure only stands on each side to the olfactory nerves instead of reaching from one hemisphere to the other, adds force to the opinion I express, that the anterior commissure only unites parts devoted to sensation.

I shall not insist further on this anatomical proposition, but if you will allow me briefly to add a few physiological deductions, I would say that the fibrous parts intermediate between the internal surface of the convolutions and the anterior pyramids, appear to me to be simple conductors, as well as those which unite the organs of sensation with the circumference of the gray substance of the convolutions; these transmitting to the muscles the influence of the gray substance which determines their contraction, whilst those convey from the organs of sense to the same gray substance impressions made on the surface of these organs.

The grey substance of the convolutions appears to me to be the material substratum, by the intervention of which the will directs the movements of the body. For the last twenty years lesions of this substance have been pointed to as those most frequently occurring in the insane, by those physicians who expect to find in the brains of such patients alterations corresponding to the characteristic symptoms of their disorders.

Atrophy of the convolutions, so frequently seen in dementia, appears to me to result from disuse of the functions of the gray substance; then the fibrous matter proceeding from this gray substance becomes atrophied also, just as the optic nerves fall into a state of atrophy in the blind.

Pathological anatomy furnishes numerous examples of lesions of the fibrous matter intermediate between the cortical substance of the convolutions and the anterior pyramids. Paralysis of the active organs of motion on the opposite side is generally the consequence.

The information afforded by pathological anatomy, relative to the effects of lesions affecting the fibrous parts intermediate between the gray substance of the convolutions and the organs of sense, is not so clear. This is owing, I imagine, to the communication in the median line between these fibrous parts of opposite sides, and the great number of these parts rendering the complete obstruction of the impressions they are meant to convey, difficult.

Before summing up these conclusions, relative to the structure and office of some parts of the brain, I should like to say a few words on the relations which the study of the cranium establishes between different regions of this bony case and the corresponding regions of the organ which it incloses.

If the two frontal eminences are divided horizontally by sawing on a plane perpendicular to their centre, and this section is carried to some depth in the brain, the lateral ventricles are opened by their anterior extremities, each of these extremities terminating in a cul-de-sac, answering to a frontal eminence. In the interval between these two cul-de-sacs of the anterior extremities of the ventricles we reach the anterior curve of the corpus callosum. If we saw in the same way the two superior occipital protuberances, we arrive at the posterior extremity of the two lateral ventricles, each of these extremities ending in a hollow cone, answering to one of the superior occipital depressions and eminences. Lastly, if the saw is made to divide the two parietal eminences at their summits, and that portion of the long vault intermediate between them, it leads to that part of the lateral ventricles which is the most spacious and projects the most outwards. It is in this part of the ventricles that is situated, so to speak, the confluence of the anterior, posterior, and temporal regions of these cavities. The same section which leads from the parietal eminences to that part of the lateral ventricles which is the most spacious and projects the most outwards, falls in the interval of the two eminences and of the corresponding portion of the ventricles upon the posterior margin of the corpus callosum.

There exists on a level with the squamous portion of the temporal bone a depression in the interior of the cranium and a corresponding eminence, almost as considerable, on the outside. If we cut through this eminence and the contiguous part of the brain at the same time, we open the base of the temporal region of the ventricle.

Now as there is nothing on the surface of the brain to account for the cranial prominences of which I have just spoken, it appears to me that we may very fairly consider them as caused by the shape of the corresponding regions of the ventricles. This conclusion is strengthened by comparing the form of these eminences with that of the portions of the ventricles corresponding to them. The frontal eminences are round, like the two cul-de-sacs forming the anterior extremities of the ventricles. The occipital protuberances, and especially the depressions answering to them in the interior of the skull, are sharper, if this language is applicable to protuberances and depressions; and the greater acuteness of the posterior extremities of the ventricles in relation with these protuberances, is a fact sufficiently notorious. Lastly, the temporal eminences are oblique in the same direction as the corresponding part of the hollow of the ventricles. But the influence of the ventricular cavities, or of the serous sacs of the brain, is not confined to the formation of the different pairs of eminences which I have just pointed out;it extends also to the form of the skull in general, which, in fact, it contributes to determine.

The different transverse sections, which I have supposed to be made on a level with the centre of all these pairs of eminences on the median zone of the cranial vault, divide this median zone into four regions, always perceptible during life, and each of them presenting an angular curve on the transverse line which separates it from the neighbouring regions. All of them display too an agreement in form and proportions with the corresponding region of the envelope of the ventricles, which seems to me incontestable.

The first region, comprised between the frontal eminences and the inferior boundary of the forehead, answers exclusively to the convolutions developed in front of and beneath the corpus callosum, and presents but a small projection from above downwards, like the corresponding part of the ventricles. The lower projection on this part of the forehead does not always indicate a considerable development of the corresponding part of the brain. It may, in fact, be simply owing to the great size of the frontal eminences, and in this case the fact may be ascertained by percussion.

The second region, included between the frontal and parietal eminences, always forms the largest division of the median zone of the cranial vault; it is arched like the corpus callosum itself, and corresponds to the convolutions above this body. Its size, compared with that of the other regions of the bony arch, bears the same proportion to theirs, that the extent of the superior part of the corpus callosum does to the other fibrous parts enveloping the serous cavities of the brain.

A considerable eminence is very often seen on the median line towards the centre of this region, in the upper part of the os frontis. This also appears to me, like all the other projections on the median line, to be owing to a thickening of the bones.

The third region, intermediate between the transverse section of the parietal eminences and the superior occipital protuberances, is scarcely ever convex longitudinally. It is mostly straight or concave in this direction. Frequently even it presents on the median line a well-marked fossa. The complete separation for the two hemispheres at this part, the absence of the corpus callosum, lifted up as in the others by the fluid of the ventricles, and the superior concavity observed in passing from those parts of the ventricles covered by the corpus callosum, to those formed by a distinct fibrous cone in each posterior half of the hemispheres, are in accordance with these peculiarities. To this region correspond the convolutions situated behind the posterior margin of the corpus callosum as far as the posterior termination of the ventricles.

Lastly, the fourth region, situated in the interval between the superior occipital protuberances and the upper curved line of the occiput, divides the latter into a median convex quadrangle, and two lateral triangles, whose pointed summits terminate near the mastoid process. Now these triangles, instead of being convex like the rest of the median arch, almost always present a plain or even concave surface. The depressed portions of these triangles correspond at their sharp summits to the insertion of the tentorium cerebelli and the external part of the lambdoidal suture.

In the temporal regions of the cranial arch, we always remark, on a level with the great wing of the sphenoid bone, a depression running upwards and backwards in the same direction as the fissure of Sylvius, to which it corresponds. A right line drawn from the top of this depression towards the centre of the parietal eminence, marks the course of the fissure of Sylvius, and allows us to measure, on the living subject, the comparative volume of cerebral substance situated in front, and of that behind, this fissure.

Now the fissure of Sylvius, and the cranial depression answering to it at the fore part of the temporal fossa, are variously modified according to the modifications of the ventricular hollow. The anterior and temporal regions of these cavities are separated by a large nervous mass which follows the crus cerebri. The fore part of the ventricle is enlarged above this mass and the temporal part of the ventricle below it.

The fissure of Sylvius, then, forms the interval between two regions of the ventricle, the frontal and the temporal, and consequently the interval between the convolutions covering in these two distinct regions of the serous cavity. It ceases above, at the part where these regions of the ventricular cavity unite into a common conflux. Its deepest part is below, at the bottom of the widest interval between the frontal and temporal extremities of the ventricle.

Thus the form of the brain, and that of the skull also, would seem to be determined in their general character by the form of the serous sacs inclosed in the hemispheres, and constantly filled with the fluid peculiar to them.

This is not meant to imply that the cerebral convolutions do not exert any influence on the secondary shape of the skull. So far from that, observation proves that when the convolutions are greatly developed, the skull, though retaining the form that we have assigned to it, swells out in the intervening regions between the eminences corresponding to the extremities of the ventricles, so as almost to obliterate them; or if they still remain very prominent, they acquire a very great diameter. In this case, the head, modified by the development of the convolutions, acquires the cerebral form, *par excellence.*

On the contrary, when the convolutions are but very little developed, the prominences, and the regions separating them, are all exceedingly well marked, whilst, at the same time the projections corresponding to the ventricular extremities have a very small diameter. The head then assumes, if I may be allowed to use such a term, the peculiarly ventricular form.

The relations just now pointed out between the projecting points of the skull and the corresponding regions of the ventricles, appear still further confirmed by observing what takes place in chronic hydrocephalus. One of the first symptoms of this dropsy of the ventricles is the increased salience of these cranial tumours. But I shall not pursue the details of the relations between the skull and the brain any further at present. I consider them useful in reference to what is called surgical anatomy, and also for all those cases which call for an exact knowledge of the relations between the skull and the brain. The subject has been pursued in a work of some extent, which I intend before long to lay before the public.

To sum up, I consider that the fibrous parts of the brain are conductors; some from without to within, others from within to without. I believe that these conducting parts may be distinguished into afferentes and efferentes, and that the distinct course of both the one and the other may be demonstrated. The first are inserted especially into the circumference of the gray substance, and the second into its internal surface.

The gray substance of the convolutions intermediate between the two preceding orders of fibrous parts, seems to me to be the material substratum, through the instrumentality of which the will directs the movements of the body.

The prominences constantly seen coupled in pairs on the arch of the skull appear to me to be produced by the projection of the corresponding regions of the ventricles. The median eminences, not universally present, appear to be produced by a thickening of the bones.

The median zone of the cranial arch is naturally divided into four sections; one, anterior, corresponding to the fore part of the corpus callosum, and to the convolutions developed before and beneath the level of the same part of that body.

The second, more extensive, intermediate between the frontal and parietal eminences, is of a length proportioned to the extent from before to behind of the corpus callosum. It ceases behind on a level with the posterior margin of that body.

The third section, often concave from above to below, sometimes even hollowed into a furrow on the median line, is proportionate in length to that of that part of the hemispheres completely separated, behind the corpus callosum.

The fourth section, intermediate between the superior occipital protuberances and the upper curved line of the os occipitis, displays in its middle a projecting quadrangle, corresponding to the hinder extremities of the ventricles, and to the convolutions situated behind and beneath these extremities *.

March 30, 1840.

V. *Remarks on the permanent Soap Film and on Thin Plates.*

By JOSEPH READE, *M.D.*

> "Scilicet ut possem curvo dignoscere rectum,
> Atque inter silvas Academi quærere verum."—HORACE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS the entire of the second book of Newton's Optics is based on the theory first advanced by Dr. Hook, or rather by Mr. Boyle, "that colours are produced by the thicknesses of the plates," supposing your scientific readers well acquainted with that great philosopher's experiments, I shall immediately proceed with my own. Should these be capable of explanation according to the Newtonian theory, I shall be ever ready to alter my opinions, and what at present I conceive to be legitimate inferences. Aware of the difficulties I have to encounter, and of the prejudices against everything opposed to a doctrine stamped with the name of Newton, and advocated by the most celebrated characters, I hope for the indulgence of my scientific readers.

Experiment 1. Having made a permanent soap film, as already described in your Journal †, and shown two years ago at the British Association at Liverpool, I placed the bottle on an inclined plane on the table until all the bands of colours had evaporated and the entire film become of a black appearance, what Newton likened to a hole in his bubble, transmitting almost the entire light. I now moved the bottle to and from me, and in a short time the entire film was clothed with silvery white, reflecting atoms, which soon formed into beautiful bands of colours by means of cohesive attraction; for, as they formed nearly simultaneously, they could not be produced by a descending fluid, causing relative thicknesses; but perhaps it may be said, that the film was thickened by evaporation of the saponaceous fluid. To obviate this objection, I made the following new, and I presume to hope, conclusive experiment.

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* An able report on the subject of Dr. Foville's researches has recently been presented to the Academy of Sciences by Professor Blainville. It contains further discoveries made by the Doctor with regard to the origin of the eighth pair of nerves.

† [Lond. and Ed. Phil. Mag. vol. xi. p. 375.—EDIT.]

Experiment 2. Having procured a cylindrical glass tube, about one inch in diameter, I dipped one end to the depth of three inches into a saponaceous solution and formed a film, which, when the tube was held perpendicularly, glided down three inches. I now corked the upper orifice, to prevent further descent, and laying the tube on the table, in a short time the bands were formed; when these disappeared, and the film was black, I shook the tube from side to side, and in a short time the black changed to a silvery white, and then formed into bands of different colours. On placing it again on the table the film appeared all over black. Here there could be no thickening of the plates by evaporation, as the air was at the bottom of the tube. This experiment, easy of manipulation, I hope may be esteemed conclusive, or, as Sir Isaac Newton calls it, an "*experimentum crucis.*" As my experiments are now shown in lecture-rooms, I am anxious that they should be accompanied by legitimate inferences, particularly as some observations made by a Dublin Professor at Liverpool, prevented an explanation which I now give.

Experiment 3. Having procured a plate of very deep blue glass, four inches square, I wiped it well, and then breathed on it through a narrow glass tube, forming a plate of vapour, which, by evaporation, went through all the relative thicknesses measured by Newton, without any variety of colours. I now breathed a second time on this plate, and drew my finger across the middle so as to make furrows in the plate of vapour; immediately all the variety of colours in nature was formed, like threads of variegated silk. This experiment evidently shows that the atoms were relatively approximated, and that it was this approximation, and not any relative thickness of the plate of vapour, which caused the colours.

Experiment 4. On repeating the experiments of the Abbé Mazéas, "*Mémoires présentes,*" with two pieces of plane glass, he justly remarks, that friction is necessary in the formation of coloured rings. Now it is evident that friction cannot alter the thickness of a plate of air, but must bring into view some substances capable of condensation; and there is nothing mixed with the air capable of such condensation, except vapour. I therefore must attribute the colours to this cause. Convinced that vapour was the cause, I improved on the Abbé Mazéas's experiment: for, on washing one of the plates of glass with soap and water, and holding it before the fire, then breathing on the other, by means of a slight degree of friction I coloured the entire glass, with a large black spot in the centre more than an inch in diameter. When this black spot was formed, the glasses were so firmly united as to require a strong force to separate them. This I attribute to the air being pressed out, and then the glasses acted in the same manner as a leather soaker used by school-boys, and not to any cohesive attraction of the surfaces, as supposed by chemists. I coated the glasses with a plate of water, and on pressing them together no colours appeared, until by a strong degree of friction I produced vapour. I also smeared the glasses with some candle-grease, and found no colours, until I held the glasses to the fire, when, on using friction, vapour and colours were produced.

Atomic Theory of Colours.

Having endeavoured to prove, I hope with effect, that colours are not produced by relative thicknesses of the plate, as advanced by Sir Isaac Newton in the second book of his Optics, I shall proceed to give what I conceive is the true explanation of this interesting phænomenon. Grimaldi and others maintained that light was capable of condensation and rarefaction. However, as they brought forward no experiments to prove it, I think it unfair to say that I took my theory from that celebrated philosopher. If we hold a candle before a black shade made with a pencil or any other slender and opake substance, and hold the paper sideways to the window, two shadows are formed, the one blue from the candle, the other brown from the daylight. Now this brown shadow can be changed to an orange by approximating the candle; a ta yet nearer distance the orange becomes a perfect yellow, and when very close the colour entirely disappears, or the light of the shadow becomes rarefied into perfect transparency. The blue in like manner undergoes rarefaction and change of colour, from a blue to a purple; and when the candle is very near the coil of paper, the shadow becomes black, because then there is but one light, that of the sun. To argue that the shadow is a mere privation, would be to say that brown, orange and yellow, blue and purple were privations:

(green was made by overlapping the blue and yellow shadows.) Surely, if it were a mere privation, the light of the candle could only illuminate the black shadow and make it white. However, the fact is, that the black shadow is condensed light, and rarefied into different colours. If we suppose a number of grains of shot to represent the soap atoms, placed at different distances from one another, and represented by a, a^2, a^3, a^4, &c., &c., the light passes through those and is reflected from the second surface, and gives, when variously condensed, this or that colour, according to the approximation of these atoms, and not the thickness of the plates*.

Ascending and Descending Currents in a Soap Film.

On making a soap film I placed it on an inclined plane, and perceived the coloured bands to descend slowly, and increasing in breadth, until at last the attraction of cohesion lost its influence, and the atoms became free and ascended in currents, particularly at the concave sides of the bottle; as they rose generally white atoms, they passed through the coloured bands, until after passing through blue, red, green, &c. they fell into the ranks of their own colour. In boiling water we perceive currents by means of powdered resin, or other light substances, evidently caused by an addition of caloric. Here there is no such addition, and we must look to some other cause, perhaps electricity. Sometimes these atoms take an elliptical or circular motion; this is best seen by placing the bottle with the plane film on the table and surrounding it with the warm hand; as the room was at 60° and my hand at 80°, I threw 20° into the film. Here there can be no differences of thickness; however, almost simultaneously, the force of cohesive attraction forms the coloured bands, and when the film becomes white, then these beautiful and interesting movements take place. How far these laws may act on the solar system, I leave to the contemplation of the astronomer. All fluids are in perpetual motion, from the broad Atlantic to the permanent soap film; and hereafter I shall be enabled, by some new experiments, to show that the same laws regulate the atmosphere.

Colours of the Clouds and Complementary Colours.

When the soap film was entirely black, after remaining perhaps an hour on the table, I placed the bottle in a basin of boiling water, and in a short time perceived the film to be clothed with white reflecting atoms; some of these soon changed to a purple, blue, orange, &c., and when the bottle was placed on an inclined plane the coloured bands were formed, broad and vivid. To ascertain whether the bands of colour were similar by transmission and reflection, I held the film above a lighted candle, at an angle of about 45°, and perceived exactly the same coloured bands, and not any complementary colours, "magnis componere parva." We must attribute the colours of the clouds to the approximation of vaporific atoms, and not to different-sized globules of water. It is an interesting object of inquiry, to say why the black reflecting atoms at great distances, should by heat or agitation, approximate to form white or bands of colour. We may, hypothetically, surmise that these atoms are spherical, and are enlarged by caloric, in the same manner as air in a balloon or bladder; or we may suppose that, by agitation, &c., they become oblate spheroids.

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* Count Rumford, as well as others, made many experiments on coloured shadows, but entirely overlooked these changes; at last, in despair, he says, it was a *deceptio oculi.*

Nothing is more surprising than the permanency of this soap film. In a few seconds Newton's bubble bursts, and consequently hindered him from making deliberate observations; but now that we have one which lasts for months, can be washed and renewed, an inferior mind may investigate the phænomena of light and colours with more success than even that great philosopher, in the infancy of experimental science. Indeed, every new experiment opens a wider field for research, and I am sanguine enough to think that the permanent soap film and the newly invented iriscope may produce interesting discoveries. With intent to ascertain this permanency, I formed two films with a similar saponaceous mixture, in equally-sized bottles; the one was corked, the other open, and consequently exposed to the action of the atmosphere; in a short time the open one broke, the other remained for weeks. I therefore inferred that the film broke from the vaporific action of the atmosphere. That it was not from pressure, I proved, by putting in a very long cork, so that the pressure might be increased; nor was it from agitation, as supposed by Newton, for in washing the film it was well shaken; neither could it proceed from any chemical action, as I made the film *in vacuo*. I remarked that a film formed on the mouth of a wine glass remained a long time when the atmosphere was heavy and charged with vapour, whereas of a dry day it speedily burst. I hence infer that the air in a corked bottle becomes saturated with vapour, and thus that evaporation is diminished. I shall now sum up the foregoing arguments, to show that the colours do not proceed from relative thicknesses.

1. The bands form simultaneously, or nearly so.

2. Plates of grease, water, vapour, go through all the thicknesses without colours; but as soon as friction forms vapour, or the finger approximates the atoms, colours appear.

3. By holding the bottle to the fire, or even by the heat of the hand, circular currents of red, white, blue, rise in a green field; and, at all times, coloured atoms are both ascending and descending, contrary to gravity and thickness, the bands increase in breadth as they descend.

4. To end. I have succeeded in making a film sufficiently thick, with soap, as to counteract cohesive attraction, yet sufficiently thin to evaporate without undergoing any change of colour.

The following diagrams give the different stages of the atomic theory of colours.

Fig. 1.

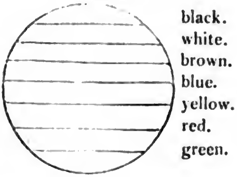


Fig. 2.

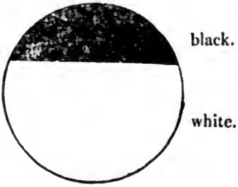


Fig. 3.

In fig. 2, how can different thicknesses produce three-fourths of the film white? In fig. 3 there is a very small segment of white at the bottom, and by shaking the bottle the entire film becomes white, and then forms into chromatic bands. The best method of seeing these minute saponaceous atoms forming colours, is to shake fig. 2, when atoms no larger than the point of a pin are diffused in the black, and after a time are seemingly dissolved in the black atoms, just as when a lump of white sugar is dissolved in hot water; at first the solution is clouded, and then the saccharine atoms become as minute as those of the water, and equally semi-transparent. A candle held before fig. 3 is faintly reflected from the second surface, as there is no actual contact of atoms in nature; all substances, even the hardest metals, are porous.

In my next communication I shall give a theory of the Iriscope, compared with Nobili's rings.

I remain, Gentlemen,
Your obedient servant,
London, June 8, 1840. J. READE, M.D.

VI. *On the Form and Optical Constants of Nitre.* *By Professor* MILLER[*].

THE following values of the angles between normals to the several faces of nitrate of potash are calculated from means of the best measurements of a large number of crystals at a temperature of about 19° centigrade. The close agreement of the results afforded by different crystals, renders it probable that the errors of the concluded angles do not in any case exeeed half a minute.

Fig. 1.

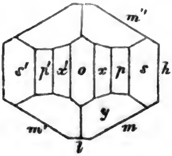
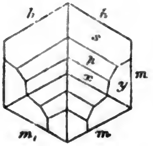


Fig. 2.



$l\ o$	90° 0′	$p\ p'$	70° 3′·5
$o\ h$	90 0	$s\ s'$	109 0
$h\ l$	90 0	$h\ y$	65 41
$m\ m'$	61 10	$l\ y$	45 50
$x\ x'$	38 38	$o\ y$	54 1·5

In twin crystals having the twin axis perpendicular to one of the faces m (fig. 2).

$m\ m_{,}$ 57° 40′ $h\ h_{,}$ 61° 10′.

The parameters are respectively proportional to the numbers 2·4285; 1·4352; 1·7023.

The cleavage most easily obtained is parallel to the faces p.

When the temperature of the crystal is increased 100° C.

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[*] Communicated by the Author.

the angle between normals to the faces $p\ p'$ increases about 44′: $m\ m'$ does not perceptibly change.

The symbols of the simple forms are

$$h\ \{100\},\quad l\ \{010\},\quad o\ \{001\},\quad y\ \{111\},$$
$$p\ \{101\},\quad s\ \{201\},\quad x\ \{102\},\quad m\ \{110\}.$$

No other forms were observed.

The optic axes lie in a plane parallel to the face *l*, and, in air, appear to make with each other an angle of 8° 40′. The minimum deviations of the brightest rays of the spectrum refracted through the faces m, m'' are 24° 15′ and 38° 49′, for light polarized in planes respectively perpendicular and parallel to the intersection of the faces m, m''. Hence the velocity of the brightest rays of the spectrum in air divided by the velocities within the crystal, will be 1·5052, 1·5046, and 1·333, for rays in planes parallel to *h*, *l*, *o*, polarized in those places respectively.

St. John's College, Cambridge, June 4th, 1840.

VII. *Observations on* Mr. Smith's *Experiments on Fermentation.* *By* R. Rigg, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE reading of Mr. Denham Smith's paper printed in the Number of your valuable Periodical for March, entitled, "Observations on the supposed Formation of Inorganic Elements during Fermentation," induces me to ask you the favour of saying, through the same publication, that in the abstract of my paper on that subject, which is noticed by that gentleman, the increase in the quantity of earthy and alkaline matter therein mentioned, does not apply to the original quantity contained in both the refined sugar and the yeast employed in my experiments, but to that in the former only. The latter previous to using being deprived of its soluble matter by repeated washings in cold water, loses very little, if any, of its solid materials when employed in promoting the vinous fermentation.

It may be added that the circumstances which I have found most favourable for the formation of these bodies during this chemical action on refined sugar, are when the sugar at the commencement forms about 20 per cent. of the weight of the mixture, and when about nine-tenths of this dissolved body has undergone decomposition by a quick process, the experiments being kept under slight pressure and at an equable temperature of near 70° Fahr.

Mixtures of this kind conducted through the vinous fermentation in silver and china vessels always afforded a larger quantity of ashes after evaporation and calcination than those made in glass apparatus. But upon the proportionate increase by this process in any experiment, we can speak at the outset with as little certainty as upon the height to which any plant will grow when the seed is first planted. One of the features which I have always found to accompany their formation is an increase in colour, which colour again disappears when they undergo decomposition by the putrefactive fermentation and have ammonia as a product.

I have found the quantity to vary with almost every circumstance, even in experiments made with the same mixture, from little or no increase to that of twenty times the weight of those included in the sugar experimented upon. My experiments made with solutions containing about ten per cent. of their weight of sugar (the strength employed by Mr. Smith) rarely gave more than an appreciable increase upon the original quantity, except when the fermentation was conducted very quickly.

The ashes were obtained by evaporating and burning over a spirit-lamp without the addition of any foreign agent, the materials being in an open platinum vessel. Their quantity was determined by weight as a whole, with the addition of their power of neutralizing acids as regards their alkaline property.

Walworth Road, April 14th, 1840.

VIII. *On Increasing the Light of a common Argand Lamp.*
By C. Holthouse, *Esq.*[*]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING seen in the Number of your Magazine of March last, a letter from Sir J. Herschel, "On a simple mode of obtaining from a common Argand Oil Lamp a greatly increased quantity of Light," I take the liberty of sending you a few observations of my own on the same subject; they are mostly corroborative of what has been advanced by the distinguished philosopher before alluded to, and if you think them worth insertion they are very much at your service.

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[*] Communicated by the Author.

The size and brilliancy of the flame in a common Argand oil lamp depend chiefly upon the shape and dimensions of the glass chimney, and its position in relation to the flame; but as the contrivances which render the latter more brilliant at the same time diminish its bulk, there is a limit, beyond which we cannot increase the brightness without diminishing the illuminating power; the increase in brilliancy not compensating for the diminished bulk of the flame. Sir J. Herschel has not told us what is the diameter of his Argand burner, or the diameter and form of his glass chimney; but from the description he has given us of his method of producing a greatly increased quantity of light, I presume he must have used one of those chimneys which are commonly employed only for gas lamps, being a simple cylinder about nine inches long, and of equal diameter throughout. But this kind of chimney is ill adapted for enlivening a flame, and seems merely to serve for protecting it against currents of air, although, when employed in the manner recommended by Sir J. Herschel, its office is reversed; it becomes a mean of increasing the intensity of combustion, but serves no longer as an efficient protector: and in this latter respect it is imperfect; for admitting the advantages which arise from the increase of light, still these can only be available while a lamp is at rest: the instant it is moved the flame must necessarily impinge against the walls of the chimney, at the great risk of breaking it, and with a certainty of smoking it. But an equal degree of light, and not subject to the inconvenience just mentioned, can be obtained by one of the chimneys ordinarily made use of in the common table lamp, which consists of a base or larger cylinder, upon which the pillar or smaller cylinder is joined by a horizontal part, termed the shoulder: upon the height of the pillar of this chimney, the proportion of its constituent parts to each other and to the burner, and their position in relation to the latter, depend the efficient burning of the lamp.

And first with reference to the *height.* The most obvious effect of lengthening a chimney is, to render the flame more flickering and unsteady, at the same time its brightness is slightly increased, and its bulk diminished: the larger the diameter of the chimney compared with the burner, the less marked are these changes. On lengthening a chimney whose pillar had a diameter of 1⅛ inch, to three feet, and placing it on a lighted lamp, well turned on, whose wick had ⅞ of an inch diameter, the flame immediately split into several small flickering cones, the largest of which was not above five or six lines in height; as the chimney was shortened, so did the flame in the same proportion become steadier and more uniform, till at seven inches, which was the length of the pillar of this chimney, it was as steady as could be desired. One fact was strikingly obvious during this experiment, viz., that the increase of brilliancy was by no means commensurate with the loss of light occasioned by the smaller bulk and great unsteadiness of the flame. From several trials with chimneys of various lengths, I should say that a seven inch pillar, for an Argand burner of the ordinary size, is well adapted for giving brightness, and at the same time steadiness, to a flame.

To ascertain the best *diameter* for the chimney, eight pieces of tin were procured, each two inches square, and having a circular aperture in their centre, varying from one and a half to half an inch in diameter; these, by turns, were fixed to the moveable stage of a microscope, and then placed directly over the flame of a lamp, so that the centre of the circle of the Argand burner should correspond with the centre of the circular aperture in the tin; a simple* glass chimney being now placed upon the tin, the whole apparatus was lowered till the flame of the lamp passed through the aperture in the tin. By trying the different sizes one after the other, we were able to decide upon that which gave the greatest degree of illumination, and by raising and lowering the apparatus over the flame, the best height from the level of the wick for obtaining the greatest light was determined. The following are some of the results. The size of the flame was in a direct ratio with the size of the aperture in the tin, but its brilliancy was in an inverse ratio; in other words, the flame diminished in size but increased in brilliancy as the aperture through which it had to pass was lessened. The effect produced by chimneys of different diameters, but of equal length, provided the aperture in the tin over which they were placed remained the same, was so trifling, that it was scarcely appreciable: the diameter of the aperture in the tin being, for instance, one inch, and that of the chimney the same, no material alteration in the brilliancy or bulk of the flame was produced when a chimney of double the diameter was substituted for it. I need hardly observe, that the diameter of the chimney must not be less than that of the tin. I said at the commencement of this letter, that there was a limit beyond which we could not increase the brightness of the flame without diminishing its illuminating power. I am not at present prepared to say where this precise point is; but I can assert, without fear of contradiction, that an aperture of one and one-eighth of an inch diameter, for a lamp whose wick is seven-eighths of an inch, is a size well calculated for giving a great degree of brilliancy to the flame, without materially diminishing its bulk. We come now to the consideration of the best height for placing the plate of tin above the wick of the lamp; this appears to be from two-eighths to three-eighths of an inch, which is that specified by Sir J. Herschel, supposing his burner to have an inch diameter: when we go below this the flame becomes brighter, less coniform, and shorter; the continuity of its circle is interrupted; chasms form in it, producing the appearance of so many distinct flames; and when the tin has reached the level of the wick, the lamp is well nigh extinguished. Sir J. Herschel, in describing the effect produced on the flame by raising and lowering the chimney, has suggested that it would be an improvement were our common lamps provided with a mechanism for this purpose; this has actually been done on the continent, and in this country by Mr. Samuel Parker of Piccadilly, the flame of whose hot-oil lamp is regulated entirely by raising and lowering the chimney, the wick remaining stationary. Now if a number of glass chimneys be made, the diameter of whose pillars shall be the same as those of the apertures in the tin, and these be suspended over the flame with their narrow end downwards, we shall have the same result: but if, instead of this, they be placed in the usual manner on their support, the large end lowermost, we shall still have precisely the same result, provided the commencement of the pillar bear the same position, in reference to the wick, as the upper end did when suspended over it. The diameter of one inch and an eighth, which I have named as a good size for having the pillar of the chimney, is eligible likewise on another account: there is less danger of its becoming smoked, to which it would be very liable were it of smaller dimensions than that just specified. The base of the chimney may be an inch and three-quarters or two inches in diameter, and its shoulder should form a right angle with the level of the burner, about half an inch above it. This is a form of chimney which I have proved to be well fitted for giving a degree of light very superior to what is seen in Argand lamps having the ordinary sized chimneys. Dr. Ure, in a very interesting paper read before the Institution of Civil Engineers in June last, seems to give the preference to chimneys with rounded shoulders; these certainly give greater steadiness to the flame, and if the light is equal to that afforded by the rectangular shouldered ones, they should be preferred.

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* When I apply the term *simple* to a chimney, I wish to be understood as speaking of those cylinders of uniform diameter from end to end; those composed of two parts, I shall, to prevent unnecessary tautology, designate *compound*.

One more point, which was barely touched upon by Sir J. Herschel, yet remains to be decided: Is this superior splendour of the flame attended by an increased consumption of oil? If we reasoned from analogy, we should certainly reply in the affirmative; intensity of combustion being ever attended with corresponding consumption of the combustible material. But to place this beyond a doubt, the following experiment was undertaken. Two Argand lamps, having wicks and chimneys of the same diameter and length, and furnished with the same kind of oil, were placed in a pair of scales, each at an equal distance and elevation from the centre of a sheet of white paper pasted on the wall. On being lighted, the flames were regulated till the shadows cast by a small ruler had an equal depth; when this had been fairly determined both by myself and others, weights were put into the stand containing the lighter lamp, till the two exactly balanced each other. The chimney was now quickly removed from one of the lamps, and another substituted for giving a clear white flame, the time being at the same moment noted: they were allowed to burn for forty-two minutes, and were then simultaneously extinguished. At the expiration of this time, the lamp burning with the bright flame had consumed 100 grains more than the other. The experiment being repeated, but with smaller flames, the increased consumption of oil in the bright burning lamp was 50 grains. Whether this greater expenditure of oil is balanced by the increased degree of illumination, and the solution of some other interesting questions connected with this subject, I shall reserve till a future period.

I am, Gentlemen,
Your obedient Servant,
C. HOLTHOUSE.

13, Keppel Street, Russell Square,
April 11, 1840.

IX. *A Letter to* Prof. Faraday, *on certain Theoretical Opinions. By* R. HARE, *M.D., Professor of Chemistry in the University of Pennsylvania*[*].

DEAR SIR,

1. I HAVE been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

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[*] From Silliman's American Journal of Science and Arts, Vol. 38, No. I. [We have taken the liberty of numbering the paragraphs of Dr. Hare's letter.—ED.]

2. You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I entertain with diffidence any opinion in opposition to yours. I may say of you as in a former instance of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candour and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

3. I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1165. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "*sensible distances.*" Agreeably to the former, you conceive that "assuming that a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the inner superficies of the bounding sphere."

4. Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible* distances," how can a particle situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?

5. How can the force thus exercised obey the "well-known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

6. Can the ponderable particles of a gas be deemed contiguous in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this af-

fection will be inversely as their number as alleged by you. No such law holds good in the communication of heat. The air in contact with a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

7. As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

8. If induction, in acting through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dielectrics," be ascribed to the modifying influence which they exert?

9. If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?

10. As the theory which you have proposed, gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phænomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power can be created?

11. When by friction the surface is polarized so that particles are brought into a state of constraint from which they

endeavour to return to their natural state, if nothing be superadded to them, it must be supposed that they have poles capable of existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized; while in the other position, they are more remote, and consequently capable of acting upon other matter.

12. But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be suggested that the change of polarity produced in particles by electrical inductions, may arise from the forced approximation of reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation, may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles? I am aware that, agreeably to the ingenious hypothesis of Mossotti*, repulsion is an inherent property of the particles which we call ponderable; but then he assumes the existence of an imponderable fluid to account for cohesion; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phænomena of electricity to the polarization of ponderable particles, unless it can be shown, that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

13. If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines; so that alternately opposite poles are presented in such a manner that of all one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity

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* [See Scientific Memoirs, vol. i., p. 448.—Edit.]

requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege that agreeably to your views it is impossible to charge a portion of matter with one electric force without the other. (See par. 1177.) But if all this be true, how can there be a "positively excited particle?" (See par. 1616.) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

14. I cannot agree with you in the idea, that consistently with the theory which ascribes the phænomena of electricity to one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phænomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavour to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shown by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed *.

15. But for any current it is no less necessary that the pressure should be on one side, comparatively minus, than that on the other side it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before, or from pressure behind. One current cannot differ essentially from another, however they may be produced.

16. In paragraph 1330, I have been struck with the following query, " What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest, between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

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* See my Essay on the causes of the diversity in the length of the sparks, erroneously distinguished as positive and negative, in vol. v. American Philosophical Transactions.

17. In any case of the intermixture of opposite qualities, may it not be said in the language which you employ, "the moment we leave the element of perfection at one extremity, we involve the element of perfection at the opposite"? Might it not be said of light and darkness, or of opakeness and translucency? in which case, to resort to your language again, it might be added, "especially as we have not in nature, a case of perfection at one extremity or the other." But if there be not in nature any two bodies, of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

18. Have you ever known electricity to pass through a pane of sound glass? My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

19. You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity, why does it not pass through it without a fracture or perforation?

20. According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "*convection.*" The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

21. In paragraph 1300, I observe the following statement: "*When a Leyden jar is charged, the particles of the glass are forced into this polarized and constrained condition by the*

electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction." As you have not previously mentioned any particular direction in which the forces are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

22. The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction*," it would be better to substitute the following, "*to compensate each other by an adequate communication.*"

23. Agreeably to the explanation of the phænomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

24. I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of rows of *contiguous ponderable particles.*

25. Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted to some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first-mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of depolarization?

26. Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

27. From a note subjoined to page 37 of your pamphlet, it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phænomena, it has not been your intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

28. It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretical inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the mass of mankind entertain with more firmness than that of the existence of matter in that ponderable form, in which it is instinctively recognised by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favour the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because

we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

29. Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency is capable of producing the phænomena of mechanical electricity, it has appeared to me inconceivable that the phænomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated. If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

30. Having devised so many ingenious experiments tending to show that the received ideas of electrical induction are inadequate to explain the phænomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satisfactorily explained by ascribing them to ponderable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

31. But if galvanic phænomena be due to ponderable (*imponderable?*) matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water, or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined? In such cases may not the material cause of electricity be considered as latent, agreeably to the suggestion of Œrsted, the founder of electro-magnetism? It is in combination with matter, and only capable of producing the appro-

priate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

32. If polarization in any form can be conceived to admit of the requisite gradations of intensity, which the phænomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms.

33. When the electric particles within different wires are polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phænomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotary influence.

34. Between a wire which is the mean of a galvanic discharge and another not making a part of a circuit, the electric matter which intervenes may, by undergoing a polarization, become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other words an electrical current of the species called Faradian may be generated.

35. By progressive polarization in a wire, may not stationary polarization or magnetism be created; and reciprocally by magnetic polarity may not progressive polarization be excited?

36. Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

37. There are it would seem many indications in favour of the idea that electric excitement may be due to a forced polarity, but in endeavouring to define the state thus designated, or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of

any hypothesis which I could imagine. How are we to explain the insensibility of a gold-leaf electroscope, to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

38. Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles, or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analogous to that which determines the capacity for heat, and the different influence of dielectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,

ROBERT HARE.

An Answer to Dr. Hare's *Letter on certain Theoretical Opinions. By* M. FARADAY.

MY DEAR SIR,

i. YOUR kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgement. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

ii. My theory of induction (as set forth in Series xi. xii. and xiii.) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject

(1667). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phænomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

iii. Bodies whether conductors or non-conductors can be *charged*. The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or which the prime conductor of a machine acquires when the latter is in action; at other times it means the state of a Leyden jar or similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

iv. Let three insulated metallic spheres A, B and C be placed in a line, and not in contact; let A be electrified positively, and then C uninsulated; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory: A will be charged positively; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it; and C will be charged negatively.

v. The ball B will be in what is often called a polarized condition, i. e. opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (iii.), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (iv.) is concerned.

vi. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly; B assumes a polar condition; and C is charged inducteously (1483.), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

vii. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (v.)) does (1672.). The word charge as applied to a Leyden jar, or to the *whole* of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

viii. As another expression of my theory, I will put the following case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (v.) as regards its inner and outer surfaces.

ix. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

x. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

xi. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external sphere C as before, and positive force equal to 100 will appear on its outer surface.

xii. Such a state of the space between the inductric and inducteous surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

xiii. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative electricities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

xiv. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductric influence of A would be conveyed outwards to the inner surface of the sphere C, though that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

xv. The actions of the two portions of space, as they have just been supposed to be constituted (xiv.), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

xvi. In my papers I speak of all induction as being dependent on the action of contiguous particles, i. e. I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductric body acts upon conductors at a distance, it does so by polarizing (1298. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it: first by saying at par. 1615, "the next existing particle being considered as the contiguous one;" then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my Researches, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

xvii. Finally, my reasons for adopting the molecular theory of induction were the phænomena of electrolytic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.), of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

xviii. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develope principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators who can decide which is in the right.

xix. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page*. In referring to my own matter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

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* We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.

xx. At paragraph 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that should be half an inch off. With the meaning which I have carefully attached to the word contiguous (xvi.) I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

xxi. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shown that the principle of communication is not the same in both.

xxii. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

xxiii. Paragraph 4 is answered by the above remarks and by viii. ix. x.

xxiv. Paragraph 5 is answered according to my theory by viii. ix. x. xi. xii. and xiii.

xxv. Paragraph 6 is answered, except in the matter of opinion (xviii.), according to my theory by xvi. The conduction of heat referred to in the paragraph itself will, as it appears to me, bear no comparison with the phænomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (viii. ix. x. xi. xii. xiii. xiv.) exactly equal to the exciting or moving force, which is still at the centre.

xxvi. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavour to set myself free. You say I "correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177). But if all this be true, how can there be a *positively excited particle*? (see par. 1616). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well as a positive pole?" Now I have not said exactly what you attribute to me; my words are, "it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177.)." I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

xxvii. Using the word *charge* in its simplest meaning (iii. iv.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of iv., v., vi., vii., fully illustrate my views (1672.).

xxviii. Paragraph 20 includes the question, "is this consistent?" implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding paragraphs. I believe that it is. I have charged small Leyden jars made of thin flint glass tube with electricity, taken out the charging wires, sealed them up hermetically, and after two and three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges*. I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my Researches; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by what I call convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating*; and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

xxix. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inducteous surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the "some other directions" spoken of (1682. 1683.).

xxx. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (iv., v.,) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (x.) or the myriads of polarized conducting particles (xi.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

xxxi. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (xxx.), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass of the jar, or the polarized globe B (v.), to their first state. My view of the relation of insulators and conductors, as bodies of one class, is given at 1320. 1675. &c. of the Researches; but I do not think the particles of the good conductors acquire an intensity of polarization anything like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy: thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

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* *Bibliotheca Italiana*, 1837, lxxxv., p. 417.

xxxii. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), "what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opakeness and translucency? and so forth.

xxxiii. I admit that these questions are very properly put;

not that I go to the full extent of them all, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phænomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in the gravitation of different bodies being considered due to differences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phænomena in question.

xxxiv. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phænomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class,) and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

xxxv. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance: and it is certain whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

xxxvi. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that hot and cold are phænomena of the same kind. As it is of consequence to show that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to show that one body insulates and another conducts only in consequence of a difference in degree of one common property which they both possess; and that in both cases the effects are consistent with my theory of induction.

xxxvii. I now come to what may be considered as queries in your letter which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act on each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (xvii.); and next, that in the arrangement given iv., ix., or x., A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

xxxviii. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687. 1688. 1699. 1700. 1701. 1702. 1703. 1704. in the Researches, would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (v., xxvi.), with the case given (iv., v.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of a conductor under induction (v.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

xxxix. Paragraph 11 involves a mistake of my views. I do not consider bodies which are charged by friction or otherwise, as polarized, or as having their particles polarized (iii., iv., xxvii.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (xxxviii.).

xl. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon the reconsideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgement as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be, with the highest esteem,

My dear Sir,
Your obliged and faithful Servant,
M. FARADAY.

Royal Institution, April 18, 1840.

X. *Notices respecting New Books.*

Report on the Progress of Vegetable Physiology during the Year 1837. *By* F. J. F. MEYEN, *M.D., Professor of Botany in the University of Berlin. Translated from the German, by* WILLIAM FRANCIS, *A.L.S.* London, 1839. 8vo, pp. 158.

OUR readers will doubtless remember the valuable Report on the Progress of Vegetable Physiology for the year 1836, which appeared in our pages about two years since*. The high position occupied by Professor Meyen in this department of science, and the vast increase which is constantly being made in the amount of our knowledge of it, by the labours of the industrious physiologists of Germany, combine to give these reports a peculiar value. A great part of the information contained in them would not have found its way to this country in any other shape; and it is much more agreeable to obtain it in the condensed form it assumes after being submitted to the Professor's *compressorium*,—which squeezes away the lighter fluid with which it is diluted, and retains the solid matter,—than in its original state. The Report at present before us is equally full of valuable information with the former one; and, when its much greater extent is considered, its importance as a contribution to scientific literature will be apparent. The rapid advance of discovery in this most interesting science will give, we are assured, a progressively increasing value to these reports: and when the utility of a well-executed translation is considered, especially from a language which needs long study and familiarity to give a certainty of the author's meaning being understood, we trust that Mr. Francis may receive sufficient encouragement to induce him to persevere in a labour which can, at best, be but scantily remunerated. At present, we regret to learn, he has not cleared the expenses of publication. We would urge those of our readers, therefore, who are interested in the promotion of this department of botanical science (as all who call themselves botanists ought to be), to do what in them lies for the continued success of this undertaking, calculated as it is in the highest degree for their benefit.

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* Lond. and Edinb. Phil. Mag., vol. ix. p. 381, *et seq.*

XI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xvi. p. 148.]

Nov. 6, 1839. A PAPER was afterwards read, "On the relative ages of the tertiary and post-tertiary deposits of the Basin of the Clyde," by James Smith, Esq., of Jordan Hill, F.G.S.; of which an abstract will be found in No. 65 of the Society's Proceedings.

A paper was last read, "On the foul air in the chalk and strata above the chalk near London," by James Mitchell, LL.D., F.G.S.

In the chalk, the most abundant deleterious gas is the carbonic acid, but it has been found to exist in greater quantity in the lower than in the upper portion of the formation, and in that division to be unequally distributed. In sinking wells, it has been noticed to issue with force from one stratum, whilst none has been perceived to be given out from the beds immediately above and below it. Dr. Mitchell mentions fatal effects due to its occurrence in a well near the race-course at Epsom, where it was met with at the depth of 200 feet; and in Norbury Park, near Dorking, at the depth of 400 feet. On Bexley Heath, after sinking through 140 feet of gravel and sand and 30 feet of chalk, it rushed out and extinguished the candles of the workmen; and in making a well in Long Lane, Bexley Heath, after penetrating 124 feet of overlying deposits, and then 90 feet of chalk, considerable inconvenience was felt from it, but 6 feet lower no gas was emitted.

In chalk, sulphuretted hydrogen gas is also occasionally met with, and is supposed to be generated from the decomposition of water and iron pyrites.

In districts in which the chalk is covered with sand and London clay, carburetted hydrogen gas is sometimes emitted, but more frequently sulphuretted hydrogen gas.

Carburetted hydrogen has seldom inflamed in wells, but in making the Thames tunnel it has sometimes issued in such abundance as to explode by the lights and scorch the workmen.

Sulphuretted hydrogen gas is more abundant, and it has been observed almost always to proceed from a coarse black sand charged with oxide of iron, whether the bed be above the blue clay, within it or below it. It has streamed out with great violence in the Thames tunnel, but has in no instance produced fatal effects. At Ash, 3 miles from Farnham, a well was dug in sand to the depth of 36 feet, and one of the workmen on descending into it was instantly suffocated. Fatal effects have also resulted from the accumulation of this gas in wells in Maiden-lane, Battle-bridge, and at Applebury-street, near Cheshunt. This gas is much increased, after long-continued rain, in consequence of the swelling of the clay driving it out of the interstices; and it is diminished after a long drought. The prevalence of a north-east wind has been noticed by well-diggers to diminish the quantity of the gas, but the effect is ascribed by Dr. Mitchell to the dry weather which usually accompanies the wind from that quarter. The author also suggests, that if wells are to be dug in dangerous districts, they should be undertaken when there is least water in the ground, or from the beginning of July to October.

The noxious gas in the Weald of Kent and Sussex is stated to be sulphuretted hydrogen.

November 20, 1839.—A paper "On the origin of the vegetation of our Coal-fields and Wealdens," by J. T. Barber Beaumont, Esq., F.G.S., was next read.

An examination of the fossil trees discovered on the line of the Manchester and Bolton Railway* has confirmed Mr. Beaumont in the opinion, that in no instance has the vegetation of the coal-fields arisen from drifted trees sunk to the bottom of mighty rivers and estuaries, but that it grew where it is found; and he is further of opinion, that the districts composing our present coal-fields were originally islands.

The principal objections of the author to the theory of the transportation of the fossil vegetation are the following:

1. The existence of a mighty river or estuary at the time of the deposition of the coal-measures, would require the existence of a vast continent of which there are no traces.

2. The coal strata near Newcastle are 380 yards in thickness, and consequently, the lowest strata must have been deposited at the bottom of a river or estuary, exceeding in depth, six times the mean depth of the German Ocean.

3. A continent producing such a river, it is reasonable to expect, would have left an abundance of fossils on its surface, as well as at the bottom of its great river; but all the land for hundreds of leagues around the coal and wealden formation swarms with the remains of marine animals, and is clearly an ancient sea bed.

4. In the coal-measures not a bone of a land quadruped is to be found, or a large timber tree, with the exception of a few Coniferæ.

5. In order that the vegetation should have sunk to the bottom of a deep river, it must first have decayed; but the plants preserved in the beds associated with the coal, present a freshness and perfection incompatible with such a condition.

6. Drifted trees are stopped in deltas only from the shallowness of the water being insufficient to float them on; we know of no deposits of trees in deep water.

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* See the Abstract of Mr. Hawkshaw's Paper, Proceedings, vol. iii., p. 139; [or L. & E. Phil. Mag. vol. xv. p. 539.]

The author then offers the following theory as affording a preferable explanation of the origin of coal-fields and Wealden formations.

He conceives they were originally swampy islands, formed out of disjointed fragments which resulted from the first elevation of the rocks; that on these islands grew a luxuriant vegetation consisting of Ferns, Calamites, coniferous trees, &c. which, decaying and regenerating, accumulated in the manner of peat bogs; that the islands, by the settling down of the disturbed crust of the earth, were depressed beneath the surface of the sea, and covered over with drifted sand, clay, and shells, till they were again, by this process, converted into dry land, and clothed with another vegetation; and he is of opinion, that the operation was repeated as many times as there are alternations of coal and sedimentary strata.

A notice on "The Fossil Fishes of the Yorkshire and Lancashire Coal-fields," by W. C. Williamson, Esq., was then read; of which an abstract is given in No. 65 of the Proceedings.

A paper was last read, entitled, "A brief notice of the Geology around the shores of Waterford Haven," by Major Austin.

In this memoir, the author describes topographically the geology of Waterford Haven, commencing at Bag and Bun Head, near its eastern entrance, and proceeding around its shores terminates his account at Ballymacaw. The formations of which the district consists are, 1. limestone; 2. a conglomerate; 3. clay-slate; 4. various trap-rocks; and 5. alluvium.

1. The limestone constitutes the promontory forming the eastern boundary of the Haven from Hook Point to the parallel of Sand Eel Bay, a distance of about 3½ miles. Some of the beds are of a dark colour, and are so fissile as to be used for roofing-slate; they are also highly sonorous when struck with a hammer, and are in consequence locally called the "Black Bell." In the lower part of the formation, in Bryce's Bay, the limestone is yellow. Fossils, including spined Productæ and Crinoideæ, compose the greater part of some of the strata. Near the conglomerate the limestone alternates with thin layers of slate, and at the immediate junction is a stratum of fine red sand containing impressions of numerous fossils. The strata, where undisturbed, dip to the S.S.W. at an angle of about 22°, but they are sometimes considerably contorted.

2. The conglomerate composes a band about half a mile broad, stretching across the promontory from Sand Eel Bay to Herrylock, and is of a deep-chocolate colour. It constitutes Broom Hill, where it assumes partly the character of a compact grit, and partly that of a coarse compound of fragments of schist, quartz, and other pebbles imbedded in a chocolate-coloured cement; the quartz pebbles being traversed by numerous fissures. Other patches of conglomerate occur between King's Bay and Buttermilk Castle, and are said to overlay the edges of highly-inclined strata of clay-slate. It forms also the hills north of the Suire in the county of Kilkenny, where it is worked for millstones, and it composes, with a few interruptions, the whole of the west side of the Haven.

3. The clay-slate is extensively developed on the east side of the

Haven from Temple Bay to Great Island at the confluence of the Suire with the Nore and the Barrow, forming a series of cliffs, in which the strata are excessively contorted. It also forms the cliffs from the west shore of the Nore opposite Little Island and the right bank of the Suire; a small patch of it occurs likewise south of Passage. The contortions in the cliffs on the east side of the Haven, Major Austin ascribes to lateral pressure, and he states that the ridges of clay-slate range at right angles to the band of conglomerate which traverses the promontory at Sand Eel Bay. These contortions are more especially marked and numerous between Dollar Bay and Bluff Head, ranging from the top to the bottom of cliffs 250 feet in height. At the south point of Booley Bay the slate affords beautiful examples of ripple marks. It is stated to be perforated along the whole line of coast, by hollows varying in size from a pin's point to five inches in length, and to the height of 20 feet above high-water mark. On the hill above the Roman Catholic chapel, north of Duncannon sands, the slate has a prismatic structure. It is stated to have been altered by igneous operations on the west side of Great Island and in Little Island. Near Ballyhack the cleavage is diagonal to the plane of stratification.

4. Trap forms at Herrylock a mass protruding from the beach; also the narrow point on which Duncannon Castle stands, where it is stated to rest on the tilted edges of clay-slate, and to have altered that rock into an indurated, stratified, yellow stone. A basaltic dyke occurs opposite Great Island on the Kilkenny side of the Nore. It rises abruptly to the height of 100 feet, and from its white colour, due to the decomposition of the felspar, is called the White Horse. A considerable mass of trap is also at the Orphan School House; and other masses at Cheek Point in the bend of the Haven to the southward, and at Newtown on the western shore of the Haven.

5. Alluvium. Modern accumulations occur in Dollar Bay; Aldridge Bay; to the north of Bluff Head, where a layer of slate shingle is converted into a coarse conglomerate by carbonate of lime; between Bluff Head and Duncannon, constituting the Duncannon sands, and extending inland up a small valley, in which is a peat bed; the narrow valley at King's Bay or Arthurstown; to the south of Dunbrody Abbey; and in Woodstown valley, on the west side of the Haven. Major Austin states, that greenstone boulders occur in all parts of Wexford, but that no rock *in situ* of character similar to them is to be found in the county.

December 4, 1839.—A paper was first read, entitled "A Description of the Soft Parts and of the shape of the Hind Fin of the Ichthyosaurus, as when recent," by Richard Owen, Esq., F.R.S., F.G.S.

The osseous frame-work of the fin of the Ichthyosaurus, Mr. Owen observes, having alone been the subject of direct examination, the exact shape and the nature of the soft parts had been matters of conjecture. A very striking deviation from the reptilian and mammalian types had, indeed, been recognised, and resemblance also to the fins of fishes had been admitted in the digits of the fin exceeding five, in their being sometimes bifurcated, and in consisting of an extraordinary number of ossicles; yet owing to the form of the digital ossicles, their breadth and flatness, and their large size, as compared with the joints of the fin-rays of fishes, it had been generally supposed that the locomotive organs of the Ichthyosaurus were enveloped, while living, in a smooth integument, which like that of the turtle and porpoise, had no other support than was afforded by the bones and ligaments within.

Sir Philip Grey Egerton in a recent examination of Ichthyosaurian remains in the possession of Mr. Lee of Barrow-on-Soar, detected, with the penetration which has enabled him to bring to light many other obscure points in the structure of the Ichthyosaurus, traces of the soft parts of the fin in a slab of lias containing a mutilated paddle; and having submitted the specimen to the examination of Mr. Owen, a detailed account of its character forms the subject of this memoir.

Mr. Owen considers the specimen to be a posterior fin of the *Ichthyosaurus communis*. It presents impressions and fractured portions of six digits, with the impression,—and a thin layer, most distinctly preserved,—of the dark carbonized integument of the terminal half of the fin, the contour of which is thus most beautifully defined.

The anterior margin is formed by a smooth unbroken well-marked line, apparently a duplication of the integument; but the whole of the posterior margin exhibits the remains and impressions of a series of rays by which the fold of the integument was supported. Immediately posterior to the digital ossicles, is a band of carbonaceous matter of a distinctly fibrous structure, varying from two to four lines in breadth, and extending in an obtusely-pointed form for an inch and a half beyond the digital ossicles. This band Mr. Owen believes to be the remains of the dense ligamentous matter which immediately invested the bones of the paddle, and connected them with the enveloping skin. The rays, above mentioned, are continued from the posterior edge of this carbonized ligamentous matter, in which their bases appear to have been implanted, to the edge of the tegumentary impression; the upper rays being directed transversely, but the others gradually lying more in the direction of the axis of the fin, as they approach its termination. The most interesting feature in these rays, Mr. Owen says, is their bifurcating as they approach the edge of the fin.

From the rarity of their preservation, their appearance and coexistence in the present instance with remains of the integument, he states, it is evident they were not osseous, but probably either cartilaginous, or of that albuminous horn-like tissue, of which the marginal rays consist in the fins of the sharks and other plagiostomous fishes. Besides the impression of the posterior marginal rays, the specimen presents a series of fine, raised, transverse lines, which cross the whole fin, and probably indicate a division of the rigid integument into scutiform compartments, analogous to those on the paddle of the Turtle and webbed foot of the Crocodile; but they differ in the absence of subdivision by secondary longitudinal impressions. The structure of the integument of the fin agrees, therefore, with the known reptilian characters of the skeleton of the Ichthyosaurus; and, as the skin with its appendages gives a character to the great primary groups of vertebrata, it might be expected that the skin of the Ichthyosaurus would exhibit some of the characters of the integument of existing reptiles.

In conclusion, Mr. Owen remarks, that the other new facts presented by the specimen, accord with the indications of the natural affinities of the Ichthyosauri afforded by their less perishable remains; and that all the deviations from the reptilian structure of the skeleton tend to the type of fishes and not to that of cetaceous remains.

A paper was afterwards read on as much of the great graywacke system as is comprised in the group of West Somerset, Devon, and Cornwall, by the Rev. D. Williams, F.G.S.

This communication is supplementary to one read in April 1839*, and contains the results of the author's last investigation into the structure of the country. Before he details his present views, he corrects what he conceives was an error in his former paper, and states, he is now convinced that the slates and limestones of South Devon and Cornwall are not a prolongation of the trilobite slates (No. 7)† of Exmoor, but a distinct formation superior to the floriferous grits or culm measures (No. 9,) of central Devonshire, and consequently the newest deposits of the country. For this formation he proposes the name of Killas or Cornish, and he considers it as No. 10 in the ascending series. According to the new arrangement therefore, in proceeding from those parts of West Somerset bordering the Bristol Channel to the south of Devon and Cornwall, he conceives a regular ascending series is passed over, the central portion of which he is of opinion, consists of the floriferous grit or culm measures (9). He places the whole series also in the graywacke system.

Mr. Williams's reasons for this arrangement are drawn from observations made at several localities in South Devon.

At Doddiscombe Leigh, about four miles to the north of Chudleigh, the Posidonia limestone, a member of No. 8, the Coddon Hill grit, underlies, Mr. Williams says, a long series of alternations, exposed on the turnpike road towards Chudleigh, and consisting of high hills of Coddon and floriferous grits with intercalated killas, the whole dipping beneath the coral limestones of Chudleigh, which pass under the ridge of Ugbrook considered by Mr. Williams to be composed of floriferous grits. On closely examining the limestones about Chudleigh, the author discovered minor alternations, which exhibited in juxtaposition the several varieties of Cornish killas, pale green, light blue, and purple volcanic ash and Coddon Hill grits (the two latter containing plants), interstratified beyond any doubt among the coral limestones, and these limestones inclosing carbonaceous beds, underlaid and overlaid by thick accumulations of Coddon Hill and floriferous grits. On the road by Grayleigh, and descending further on to Waddon Burton, Mr. Williams has traced these alternations foot by foot, and he says the sequents are so manifestly palpable in their interchanges, great and small, that nothing is left for inference or conjecture.

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* See Proceedings, vol. iii., p. 115, [or L. & E. Phil. Mag., vol. xv., p. 396.]

† The following is the descending arrangement given in Mr. Williams's former paper: 9, floriferous slates and sandstones (*culm measures*); 8, Coddon Hill grits; 7, trilobite slates; 6, Wollacomb sandstones; 5, Morte slates; 4, Trentishoe slates; 3, calcareous slates of Linton; 2, Foreland or Dunkerry sandstone; 1, Cannington Park limestone.

The inseparable connexion of the coral limestone and Cornish killas with the floriferous series, he states, is also most plainly exhibited to the N.E. of Kingsteignton, at Ashburton, Newton Bushel, Abbots, and King's Kerswell, Marldon, Berry Pomeroy, Higher and Lower Yalberton, and thence to Brixham and Berry Head. At Meadfort sands, on the south, the author says, there is a strange intermixture, yet in regular stratified order, of Cornish clay slate, and buff-coloured, finely arenaceous beds inclosing shells and corals, with true floriferous sandstones containing plants, also with culmy slates, and strata of volcanic ash and coral limestones, forming an anticlinal axis, ranging north and south, and throwing off the great mass of the Torquay limestones to the east and west.

To the east of Dartmoor, Mr. Williams has little doubt the same alternations on a great and small scale occur, as far as the parallel of Torquay, immediately to the south of which line, but west of the bay, are said to be lofty conspicuous hills of the floriferous strata (No. 9), which he conceives "may be abruptions"; but their bases are commonly concealed by a thick mantle of new red sandstone. To the west of Dartmoor, independent of what he believes to be an axis of No 8, and forming the southern margin of the culm-trough, Mr. Williams has been led to conclude, that the floriferous beds, by other arcs of undulation, have broken through the thinner northern border of the killas, throwing it off on each shoulder and intercepting it in troughs. Instances of this, he says, are sufficiently apparent on each side of a line drawn from Greston Bridge on the Tamar, S.E. of Launceston, to Heath Field, north of Tavistock; the high central ridge consisting of floriferous and Coddon grits, flanked on the north by undoubted killas and volcanic ash and breccia. To the N.W., W.S.W., and S. of Greston Bridge, the floriferous series is stated to be exposed in great force, extending almost uninterruptedly in a straight line into the great body of the culm-field; but to the west it is said to be generally concealed by killas, though a continuous contracted range branches off to the westward by Lezant, Lawanick-down, and Alternau; and it appears to be depressed under, or to pass into the killas and speckled slates of Davidstow, N.E. of Camelford.

A small area of an inverted arc of undulation is exposed in and around Tavistock; and the floriferous grit, principally identified by its associated slates, is stated to underlie the high killas range of Whitcombe Down on the south; and on the north to be laid open in the road to Launceston, similarly associated, with the additional accompaniment of intercalated killas and Coddon Hill grit dipping beneath the volcanic ash of Marly Mead. Mr. Williams also mentions a shaft sunk in a copper lode close by the turnpike gate on the road from Tavistock to Callington, as another instance of floriferous grit, or a rock exactly the same as that which contains the copper lode of Wheal Friendship, underlying pale green slate or killas: and he adds, there can be no doubt that Wheal Friendship mine is in No. 9 or the floriferous series. The next instance mentioned in the paper, is at Linkinghorn and South Hill, north of Callington, where there is stated to be an entire suite of alternations of killas, Coddon grits, and floriferous grits with plants, all dipping south. Again, near Pillaton, between Callington and Saltash, there are said to be countless minor alternations of floriferous and Coddon grits, each thinning out and interlocking like the teeth of a trap. Another instance of the killas overlying the floriferous grit, is said to occur in a hill at Penter's Cross on the road from Callington to Saltash, where a cutting exhibits in the lower part, a series of alternations of floriferous sandstone, and culmy slates which in ascending disappear, and in their place a delicate pale green killas alternates with the sandstone beds, while at the summit the sandstones disappear and are replaced altogether by killas. A fault traverses the hill, and to the south of it only killas occurs.

The general results of his observations, Mr. Williams says, show, that in the ascending order, from Cannington Park and the Quantocks in Somersetshire, to the Land's End, there is a group constituted of ten strikingly simple, consecutive series, severally varying in their mineral and zoological character; that as respects the limestone suites, the thin and spare dimensions of such as occur in the Trilobite slates of Exmoor (No. 7) render them too insignificant to be noticed; that the Posidonia limestones, which by mineral gradation succeed and conformably overlie the latter, are elliptically included in the Coddon Hill grits (No. 8), and together distinctly underlie and constitute the base of the great floriferous series (No. 9): that higher up in No. 9 is an extended horizon, which separates the series into an upper and a lower, containing the Bampton, Hockworthy, Holcomb Rogus, and Hastleigh limestones, with red and black slates on the north; the Petherwen and Landlake slates and limestones on the south; and the entire suite of coral limestones to the east of Dartmoor, extending from Chudleigh to Berry Head and Brixham: that the Plymouth limestones included in the killas (No. 10) are higher in the group than those just mentioned, and are introduced first at Millaton, about a mile and a half west of St. Germains.

Mr. Williams considers the slate or killas series of S. Devon to be distinguished from the slates of Exmoor by a peculiar extraneous cleavage. In Exmoor the cleavage, he says, is at all angles, from less than 10° to the vertical, its planes having a direction of about east and west; whilst the cleavage lines of the killas either coincide with the magnetic or true meridian, or depart from it to the east or west only a few degrees, and the inclination approaches the vertical with a strike N. and S., or nearly perpendicular to that of Exmoor.

What, says Mr. Williams, are the results, if the question be tested by the assumed law, that strata may be identified by their organic remains? If the Posidoniæ and Goniatites of the lenticular, black limestones, never exceeding thirty-five feet in thickness, be appealed to, to identify them with the mountain limestone, the weight of organic remains opposed to them in the Launceston and Petherwen fossils, the corals and other organic remains of South Devon belonging to the floriferous series, reduces the evidence to dust. If mineral characters be appealed to, he says, they fail altogether.

In conclusion, Mr. Williams remarks, that in this supplement, he has endeavoured faithfully to transfer the simple truths of nature to his pages, without reference to the theories of others. He would, however, remind geologists that the proposed law of Mr. W. Smith is no law, if it do not imply a final and universal extinction of species. This being his own view, Mr. Williams says, he could not admit that the Goniatites and Posidoniæ of Devonshire were first introduced and became extinct with the mountain limestone, being justified by the fact of superposition, and more reasonable analogy, in concluding that these genera existed elsewhere in congenial conditions during the entire period of the deposition of the Trilobite slates, when that formation ceasing in Devonshire, the ova of the creatures or the creatures themselves were transported to a region favourable to their existence, and were continued during epochs of duration up to the period of the mountain limestone, and probably beyond it, if they be not now in existence. They appear, in his opinion, like the corals of Devon, to have been subject to repeated mineral accidents, and to have been locally destroyed in groups, not universally effaced in species. To guard himself, however, from misconstruction, Mr. Williams adds, he believes entirely in the extinction of genera and species; but at very distinct epochs, and in far thicker and more extended groups of strata than is imagined; and that consequently the identification of strata must be regulated by a per-centage test similar to that applied by Mr. Lyell to the tertiary series. Lastly, he protests against the determination of the age of the Devonshire formations by reference to the structure of a foreign district.

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE ROYAL INSTITUTION.

May 1.—Mr. Griffiths on the sources and uses of sulphuric acid.

May 8.—Mr. Faraday on the origin of electricity in the Voltaic pile.

May 15.—Mr. Macilwain on respiration and its relation to animal temperature.

May 22.—Mr. Brande on white lead.

May 29.—Mr. Brockedon on some new applications of caoutchouc.

June 5.—Rev. Dr. Scoresby on magnetism.

June 12.—Mr. Carpmael on the manufacture of wire cards.

XII. *Intelligence and Miscellaneous Articles.*

LETTER TO PROF. LIEBIG ON THE THEORY OF SUBSTITUTIONS[*].

SIR,

I HASTEN to communicate to you one of the most brilliant facts of organic chemistry. I have verified the theory of substitutions in an extremely remarkable and perfectly unexpected manner. It is only from the present time that we shall be able to appreciate the high value of this ingenious theory, and to foresee the immense discoveries it promises to realize. The discovery of chloracetic acid, and the constancy of the types in the chlorinated (*chlorés*) compounds derived from æther and the chloride of æthyle, have led me to experiments which I will now describe. I passed a current of chlorine through a solution of the acetate of manganese under the direct influence of solar light. After twenty-four hours I found in the liquid a superb crystallization of a yellow violet salt. The solution contained nothing further than this salt and hydrochloric acid. I analysed this salt: it was the chloracetate of the protoxide of manganese. Nothing extraordinary as yet; a simple substitution of the hydrogen of the acetic acid by an equal number of equivalents of chlorine, already known from the beautiful experiments on chloracetic acid. This salt, heated to 110° in a current of dry chlorine, was converted with disengagement of oxygen into a new compound of a gold yellow colour, the analysis of which had for its composition to the formula $Mn\,Cl\,2 + C4\,Cl\,6\,O\,3$. There was therefore a substitution of the oxygen of the base by chlorine, similar to what has been observed in a multitude of cases. The new substance dissolved in quite pure chloral with the aid of heat. I employed this liquid, unalterable by chlorine, in order to continue the treatment by means of this agent. I passed dry chlorine during four days, keeping the liquid constantly near its boiling point. During this time a white substance was constantly deposited, which, when attentively examined, proved to be the chloride of protochloride (?) of manganese. I cooled the liquor when all precipitation had ceased, and obtained a third body in small needles, silky and of greenish yellow colour; it was $C\,4\,Cl\,10\,O\,3$, or in other terms it was the acetate of manganese, in which all the hydrogen and the oxide of manganese had been replaced by chlorine. Its formula should be written $Cl\,2\,Cl\,2 + C\,4\,Cl\,6\,O\,3$. There were therefore six atoms of chlorine in the acid, the four other atoms representing the oxide of manganese. Just as hydrogen, so also manganese and oxygen may be replaced by chlorine, and nothing surprising will be found in this substitution. But this was not the end of this remarkable series of substitutions. On letting a new chlorine act on a solution of this substance in water, there was a disengagement of carbonic acid; and on cooling the liquid to + 2°, a yellowish mass, formed of small laminæ, was deposited, very much resembling the hydrate of chlorine, and indeed it consisted of nothing further than chlorine and water. But in taking the density of its vapour I found that it was formed of twenty-four atoms of chlorine and of one atom of water. Here then is the most perfect substitution of all the elements of the acetate of manganese. The formula of the substance should be expressed by $Cl\,2\;Cl\,2 + Cl\,8\;Cl\,6\;Cl\,6 + Cl\,9$. Although I am aware that in the bleaching action of chlorine there is a substitution of the hydrogen by the chlorine, and that the stuffs which are at present bleached in England, according to the law of substitutions, preserve their types*, I nevertheless believe that the substitution of carbon by chlorine, atom for atom, is a discovery belonging to me. I trust you will take a memorandum of this note in your valuable Journal, and believe, &c.,

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[*] From Liebig's *Journal der Chemie und Pharmacie*, vol. xxxiii. part 3.

SCHWINDER†.

ON THE FORMATION OF LAMPIC ACID.

Lampic acid, according to the experiments of Stas and Martius, is a mixture of formic and aldehyd acids. Mr. R. F. Marchand states that these acids vary in their respective proportions in the combination according to the temperature of the platina wire generating the acid, and that they may be obtained in a constant proportion, which may be nearly accurately determined when the experiment is made with alcohol or æther. When alcohol or æther is dropped upon a red-hot platina dish, the peculiar phænomenon is instantly produced; the liquid runs about the red-hot metallic surface without quickly evaporating, and forms the known figures which have been described by Bœttcher. The vapour may be collected by putting a tubulated glass retort with the bottom off over the platina dish. By pouring fresh liquid through the tube a considerable quantity may be obtained in a short time. Upon examination it is easily ascertained that it is no longer alcohol, but possesses all the properties of lampic acid. The composition, or rather the mixture of the two ingredients, varies according to the temperature of the platina dish, in the same manner as that produced by platina wire. In order to ascertain whether the effect was peculiar to platina, Mr. Le Marchand tried glass, porcelain, *polished* copper and iron dishes; these also produced the same effects. They must, however, be polished and not present any rough surfaces; in the latter case the peculiar effect was not produced: it is also not produced when sand or glass is strewed over the platina dish.

The temperature to which the alcohol rises varies (as is the case with water) according to the heat of the dish and to the size of the drop. With water Mr. Le Marchand found it very accurately beginning at 180°, increasing to 204°, and then to 212° Fahr.: arrived at this point, the experiment is over, by the boiling of the water. The same takes place with regard to alcohol; the temperature, beginning at 140°, rises at last to the boiling point of the liquid. When this takes place, the greater part of the alcohol burns; but before the burning commences, light blue flames are visible, which Dœbereiner also observed upon the vaporization of æther dropped on hot platina. Mr. R. F. Marchand wishes to guard against the inference that might be drawn from this experiment, that he considered that the liquids used were decomposed. The formation of lampic acid is based upon the fact, that the undecomposed vapour of alcohol passing within a certain distance, immediately over the hot metallic surface, is oxidated, which is also the case with platina wire. If the vapour were in actual contact with the metallic surface it would inflame.

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* I have just learnt that there are already in the shops of London stuffs in spun chlorine (*étoffes en chlore filé*), much in request, and preferred to everything for night-caps, drawers, &c.

† [*Schwinder* we presume means Hoaxer."—EDIT.]

Both experiments are a confirmation of Buff's explanation of the repulsion in Poggendorff's Annals, xxi., which is so full and satisfactory that it is unnecessary to look for any further elucidation.—*Journal für Pratische Chemie*, 1840. No. 1.

ANALYSIS OF THE ASHES OF THE *Salsola Tragus.*

M. Guibourt has analysed the ashes of this plant from the neighbourhood of Cherbourg. He found that it consisted, independently of some silica, of

Carbonate of potash	29·04
Chloride of potassium	17·89
Sulphate of potash	4·93
Carbonate of lime	40·26
Phosphate of lime and oxide of iron	7·88
	100.

M. Guibourt observes, that it is curious that the salts of the ashes should contain potash as the alkali, and that it constitutes an exception to a maritime plant containing no soda.—*Journ. de Chim. Med.* Mars, 1840.

UNCOMBINED HYPOSULPHUROUS ACID.

M. Langlois, professor of chemistry at Strasburg, has succeeded in isolating hyposulphurous acid; he obtained it perfectly pure by decomposing hyposulphate of potash, with oxichloric acid, which forms an insoluble salt with potash.

The acid thus obtained is liquid, colourless, and of a slightly syrupy consistence. A period arrives when its density cannot be increased without partially decomposing it. Its taste is strongly acid and bitter; it does not appear to be very caustic. When exposed to the air, it attracts moisture, when heated in a glass tube to 176° Fahr., and there are produced a deposit of sulphur and sulphurous acid gas. It does not render the solutions of the salts of lime and strontia turbid. It produces no effect upon the solutions of the salts of iron, zinc, or copper; but with the salts of lead it yields a white precipitate which becomes black when heated. In the solution of nitrate of silver it forms at first a yellowish precipitate, which soon becomes black, and sulphuret and sulphate of silver are produced. The salts of mercury and platina are precipitated black by this acid; and it will be observed that it acts on the different salts in the same way as hyposulphate of potash.

Nitric acid reacts instantaneously on concentrated hyposulphurous acid, nitric oxide is evolved, sulphur is deposited and the solution contains sulphuric acid. The action of chloric acid is not less remarkable than that of nitric acid; the decomposition of both acids occurs instantaneously, with tumultuous action; sulphur and chlorine evidently appear, and reagents show the presence of sulphuric acid in the solution: the phænomena are similar to those observed when chloric acid is dropped into alcohol or æther. In this latter case as in those also happens inflammation of the excess of the combustible body.—*L'Institut,* No. 327.

ON THE PRESENCE OF IODINE IN COD OIL. BY R. F. MARCHAND.

The different statements of chemists with regard to the presence of iodine in cod oil (Leberthrane) appear to have arisen from the circumstance of those who have not found it having examined impure oil. Cod oil was examined by a chemist of this place which had been received direct from Bergen, without his having succeeded in detecting any iodine in it.

As in all probability its energetic action depends upon the presence of iodine, that statement might lead the medical professor to reject Bergen cod oil. I have also received cod oil from the same source which was certainly pure. I examined it according to Gmelin's method (*Ann. der Pharm.*, B. xxxi. p. 523.) which is both a simple and sure method. This indicated the presence of iodine in a manner about which there could not be the least doubt. Twenty grammes was about the quantity acted upon.—*Journal für Praktische Chemie.* 1840. No. 4.

MR. M'CORD'S OBSERVATIONS ON THE SOLAR AND TERRESTRIAL RADIATION MADE AT MONTREAL.

A Meteorological Register for 1838 was some time since kindly communicated to us by Dr. Daubeny, kept at Montreal, Lower Canada, in lat. 45° 50′ N., lon. 73° 22′ W., by J. S. M'Cord, Esq., Corresponding Secretary to the Natural History Society, and Member of the Literary and Historical Society, Quebec. It is an abstract of observations of the Barometer, Thermometer, Rain Gauge, Snow Gauge, Actinometer, and Register Thermometer; all made by the first British artists, and carefully compared with standard instruments.

The mean pressure of the year, corrected and reduced to 32° Fahr. is 29·884 in.; the mean temperature, (mean of maxima and minima by Register Thermometers) 41°·58.

We extract the observations of the solar and terrestrial radiation entire, principally because so few observations have been made public, or even we believe instituted, with the Actinometer, an instrument which we are glad to see appreciated by distant observers.

SOLAR RADIATION.

DAY & HOUR.		2	3	4	WEATHER.
	A.M.				
July 19	10,30	19·50	68	114	Clear.
	11,00	17·62	69	122	A few light clouds.
	11,30	19·50	69·5	118	Clear.
	Noon.	20·00	71	120	Fleecy clouds.
	12,30	20·00	73	116	Perfectly clear.
	1,30	20·00	73	......	Clear.
26	Noon.	14·67	71	94	Clear, blowing fresh.
27	4 P. M.	17·07	77	......	Clear.
28	Noon.	19·05	77	98	Clear.
	3 P. M.	19·50	78	101	Clear.
29	Noon.	19·80	83	105	Clear, fresh breeze.
Aug. 2	Noon.	17·30	73	100	Clear.

REMARKS.

These observations were made during the warmest and brightest days of July, on the Montreal mountain, place of observation 307 feet above St. Lawrence. The *first* column gives the day and hour; the *second*, the indication of Sir John Herschel's Actinometer,—by Robinson, London,—mean of three observations; the *third*, the temperature of air in the shade, Thermometer 5 feet above the earth; the *fourth*, the indication of the Thermometer placed in the sun on garden mould, not blackened, after an exposure of ten minutes. N.B. This Actinometer was, in Sept. 1837, compared on the same spot, with one in the possession of Dr. Daubeny, of Oxford, England, and gave similar indications.

TERRESTRIAL RADIATION.

DAY.		2	3	4	Wind	
July..	18 to 19	69	60	50	NW.	Clear.
	19 to 20	73	54	50	do.	do.
	22 to 23	71	55	50	N.	do.
	26 to 27	73	61	49	SE.	do.
	27 to 28	78	69	62	SW.	do.
	28 to 29	80	70	65	SW.	Showery.
Aug..	6 to 7	76	69	63	SW.	Clear.

The *first* column indicates the 24 hours during which the observation was made, reckoning from 9 A.M. of one day to 9 A.M. of the next; the *second* gives the *max.* of the Thermometer in the shade, for the same period; the *third*, the *min.*; the *fourth*, the indications of a register spirit Thermometer, placed on a grass plat and exposed freely to the heavens during the night. All these instruments are made by first artists, British, and carefully compared with standards.

METEOROLOGICAL OBSERVATIONS FOR MAY, 1840.

Chiswick.—May 1. Slight haze: fine. 2—4. Hot and dry with easterly wind. 5. Overcast. 6, 7. Slight haze. 8. Heavy showers: fine. 9. Rain with sultry intervals. 10. Cloudy: sultry. 11. Drizzly. 12. Overcast. 13. Rain: sultry. 14. Cloudy and fine. 15. Heavy rain with thunder. 16. Cloudy: showery: hail shower at 12¼ P.M. 17. Rain. 18. Cloudy and fine: rain. 19, 20. Cloudy and cold. 21. Clear and cold. 22. Overcast. 23. Clear and fine. 24. Overcast: rain. 25. Cloudy. 26. Rain. 27—30. Very fine. 31. Hot and dry: cloudless.

Boston.—May 1. Cloudy. 2—4. Fine. 5, 6. Cloudy. 7, 8. Cloudy: rain early A.M.: rain P.M. 9. Rain: rain early A.M. 10. Cloudy: rain early A.M. 11. Rain: rain early A.M. 12. Cloudy: rain early A.M. 13. Rain. 14. Cloudy: rain with thunder and lightning P.M. 15. Rain. 16, 17. Cloudy: rain A.M. and P.M. 18, 19. Cloudy. 20. Cloudy: rain P.M. 21, 22. Stormy. 23, 24. Cloudy. 25. Stormy: rain A.M. 26. Rain: rain A.M. 27. Cloudy. 28. Fine. 29. Stormy. 30, 31. Fine.

Applegarth Manse, Dumfries-shire.—May 1, 2. Beautiful day. 3. The same: Thermometer in shade 75°. 4. Very dry and warm till P.M. 5. Very droughty. 6. The same increased: cloudy. 7. Slight showers all day. 8. The same A.M.: cleared up. 9. Slight showers early A.M. 10. The same: thunder P.M. 11. Rain heavier. 12. Rain nearly all day. 13. Fair. 14. Showery P.M.: thunder. 15. Occasional showers: thunder. 16. Rain preceding night: clear day. 17. Rain in the night: fair. 18. Fresh and cool. 19. Dry and rather boisterous. 20. Dry and more moderate. 21. Very droughty: clear sky. 22. Calm and warm. 23. Showery: high wind. 24. Showery. 25. Showery, and very high wind. 26. Fresh and showery. 27. Fine A.M.: wet P.M. 28. Very wet till P.M. 29, 30. Fine growing day. 31. Variable: bright at midday: wet evening.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. May.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max.	Min.		Max.	Min.									
1.	30·356	30·343	30·325	29·80	30·25	30·23	48·7		48·2	67	43	49·0	71	45½	NNE.	NE.	E.	SE.	...	...	...	...	50
● 2.	30·288	30·290	30·194	29·82	30·21	30·14	55·4		45·2	74	42	53	72	43	E.	E.	E.	SSE.	...	...	...	...	52
3.	30·172	30·161	30·109	29·72	30·14	30·10	59·3		46·3	74	43	55	75	44	E.	E.	E.	SE.	...	...	...	...	49
4.	30·126	30·105	29·994	29·68	30·05	30·00	58·3		48·7	70	41	58·5	73	49½	E.	E.	E.	E.	...	...	...	...	49
5.	29·912	29·924	29·868	29·40	29·96	29·90	55·7		45·4	66	46	52	62	45	NW.	E.	E.	E by N.	...	...	...	...	49
6.	29·888	29·903	29·800	29·36	29·87	29·83	54·7		49·0	68	49	50	52	40½	N.	E.	E.	E by N.	...	·01	...	0·18	51
7.	29·760	29·761	29·702	29·22	29·70	29·64	57·3		49·7	68	50	51	46½	42½	SW.	SW.	calm	ENE.	·011	...	·03	...	51
☽ 8.	29·706	29·698	29·646	29·07	29·57	29·53	59·7		54·7	70	51	55	50½	41½	S.	S.	calm	ENE.	...	·12	·05	...	54
9.	29·524	29·525	29·425	29·07	29·50	29·53	55·0		54·0	71	51	55	50	42½	SE.	SE.	calm	NE.	·063	·60	·56	...	53
10.	29·402	29·474	29·392	28·86	29·55	29·57	59·8		53·3	72	50	52	51	38	S.	W.	NE.	NE.	·450	·01	·32	...	58
11.	29·534	29·644	29·533	28·95	29·60	29·67	52·8		51·6	55	50	49	54½	39	NW.	N.	NE.	NE.	·133	·01	·15	...	53
12.	29·670	29·694	29·657	29·14	29·65	29·68	53·7		51·2	62	48	51·5	50	40	ENE.	E.	E.	E.	...	·16	·18	...	52
13.	29·662	29·702	29·654	29·21	29·65	29·58	53·3		49·8	67	49	53	56	43	N.	S.	calm	E by S.	·180	·05	...	0·71	52
14.	29·678	29·682	29·562	29·09	29·50	29·40	59·3		51·9	66	50	55	63½	44	S.	S.	calm	E by S.	...	·34	·21	...	52
15.	29·414	29·442	29·404	28·79	29·29	29·23	53·7		53·2	60	49	53·5	64	48½	NW.	SW.	calm	NW.	·450	·41	·31	...	55
○ 16.	29·404	29·413	29·379	28·73	29·14	29·16	57·8		51·4	65	47	56	58½	48	S var.	SW.	W.	NW.	·447	·14	·11	...	53
17.	29·374	29·469	29·387	28·76	29·24	29·38	55·7		50·2	62	46	55	53½	45½	S.	S.	calm	NNE.	·033	·09	·14	...	52
18.	29·626	29·689	29·883	29·06	29·55	29·89	55·8		48·3	62	45	49	53½	42	W.	W.	NE.	NW.	·250	·01	·15	...	51
19.	30·080	30·132	30·060	29·60	29·93	30·01	47·3		46·6	56	34	45	52	42	NNW.	N.	NE.	NW.	...	...	...	...	49
20.	30·052	30·138	30·060	29·52	29·96	30·05	52·8		41·3	60	40	53	58½	46	NW.	NW.	NE.	NNW.	...	·01	...	0·18	48
21.	30·216	30·305	30·202	29·67	30·18	30·21	47·8		41·6	54	42	46	63	35½	NW.	N.	N.	W.	·066	...	·08	...	40
22.	30·344	30·393	30·361	29·83	30·25	30·20	48·8		44·3	64	36	49	60½	36½	NW.	N.	N.	SSW.	...	...	...	...	45
23.	30·356	30·361	30·229	29·75	30·10	29·89	57·7		44·2	71	51	58·5	56½	52	W.	SW.	calm	SW.	...	...	...	...	46
☾ 24.	30·172	30·182	29·953	29·47	29·82	29·57	61·5		53·3	67	55	61	60	45	SW var.	SW.	W.	WSW.	...	·20	...	...	53
25.	29·808	29·959	29·684	29·	29·40	29·65	58·8		56·3	66	45	62	54	46	S.	W.	W.	WNW.	·183	·01	...	...	55
26.	29·844	29·962	29·764	29·26	29·63	29·73	54·6		49·4	65	38	48	56	41	S.	W.	W.	W.	...	·03	·06	...	50
27.	30·050	30·041	29·960	29·43	29·77	29·70	56·7		45·6	74	39	57	59	41	SW.	SW.	W.	SW.	·063	...	·11	1·05	51
28.	29·926	29·960	29·887	29·25	29·58	29·65	60·8		50·9	77	39	61·5	59	41	SSW.	SW.	W.	SW.	...	...	...	..	52
29.	30·088	30·284	30·120	29·37	29·84	30·07	60·7		55·0	70	44	60	59½	44	NW.	W.	W.	WNW.	...	...	...	...	52
30.	30·398	30·388	30·365	29·76	30·10	30·05	61·4		51·4	73	48	62	57	40	S.	S.	calm	WSW.	...	...	...	...	54
31.	30·400	30·379	30·326	29·71	30·00	30·00	63·8		56·4	80	44	67	64	50	S.	S.	W.	SSW.	...	...	...		56
Mean.	29·911	29·948	29·867	29·33			56·3		49·6	67·00	45·32	54·2							Sum. 2·329	2·20	2·46		Mean. 51·1

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[THIRD SERIES.]

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XIII. *On the Theory of the dark Bands formed in the Spectrum from partial Interception by transparent Plates.* *By the Rev.* BADEN POWELL, *M.A., F.R.S., F.G.S., F.R.Ast.S., Savilian Professor of Geometry in the University of Oxford*[*].

(1.) THE phænomenon of peculiar dark bands crossing the prismatic spectrum, when half the pupil of the eye (looking through the prism) is covered by a thin plate of any transparent substance, the edge being turned *from* the *violet towards* the *red* end of the spectrum, was first described by Mr. Fox Talbot in 1837 (Lond. and Edinb. Phil. Mag. and Journal of Science, vol. x. p. 364.), who showed that these bands are due to the interference of the two halves of each primary pencil, one of which is retarded by the plate.

(2.) Sir David Brewster has given various new modifications of these experiments (British Association Reports, vol. vii. Trans. of Sections, p. 13.), the most material of which tend to show that the effect is fully produced *only* when the plate is in the position just described, and diminishes and disappears as it revolves in its own plane; the same observation being also extended to the case of the spectra formed by interference from grooved surfaces, or gratings.

(3.) The explanation given by Mr. F. Talbot accounts for *the production of the bands simply*, but assigns no reason why the interception must take place *on one side* more than the other. That it does so, is considered by Sir David Brewster as indicating an entirely *new property of light*; having reference to the different sides of the pencil related to their position of greater or less refrangibility, and which he has not inexpressively termed a peculiar "polarity."

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[*] Communicated by the Author.

(4.) My attention was drawn to the subject in the course of last summer, when I repeated the experiments, and devised several new modifications with reference to an explanation which it appeared to me was supplied by the undulatory theory; to these investigations I referred briefly at the Birmingham meeting of the British Association, 1839. For several reasons (on which I need not here enter) I have delayed publishing any details; nor should I do so now, but that having learned that Mr. Airy has recently pursued the research to many entirely new conclusions*, I am anxious to put on record the few points I have been able to establish, and to vindicate my views from misconceptions to which they have been exposed.

(5.) The following distinctions are important to be borne in mind with reference to the explanation of the phænomena.

In these experiments we have to consider the different elementary pencils of which the spectrum, as presented to the eye, is formed; and with respect to each of these, in the case of the *prismatic* spectrum it is easily seen that the edge of the plate intercepts *that half which lies towards the* EDGE *of the prism.*

In the *interference*-spectrum (according to Fraunhofer's method), the spectra are formed one on each side of the axis, with their violet ends towards it. The edge of the plate in this case must always intercept *that half of each primary pencil, which after passing the focus lies* NEAREST *to the axis.*

(6.) I have found that with the same prism the intercepting plate must be within limits of thickness, which differ according to the substance of the plate, and with the same plate the character of the bands differs with the medium of which the prism is formed. These differences appear to depend on the refractive and dispersive powers of the substances.

(7.) With a prism of flint glass and a plate of mica, the greatest thickness which can be used may be about the $\frac{1}{100}$th of an inch. In this case the bands appear fine and numerous, and it seems only in consequence of their increase in number that they cease to be distinguishable when the thickness is increased beyond this.

If we use less thicknesses (such as those into which mica is easily split) the bands become broader and fewer, and at length faint and ill-defined. It is perhaps not possible to distinguish them if fewer than four or five are formed throughout the spectrum. The bands are never very dark; showing that only a portion of the rays is concerned in their formation.

[* A notice of Mr. Airy's paper on this subject will be found in our report of the proceedings of the Royal Society for June 18, 1840.—EDIT.]

(8.) When the plate is very thin, another set of appearances presents itself.

On splitting a piece of mica to such a tenuity that only a few indistinct broad bands were barely visible with a flint-glass prism, I observed at the same time *another set of very fine but extremely faint bands*, evidently independent of the former.

(9.) When the film of mica was still thinner, the *broad* bands ceased to appear altogether, and *only* the *fine* set were visible. To show *these* bands the films must be so thin as to be nearly iridescent: it is difficult to succeed in tearing them off sufficiently fine. I have sometimes used drops of water between glass plates pressed hard together. These bands are always very faint; but they are somewhat more conspicuous with prisms of the more dispersive oils, and always require a strong light to be seen.

(10.) There is, however, a more remarkable circumstance connected with *this set of bands*; *they continue to be formed when the edge of the thin film is towards the thicker side of the prism.*

(11.) In pursuing the *theoretical* explanation, we have to consider the conditions which may affect the rays situated towards the opposite sides of those primary homogeneous pencils, into which the incident beam of light is separated, and which converge in the eye to form the several points in the spectrum, both in the case of the prism, and of interference from grooves or gratings.

Now in either case a distinction of this kind is deducible from the wave-theory, on comparing the length of undulatory route of the two extreme rays of any primary pencil; from which it appears that *one of these rays is always more retarded than the other*, as well in the prismatic as in the interference spectrum: that side of the pencil which is *previously the least retarded*, *being that to which the plate is applied* in the original form of the experiment. This distinction, combined with the general principles of explanation at first referred to, appear to me not only sufficiently to account for the *ordinary* phænomena, but in *my modification* of the experiment to assign a reason why a similar effect should be produced on the *opposite* side.

(12.) With regard to the mathematical investigation, in the case of the prism, without going into a formal discussion, it is sufficient to observe, that on the principles of mathematical optics, when a diverging pencil of homogeneous light is refracted through a prism in the position of minimum deviation, the emergent pencil will originate from a geometrical focus, which is *not a single point*, but a *caustic*, whose *convexity* is towards the edge of the prism. Hence, on the principles of the wave-theory, it follows that the side of the pencil which lies *towards the edge* of the prism is that which undergoes less *retardation*, or has the *shorter* undulatory route; this difference varies slightly for the different primary rays.

I had arrived at this conclusion by a different approximate method, when in some correspondence the Astronomer Royal pointed out to me the above view of the problem, as connected with his investigations "on the light in the neighbourhood of a caustic." (Camb. Trans., vol. vi. part 3*.)

(13.) With respect to the interference-spectrum, we have only to follow out the investigation given in Mr. Airy's tract (Arts. 80, 83.) (as that gentleman has suggested to me) in the following manner.

Taking the focus as the origin and the axis of the object-glass as the axis of x, let $x\,y$ be the coordinates of any point in the wave, $a\,b$ those of a point in the focal image on the same side of the axis, then the radius of the wave being c, we have

$$c^2 = x^2 + y^2;$$

and expanding and neglecting powers of y above the third, we find

$$x = c - \frac{y^2}{2\,c}.$$

The distance ϱ from $x\,y$ to $a\,b$ will be

$$\varrho = \sqrt{((x-a)^2 + (y-b)^2)}\ ;$$

here performing the various expansions, and for brevity writing $e^2 = (c-a)^2 + b^2$, we at length obtain (going to the third power of y)

$$\rho = e \times \left\{ \begin{array}{l} 1 + \frac{1}{2}\left\{\frac{2\,b}{e^2}\,y + \frac{a}{c\,e^2}\,y^2\right\} \\ \quad - \frac{1}{8}\left\{\frac{-2\,b}{e^2}\,y + \frac{a}{c\,e^2}\,y^2\right\}^2 \\ \quad + \frac{1}{16}\left\{\frac{-2\,b}{e^2}\,y\right\}^3 \end{array} \right\}.$$

The terms involving the second power of y have the same value on each side of the axis, and those depending on the third power are found to be

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[* See Lond. and Edin. Phil. Mag. vol. xii. p. 452.—EDIT.]

$$\left.\begin{aligned} &-\tfrac{1}{8}\left\{\frac{-4\,b\,a}{c\,e^4}\,y^3\right\} \\ &+\tfrac{1}{16}\left\{\frac{-8\,b^3}{e^6}\,y^3\right\} \end{aligned}\right\} = \frac{b\,y^3}{2\,e^4}\left\{\frac{a}{c} - \frac{b^2}{e^2}\right\}.$$

Here, if $\frac{a}{c} < \frac{b^2}{e^2}$, (which is the case, the image being formed in the focus, so that in fact we might assume $a = 0$,) it follows that when b and y have the same sign, this expression will be negative; that is, for the ray which has b and y on the same side, or is *nearest the axis after passing the focus*, the route will be the *shortest*. The difference will be very small, and will vary slightly for the different rays of the spectrum.

(14.) This difference of retardation in the several rays of each primary pencil, combined with the obvious principle laid down by Mr. F. Talbot, appears to me to supply an explanation of the phænomena.

The whole effect in these experiments is made up of two parts, the original retardation, and that superinduced by the plate. If the previously *least* retarded ray be intercepted, we take the *difference*, if the *most* retarded, the *sum* of the two effects.

When we apply the plate, the whole resulting retardation may fall within the limits, (before mentioned, § 6.) or not, according to the magnitude of the two retardations, and according as we take their sum or difference. If it be beyond the limits for one portion of the pencil, it may be within them for another.

In general, in the original form of the experiment, that is, for plates of ordinary thickness, the *difference* falls *within* the limits, though the *sum* is *beyond* them, for all portions of the pencil. But with a very thin plate, the *sum* may also be *within* the limits for *those parts* of the pencil whose difference of retardation is small: Or, in other words, with plates of a *certain thickness*, the retardation is too great to give bands with any portion of the pencils, when the plate is applied to the previously *most* retarded side: but it will give bands with some portion when applied to the previously *least* retarded side.

On the other hand, if an *extremely thin* plate be applied to the *most* retarded side, it will still give bands with one portion of the pencils, as well as when applied to the least retarded side with other portions.

Oxford, July 5, 1840.

XIV. *On the Potatoe Spirit Oil of the French Chemists.* *By* JAMES APJOHN, *M.D., M.R.I.A., Professor of Chemistry in the Royal College of Surgeons, Dublin*[*].

IN December 1838, I received from my friend Mr. Scanlan a specimen of an oily fluid which had been given him by Mr. Bowerbank, an eminent London rectifier, and which the latter gentleman had found in small quantity in the faints or weak spirit drawn off towards the close of the rectification of common whisky. Shortly previous to this time, Mr. Coffey, the inventor of the celebrated patent still, had observed the same substance at the extensive distillery of Sir Felix Booth; and upon coming over to Dublin, and visiting the establishment of Mr. Busby at Blockpitts in this city, Mr. Scanlan had the satisfaction of recognizing this same oil in the faint vessel, constituting a thin stratum resting upon the surface of the remainder of the fluid.

The oil obtained from Mr. Busby's concern had a reddish-brown colour, owing to dissolved vegetable matter, and its specific gravity was ·8401, that of the faints on which it rested being ·9269. Shaken in a graduated tube with an equal bulk of water, its volume was reduced 20 per cent., and the water upon distillation yielded alcohol. To insulate the oil, therefore, the following method was adopted.

The fluid obtained from the faint receiver was first washed with an equal bulk of water; then shaken in a bottle with an equal weight of pulverized and anhydrous carbonate of potash, and finally distilled from a glass retort, the condensation being effected by Liebig's tube refrigeratory. It began to boil at 262°, after which the temperature rose gradually until it became 268°, at which it continued until the whole of the oil was nearly over. The fluid first drawn off was set apart, as still containing alcohol, and that alone reserved for further purification which distilled over at 268°. This portion was redistilled. The ebullition commenced a little over 267°, and in less than a minute rose to 268°, at which point it continued until the rectification was nearly completed. The first and last portions being rejected, the middle portion, or that which came over at 268°, was set apart for experiment.

The oil thus procured is a perfectly colourless liquid, destitute of all viscidity. The specific gravity is ·8138, and, as has been already observed, it boils steadily at 268°; cooled to −6° it does not congeal. With rectified spirit it is miscible in all proportions, its specific gravity being thus augmented, and its boiling point lowered. It is immiscible with water, but nevertheless when agitated with this liquid, it absorbs an appreciable quantity of it. It has a pungent and peculiar odour, and a sharp biting taste, somewhat similar to that of the oil of cloves. When gently heated it readily takes fire upon approaching to it a lighted taper, and burns with a clear flame unaccompanied by smoke. It is an excellent solvent for the fats, and also for resinous substances. Camphor, for example, is readily dissolved by it; and the same may be said even of copal, if a gentle heat be applied. Potash is taken up by it in considerable quantity, oil of vitriol gives it a crimson colour. To determine its composition the following experiments were made:

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[*] Communicated by the Author.

(1.) 4·24 grains of the oil burned in the usual manner with oxide of copper gave of water 5·06 grains, and of carbonic acid 10·42 grains.

(2.) 7·71 grains gave of water 9·22 grains, and of carbonic acid 19·12 grains.

(3.) 6·63 grains gave of water 8·05 grains, and of carbonic acid 16·26 grains.

The following are the results deducible from these experiments:

	(1.)	(2).	(3.)
Carbon.........	67·96	68·59	67·84
Hydrogen	13·25	13·28	13·48
Oxygen	18·79	18·13	18·68
	100	100	100

The means of the numbers yielded by the three experiments are given underneath in column (*a.*). The numbers in column (*b.*) are the quotients of the corresponding ones in (*a.*) divided by the respective atomic weights of carbon, hydrogen, and oxygen, and those in column (*c.*) are others in the same ratio with the quotients.

	(*a.*)	(*b.*)	(*c.*)
Carbon	68·13	11·132	4·804
Hydrogen ...	13·33	13·330	5·753
Oxygen	18·54	2·317	1·00
	100·		

The inspection of the latter shows that the most probable formula for the oil is $C_5 H_6 O_1$, which would give the following parts per cent.

Carbon	68·60
Hydrogen.........	13·45
Oxygen............	17·95
	100.

Assuming this formula as the true one, the deficiency in the carbon experimentally determined is not greater than what usually takes place; but the error in the hydrogen, though trifling in amount, being upon the opposite side to that on which it usually occurs, it became expedient to resort to some method of verification. The specific gravity of the vapour of the oil was therefore taken according to the well-known method of Dumas.

The weight of glass ball filled with dry air, the pressure being 30·324 and temperature 48°·5, was 914·86 grs.

The ball was sealed at 364°, and then weighed 917·78 grs. Hence $917·78 - 914·86 = 2·92$ grains is the excess of the weight of vapour in ball over that of the air displaced.

The capillary end of the beak attached to the ball having been broken under mercury, it was ascertained by the amount of this metal which flowed into the ball, that its capacity was 16·76 cubic inches, which, at a pressure = 30, and a temperature = 60°, will (supposing it air) become $16·76 \times \frac{448+60}{448+48·5} \times \frac{30·324}{30} = 17·333$ cubic inches. From this must be subtracted ·1 cubic inch which was found to remain in the balloon, so that the bulk of air excluded by the vapour, when reduced to the mean temperature and pressure, was 17·233 cubic inches, whose weight = 5·344 grains. Hence $2·92 + 5·344 = 10·264$ is in grains the weight of the vapour.

The capacity of the glass balloon at 48°·5 being 16·76 cubic inches, it will, owing to the expansion of glass, become at 364° 16·843 inches. This therefore is the volume of the vapour and bubble of air at 364°. But the volume of the latter being 0·1, it will at 364° become 0·16. Hence the true volume of the vapour at $364° = 16·843 - 0·16 = 16·683$; so that $16·683 \times \frac{448+60}{448+364} \times \frac{30·324}{30} = 10·549$ is the volume of the vapour reduced to a temperature = 60° and pressure = 30. But as this weighs 10·264 grains, 100 cubic inches of it would weigh 97·298 grains. The specific gravity therefore of the vapour is $\frac{97·298}{31·0117} = 3·137$.

Now if the composition of the oil be $C_5\,H_6\,O_1$, the specific gravity got by adding together the products of the densities of the vapours of the different elements by the number of atoms of each would be 3·072. But we have here so close a correspondence between experiment and calculation, that no doubt can remain as to the correctness of the basis on which the latter rests, or that the true formula for the oil is that already assigned.

These experiments were made in the winter of 1839, and at the time I had concluded them I was under the impression that the oil, to which they relate, was a new substance, or rather one which had not been previously described. In some time after, however, upon looking over the second part of Mr. Graham's Elements of Chemistry, which had been sent me by the author, I was much surprised at finding at page 134, in a table of the volumes of atoms in the gaseous state, mention made of a substance under the designation of " oil of the ardent spirit from potatoes," to which he attributed the very same formula and density of vapour which I had found to belong to the oil found in grain-whisky, in the examination of which I had been recently engaged.

Anxious to investigate the matter further, and to ascertain if the two oils were certainly the same, I looked into Berzelius's System, and the 5th volume of the *Traité de Chimie, appliquée aux Arts*, by Dumas, devoted to the subject of organic chemistry, but could not find any mention in either of the essential oil from potatoe spirit alluded to by Graham. Upon, however, turning to Dr. Thomson's recent volume on vegetable chemistry, I found, page 481, a notice of this fluid, and references to the 30th and 56th volumes of the *Annales de Chim. et de Phys.* in the former of which its origin and properties are described by Pelletier, and in the latter of which its analysis is detailed by M. Dumas. The properties I find ascribed by these chemists to the potatoe spirit oil are precisely those which belong to that which I have examined from corn-whisky, the only difference being that Pelletier represents its specific gravity as ·821, whereas I have found that of the oil I obtained from Mr. Scanlan but ·813, a difference, however, easily explained by the circumstance of his not having taken the necessary steps for rendering the fluid he examined perfectly free from water and alcohol. The analytic results also of M. Dumas are nearly identical with mine, approaching however more nearly, as might indeed be expected from his great skill in this department of chemistry, to the formula $C_5\ H_6\ O_1$ which he adopts, and which Professor Graham has taken from his memoir. I may lastly mention, as a very unusual coincidence, that the specific gravity of its vapour, as obtained by Dumas, is 3·147, or but unity in the second place of decimals greater than what has resulted for the corn-spirit oil from my experiments.

We thus arrive at the conclusion, that the two fluids are identical, or that the oil which has hitherto been considered as peculiar to potatoe spirit, occurs also in that which is developed during the fermentation of grain. From this latter source it admits of being procured in great quantity. When first observed by Mr. Coffey at Sir Felix Booth's, there was an inch of it in the faint receiver, and from the diameter of the vessel he estimated its total amount at 50 gallons. This is the quantity produced at that establishment every fortnight, the excise laws compelling the distiller to distil and brew alternately, and a week being generally consumed in each process.

The whisky manufactured some years ago contained a considerable quantity of this oil, and owed to its presence a great deal of the pungency of taste by which it was distinguished.

From its high boiling point, and the nature of the stills at present used, but a very small portion of this substance now passes over, and hence the reason why the spirit at present made is, as compared with the product of the old processes, less disagreeable to the palate, and probably less injurious to the constitution. It is no doubt owing to the same cause, viz. an improvement in the process of distillation, that this oil has at length been noticed in the distillers' faints. Upon the old system of manufacture the greater portion of it was driven over, and was held dissolved by the spirit into which it was thus introduced; but with the modern stills, particularly that devised by Mr. Coffey, nothing having so high a boiling point as this oil can by possibility pass into the part of the apparatus where the spirit is condensed. With respect to the manner in which the substance originates, whether it exists ready-formed in the materials subjected to fermentation, or is a product of the process, I am not aware of any facts calculated to decide such a question. As, however, it is found in the fermented wash of both corn and potatoes, it may be presumed to be derived from the starchy principle, which is common to both.

The potatoe spirit oil, as it has hitherto been called, has I find of late attracted much attention. Pelletier from some rough experiments upon it with acids, threw out the conjecture that it was more analogous to alcohol than to the true volatile oils, and this opinion would seem to have been in some degree adopted by Dumas. More recently (*Ann. de Chimie et de Phys.*, Jan., 1839) M. Auguste Cahours has revived this opinion, and concluded it to be one of the group including alcohol, pyroxylic spirit, and acetone, and has even succeeded in procuring from it a carbo-hydrogen, in which the elements are, as usual, associated in the ratio of atom and atom. M.

Cahours represents potatoe oil by the formula $C_{10}\ H_{11}\ O_1 + H\ O$, which makes it as to composition perfectly analogous to ordinary alcohol. The carbo-hydrogen $C_{10}\ H_{10}$ he insulated by distilling the oil from anhydrous phosphoric acid. He calls it amylene, and finds the specific gravity of its vapour to be 4·904, so that an atom of it gives but one volume of vapour, a circumstance in which, as Cahours observes, it agrees with Dr. Kane's mesitylene, $C_6\ H_3$, but differs from the carbohydrogens which occur in alcohol and pyroxylic spirit. By acting on potatoe oil with sulphuric acid and chlorine, Cahours obtained compounds corresponding perfectly with those yielded by alcohol when similarly treated. These researches give additional interest to the discovery of this fluid in grain-fermented wash, and in such quantity as to be much more than adequate to meet any demand for it with a view to the interests of science.

I may here observe, that I should have long since presented this notice to the Academy, but for the following reasons.

There is another oily substance having, at common temperatures, the consistence of butter, which is long known to exist in the faints of grain spirit, and in smaller quantity in the spirit itself. Upon looking through systematic treatises on chemistry, I found that this oil had been but very imperfectly described, and that, in particular, no experiments had been made with the view of determining its composition. I had therefore resolved to submit it to an accurate examination and analysis, and to keep back what I had ascertained in reference to the fluid oil until I had completed my investigations into the nature and constitution of that which is a soft solid at common temperatures. In this investigation I had made some progress, when my attention was directed to a paper by Liebig and Pelouze, in the 63rd volume of the *Ann. de Chim. et de Phys.*, in which, with their usual ability, they develope the nature of a butyraceous or fatty product which they had received from M. Deleschamps, and which comes over towards the close of the process of distilling wines with a view to the production of *eau de vie* or brandy. This oil they found to be a mixture of an acid which they called œnanthic acid, and of a compound of this acid with the oxide of æthyle, that is of œnanthic acid and œnanthic æther. Upon perusing this paper I saw at once, from the experiments I had already made, that the fatty oil of grain spirit was identical with this mixture, with the exception that some third oleaginous material was present, which Liebig and Pelouze had not found in what they had operated upon. Upon this third substance I have made some experiments, the results of which I shall probably at some future period submit to the Academy. I have resolved, however, no longer to defer giving publicity to my experiments identifying the fluid oils of grain and potatoe spirit, having had my attention drawn by Dr. Kane to a recent volume of Poggendorff's *Annalen*, containing a paper by M. Mulder, in which I find myself anticipated on the other point; and the butter of corn spirit is satisfactorily shown to be what I had concluded it to be, not entirely from my own experiments, but from a comparison of them with the researches of Pelouze and Liebig. Mulder also notices the third principle which is associated with the œnanthic acid and œnanthic æther, and describes it under the name of *oleum siticum*. The object therefore of the present communication is much more limited than it was originally intended to be, professing only to announce the detection of the potatoe-spirit oil of Pelletier and Dumas in fermented infusions of the mixed grains employed by the distiller. But as Mulder conceived his discovery of sufficient interest to justify him in giving it to the scientific world, I shall, I trust, be pardoned for bringing an analogous fact under the notice of the Academy.

XV. *Observations on the Climate of Italy and other Countries in ancient times**.

A VAGUE notion seems to have prevailed for some time past among persons conversant with ancient authors, that the climate of Europe in the classical ages of Greece and Rome must have been considerably colder than at the present day. Latterly this question has been taken up by two philosophers, who from a consideration of the vegetation have come to a different conclusion. Most persons probably have read the interesting essay by Arago in the *Annuaire* for 1834, who states that several of his facts have been borrowed from the writings of Schouw. The conclusions drawn by these writers are probably in the main correct; but some of the facts stated by them appear to require modifications, which are more fully explained in the following pages.

I. *Vegetation of Ancient Italy: the beech, the date-palm, the olive.*

It has been said that Virgil speaks of the beech as growing in the neighbourhood of Rome; whereas now the climate is too hot for that tree, which is not found till we reach a considerable height on the Apennines. In fact, Tenore (*Cenno*

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* Communicated by the Author.

sullo Geografia Fisica e Botanica del Regno di Napoli, p. 60*.) places the region of the beech in Southern Italy, at from 400 to 600 toises above the level of the sea. Virgil, however, in reality never speaks of the "fagus," generally supposed to be the beech, as growing near Rome. That tree is mentioned twice in the Georgics (i. 173. and ii. 71.), but in a way from which nothing can be concluded with regard to its locality; and once in a similar way in the Eclogues (iii. 12.): there are, however, two other passages where the indication of the locality is more precise. The first is the well-known passage of the first Eclogue. It is too obvious to require any discussion, that in this instance the scene is laid in the territory of Mantua. The second passage (Ecl. ix. 9.) refers to the same country, and specifies the position of the beeches still more clearly as in the plain, but not far from the foot of the hills,

> "———— quà se subducere colles
> Incipiunt, mollique jugum demittere clivo,
> Usque ad aquam et veteres, jam fracta cacumina, fagos."

With the exception of a passage in the 2nd Eclogue (l. 3.), where the scene is laid "Siculis in montibus," these are, I believe, the only occasions on which the "fagus" is mentioned by Virgil; nor in any of them is it connected with the neighbourhood of Rome. Still, however, it is remarkable that Virgil should speak of the "fagus" as growing in the plains of Northern Italy, while in the present day we must ascend the adjacent mountains to a considerable height before we find it. The lower limit of the beech on the southern side of the Alps (in the Valtelline and the Veronese) is placed by Schouw (*Pflanzen-Geographie*, p. 199.) at 2000 Parisian feet above the level of the sea, while the altitude of the plain of Lombardy is only 400. It would appear therefore that in Northern Italy the lower limit of the beech has ascended 1600 feet since the time of Virgil.

The position of Virgil's "fagi" in the plain near the foot of the hills, is in accordance with a passage of Pliny, where he puts the fagus among the mountain trees, that also descend into the plain. (*Hist. Nat.* xvi. 30.)

In another passage (xvi. 15.) Pliny states that fagi formerly existed on the site of the temple of Jupiter Fagutalis within the precincts of the city of Rome. Brocchi, in his Physical Map of Rome, at the earliest period of its existence, lays down the Lucus Fagutalis on the Esquiline, not far from the Agger of Servius Tullius. The mean height of the Esquiline above the Mediterranean does not exceed 200 Parisian feet, and the highest point of the Agger only reaches 237, accord-

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[* See L. & E. Phil. Mag. vol. iv. p. 276.—EDIT.]

ing to the measures of Schouw (Brocchi *Suolo di Roma*, p. 211.): consequently this grove of beeches must have existed at a height of little more than 200 feet above the sea.

I am aware that it is denied by some that the Fagus of Virgil is our beech: but even those persons admit that the Fagus of Pliny is the beech; and indeed in the description of that author (*Hist. Nat.* xvi. 7.), the triangular coat of the gland, the smoothness of the leaf, and the care with which the fagus, though bearing an edible and sweet gland, is distinguished from the trees bearing real "glandes," that is acorns, seems pretty decisive on this point.

The only argument urged (Gardener's Magazine for January, 1839, pages 10 and 19.) against the Fagus of *Virgil** being the beech (beyond the fact now under discussion of its not being found in the plains of modern Italy), is founded on an erroneous reading of a passage in the Georgics, according to which Virgil is made to speak of engrafting the fagus on the castanea or chestnut. But in the text of the best modern editions, Virgil says no such thing. The passage (Georgic. ii. 69.) stands in Heyne thus:

> "Inseritur vero et fœtu nucis arbutus horrida;
> Et steriles platani malos gessere valentes;
> *Castaneæ fagus, ornusque incanuit albo*
> *Flore pyri*........................"

It is evident that the poet speaks of engrafting the chestnut on the beech, and not the beech on the chestnut. Whether such a graft be really possible, I do not know; but it is not more out of the way than the other operations of a similar kind alluded to in this passage.

The statements of Pliny regarding the existence of the beech in the neighbourhood of Rome, are remarkably confirmed by Theophrastus, who speaking of the plain of Latium, says (*Hist. Plant.* iii. 10.), *ἡ μὲν πεδινὴ* (*τῶν Λατίνων*) *δάφνην ἔχει καὶ μυῤῥίνην καὶ ὀξύην θαυμαστήν.* The *ὀξύα* of Theophrastus appears clearly from his description to have been the beech; and *ὀξύα* is still the name of that tree in modern Greece, according to Sibthorp. (Schneider *in Theophrast.*, vol. v. *Indic. in voce ὀξύα.*) That the plain of Rome should have produced, together with laurels and myrtles, beeches of wonderful size, is an extraordinary botanical fact, and one of the highest importance for the purposes of the present inquiry. For if we confine ourselves to the circumstances connected with the vegetation of the beech, we should be led to fix the mean temperature of ancient Rome at 3° or 4° centigrade lower than at present *; a temperature by no means suitable to the laurels and myrtles, and equally at variance with other phænomena, as I shall proceed to show.

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* I say Virgil, for the most serious difficulty about the meaning of the word "Fagus" arises from a passage of Cæsar (*Bell. Gall.* v. 12.), who speaks of this tree as not found in Britain. I may observe, that it is not more extraordinary that Cæsar should assure us that the beech was not found in Britain, than that Herodotus (ii. 77.) should say that the vine was not grown in Egypt, where, however, existing monuments, (Rosellini, *Mon. Civil.*, t. ii. p. 366.) and various passages of the Pentateuch (Genesis, xl. 9. Numbers, xx. 5.), show it to have been cultivated from the earliest period. But I have only to do at this moment with the "Fagus" of Virgil and Pliny. As to the latter, it is true that the passage, "dulcissima omnium fagi," (*Hist. Nat.* xvi. 8.) appears to be a translation of *γλυκύτατα δὲ τὰ τῆς Φηγοῦ* (*Hist. Plant.* iii. 8.); but on the other hand, while Theophrastus (*Hist. Plant.* iv. 12.) speaks of the *Φηγὸς* on the tomb of Ilus, Pliny (*Hist. Nat.* xvi. 8.) uses the term "Quercus," which shows that this latter careless and inconsistent writer did not always confound the Greek *Φηγὸς* with the Latin "Fagus."

It is important to be noticed, as bearing on the question of temperature, that Pliny (xvi. 59.) asserts that the chestnut would hardly grow in the immediate vicinity of Rome: "Juxta Romam ipsam castaneæ cerasique ægrè proveniunt,"—doubtless on account of the heat. In the present day the region of the chestnut in southern Italy is placed by Tenore (p. 58.) at from 150 to 400 toises above the level of the sea. In the Abruzzi, the same region is placed by Schouw (*Pflanzen-Geographie*, p. 475.) at from 200 to 600 toises. Now the highest of the seven hills of Rome does not reach 50 toises; and Monte Mario, the most conspicuous summit in the neighbourhood, only attains 74. (Brocchi *Suolo di Roma*, p. 213.) Consequently Rome does not lie within the existing region of the chestnut, and that tree, if planted near it, would probably grow but "ægrè."

That the climate of Italy was not very much colder in ancient times than at present, may, I think, also be inferred from the fact of the date-palm growing there (in the cultivated state), though then, as now, it remained sterile. (Pliny, xiii. 6.) In Italy at present, if we except the garden of a convent at Rome (a very sheltered spot), and also a small tract of coast between Nice and Genoa, which from local causes enjoys a very high temperature, the northern limit of the date is at Terracina. Now the mean temperature of Rome is 15°·5 C., of Naples 17° C. Terracina, lying nearly half-way between them, certainly has more than 16° mean temperature. At Nice the temperature is 15°·5; at Genoa 15°·9. At neither of these places will the date-tree grow; but at Bordighiera, which lies between them, in a very warm exposure, it is cultivated rather extensively. We must then put the mean temperature of Bordighiera above 16°, and probably it is above that of Toulon, viz. 16°·6. From these circumstances it evidently results that the cultivation of the date-palm in Italy requires a mean annual heat of from 16° to 17° C., and probably nearer the latter. Now the temperature of the southernmost provinces of Italy is not in all likelihood above 17°·5, the temperature of Palermo; and consequently had the climate been two centesimal degrees lower in the days of Pliny, the date-tree could not have been cultivated in Italy; certainly not, as he tells us, "vulgò *."

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* The lower limit of the beech appears to have ascended from less than 50 to 400 toises above the level of the sea. We may assume, approximately, a decrement of 1° C. in the mean temperature for every 100 toises of elevation.

I have argued on the supposition that the words "sunt quidem et in Europâ, vulgòque Italiâ, sed steriles," refer to the date-palm: I think he cannot mean the palmetto, Chamærops humilis, as he adds, "Nulla est in Italiâ sponte genita;" and it is difficult to suppose that the Chamærops could have been a cultivated plant: yet even in this, as it seems to me, very improbable supposition, the argument would not be affected, since the limit of the Chamærops is very nearly that of the date-tree. If we except the tract of coast at Bordighiera, before spoken of, the northern limit of the palmetto in Italy is at Cape Circeii near Terracina †. (Tenore, *Cenno*, p. 68.)

It is observed by Arago, that Virgil speaks somewhere of the rivers freezing in Calabria. He alludes, I presume, to a passage in the fourth Georgic (l. 125, &c.), in which the poet speaks of the success of a gardener in the neighbourhood of Tarentum ‡.

> "Et cum tristis hyems etiam nunc frigore saxa
> Rumperet, et glacie cursus frænaret aquarum,
> Ille comam mollis jam tondebat hyacinthi."——

These are evidently merely poetical exaggerations. Horace, writing about the same time, praises Tarentum for the mildness of its winters. (*Od.* ii. 6.)

> "Ver ubi longum *tepidasque* præbet
> Jupiter *brumas*:"

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* I am surprised that the learned and accurate Ritter (*Asien.* vol. iv. lib. 3. §. 99.) should think that the date-tree was introduced into Spain by monks from Egypt. "Ferunt (palmæ) et in maritimis Hispaniæ fructum, verùm immitem." Plin. xiii. 6.

† It may be observed in passing, that the Chamærops was abundant in Sicily in ancient times as it is now. (Theophrast. *Hist. Plant.* ii. 6. Plin. *Hist. Nat.* xiii. 9.) This fact alone proves that the temperature of Sicily was not 2° C. lower than at present.

‡ The Calabria of the ancients, it will be recollected, was not the province now called by that name; it comprised the peninsula which is cut off by a line from Brundusium to Tarentum.

and what is more precise, boasts of the excellence of its olives. Now this tree is too delicate to endure such winters as those described by Virgil; it will not even bear those of northern Italy, where, however, the freezing of the rivers is a rare occurrence. The conditions laid down by Humboldt for the successful cultivation of the olive are a mean annual temperature from 14°·5 to 17° C., a mean temperature of the coldest month not below 5° or 6° C., and of the whole summer from 22° to 23° C.

II. *Palestine, Egypt, the southern limit of the Vine.*

An ingenious argument has been founded upon the simultaneous cultivation of the date-tree and the vine in Palestine at an ancient epoch. The date-tree, it is argued, requires a mean temperature of at least 21° C. to ripen its fruit; and on the other hand, the vine it is said cannot be cultivated for the purpose of making wine at a higher mean temperature than 22° C. It is concluded that the mean temperature of ancient Judæa was between 21° and 22°; and it is said that this appears to be about its temperature at the present day. One part of these propositions I think open to doubt. Schouw, it is true, agrees with von Buch in placing the southern limit of the vine (at the level of the sea) in the island of Ferro, which has probably from 21° to 22° mean temperature. He states that the vine succeeds only in the northern portions of Barbary; that the cultivation of it in Egypt is insignificant; that at Bushire, on the Persian Gulf, it can only be cultivated in deep ditches to protect it from the sun. Now the mean temperature of Bushire is stated by Humboldt at 25°·5; this example, therefore, proves nothing. As to Barbary and Egypt, Mahometan prejudices prevent the making of wine in those countries at the present day; but we know that in ancient Egypt the vine was cultivated on a great scale, and many excellent wines produced. Nor was this by any means confined to Lower Egypt; on the contrary, excellent wine was made in the Thebaid, and particularly at Koptos, in 26° north latitude[*], and even in the Great Oasis, still further south[†]. Theophrastus, as Arago has already observed, speaks of the vine reaching as far as Elephantine[‡]. But I will be contented with the southernmost limit, where we know good wine to have been made, viz. the Oasis. Now the mean temperature of Cairo is 22°·5 C.[*] The Oasis lies from 4° to 6° south. Let us take as a mean 5°. The increase of mean temperature corresponding to 5° of latitude is, as I shall presently show, for these countries, not less than 1½° C. This makes the mean temperature of the Oasis 24°; and unless we believe the climate of ancient Egypt to have been cooler than at present, we must admit that good wine may be made under that temperature.

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[*] Athenæus, lib. i. *ἡ δὲ περὶ τὸν Νεῖλον ἄμπελος, πλείστη μὲν αὐτὴ ὅσος καὶ ὁ ποταμός· καὶ πολλαὶ τῶν οἴνων αἱ ἰδιότητες κατά τε τὰ χρώματα καὶ τὴν προσφοράν......ὁ δὲ κατὰ τὴν Θηβαΐδα, καὶ μάλιστα ὁ κατὰ τὴν Κόπτον πόλιν, οὕτως ἐστὶ λεπτὸς, καὶ εὐανάδοτος, καὶ ταχέως πεπτικὸς, ὡς τοῖς πυρεταίνουσι διδόμενος μὴ βλάπτειν.*

[†] Strab. xviii. 1.

[‡] *Hist. Plant.* i. 3.

The vine, in fact, is less impatient of heat than might be supposed from the circumstances said to attend its cultivation at Bushire. Superb grapes are grown at Cawnpore in British India in about 26½° latitude, at an elevation of not more than 1000 or 1200 feet above the sea[†]. Humboldt informs us that excellent grapes are found at Cumana in 10½° latitude with a mean temperature of 27°·7 C[‡]. It must be recollected that the heat of the Persian Gulf is excessive. The mean temperature of the month of July is 34° C., and the thermometer at noon rises even to 44° C.[§], a degree of heat surpassed at few spots on the surface of our globe. To find higher temperatures than this we must go to Nubia, where Caillaud frequently observed 48°[||], and the Oasis of Mourzouk, where Ritchie and Lyon experienced even the extraordinary heat of 53°·8 C.; the thermometer rising daily for whole months to from 46° to 52°[¶].

To find the increment of mean temperature from Cairo to the Oasis, I have calculated two years' observations made by Browne in the Oasis of Darfûr. We know so little of the interior of Africa, that these observations possess a good deal of interest. They were made partly at Cobbé in latitude 14° 11′, and partly at a place called El Fasher, which seems to have been in the neighbourhood, but which I do not find on Browne's map. The hours of observation were 7 a.m. and 3 p.m., a few days only are wanting to complete two years. I find the following monthly means: —

January ...	18°·5 C.	July	30°·6 C.
February...	19·9	August......	32·0
March......	27·2	September	33·1
April	30·1	October ...	28·9
May.........	29·7	November ..	25·6
June.........	29·4	December ..	23·4

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[*] Humboldt, *Lignes Isothermes. Mém. d'Arcueil*, vol. iii. Tableau.

[†] *Nouvelles Annales des Voyages*, vol. xxx. p. 317.

[‡] Quoted by Schouw, *Pflanzen-Geographie*, p. 209.

[§] *Voyage aux Régions Equinoxiales*, vol. xii. p. 208. 8vo edit.

[||] *Voyage à Méroé et au Fleuve Blanc.*

[¶] Lyon's Travels, Meteorological Register.

and hence the mean of the whole year = 27°·4 C. Comparing this mean temperature with that of Cairo, in latitude 30°, the increase for five degrees of latitude is $1\frac{1}{2}$° C., as assumed before.

Probably the number 27°·4 is rather too high, as the hours of observation selected would lead to a result erring in excess. But on the other hand it is to be remarked, that Darfûr comes within the limits of the tropical rains, which in this part of Africa reach as far as 17° north, and tend to keep down very much the heats of summer. In fact, Browne never marks the thermometer above 38°·3 in Darfûr, while he mentions having observed 46°·7 in the Great Oasis of Egypt. It is in those portions of the torrid African zone, where rain never or rarely falls, as in Upper Egypt, Nubia, and Fezzan, that the heat of the day is most excessive. It is true that the nocturnal radiation is also very great [*], which keeps down the mean temperature of the twenty-four hours; but during certain hours the vine would be probably exposed to greater heat in the Egyptian Oasis than in Darfûr itself.

III. *Cultivation of the Vine in Britain.*

I think it is difficult not to feel very sceptical about any extensive cultivation of the vine in ancient times in the northernmost provinces of France, and in England. Strabo, as M. Arago has remarked, indicates the line of the Cevennes as the limit of the culture of the olive in France, which it still is; but the remainder of the sentence is, I think, equally remarkable: *ἡ ἄμπελος δὲ προϊοῦσιν οὐ ῥᾳδίως τελεσφορεῖ*[†], "as one goes further into the country, the vine does not easily bring its fruit to maturity." As to Britain, we are told expressly by Tacitus, the climate was not warm enough for the vine [‡].

The scanty notices of the climate of Great Britain to be found in ancient authors agree remarkably with what we experience at the present day. Thus Cæsar says, "Loca sunt temperatiora quam in Galliâ, remissioribus frigoribus." (*Bell. Gallic.* lib. v. c. 12.) Thus Strabo: *ἔπομβροι δ' εἰσὶν οἱ ἀέρες μᾶλλον ἢ νιφετώδεις, ἐν δὲ ταῖς αἰθρίαις ὁμίχλη κατέχει πολὺν* χρόνον. (Lib. iv. c. 5.) Thus, too, Tacitus: "Cœlum crebris imbribus ac nebulis fœdum; asperitas frigorum abest...... (fruges) tardè mitescunt, citò proveniunt: eadem utriusque rei causa, multus humor terrarum cœlique." (*Agricol.* c. 12.) And Minucius Felix, in a passage (§ 18.), for which I am indebted to the *Archæologia*, vol. iii. p. 54: "Britannia sole deficitur, sed circumfluentis maris tepore reficitur."

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[*] Caillaud speaks of water freezing at night on his journey between the Great and Little Oasis: at 7 a.m. the thermometer stood at + 2°, at noon at 19°. Capt. Lyon (p. 256.) speaks of ice half an inch in thickness in the plains of Fezzan, south of Mourzouk. After these examples, it is difficult to understand why the thin cake of ice observed by Oudney and Clapperton on their road from Bournou to Sockatoo should have excited so much surprise.

[†] Strab. iv. 1.

[‡] Agricola, c. 12.

Several authors have quoted a passage from Vopiscus*, stating that the Emperor Probus gave permission to the Spaniards, Gauls and *Britons* to plant vines; which permission to the Britons, it is said, would have been a derision, had the climate been too cold for the cultivation of that plant. The first objection that arises, is that Eutropius†, an earlier historian, and Aurelius Victor, in speaking of the same circumstance, mention Gaul and Pannonia, but not Britain. Let us assume, however, the statement of Vopiscus to be correct. Why, it may be asked, was the Imperial permission necessary for this cultivation? To this it is replied, that Domitian, according to Suetonius‡, had forbidden the laying out of new vineyards in Italy, and ordered the destruction to the extent of at least one half of those existing in the provinces. Unfortunately, Suetonius adds, "nec exsequi rem perseveravit." Waving, however, this second difficulty, let us suppose, for the sake of argument, that the edict of Domitian was enforced.

Now as Tacitus, who wrote after Domitian, expressly denies Britain the vine, the olive, "ceteraque calidioribus terris oriri sueta," it follows that the vine could not have been cultivated in that island before the time of Probus, and that consequently (unless the Britons are introduced by a mistake of Vopiscus) the permission of that emperor could only have been a permission to make the experiment of the culture.

I believe the real explanation of the cause of the edict of Probus is to be found in a passage of Cicero de Republicâ, which shows that the prohibition to plant the vine in the provinces was a portion of Roman policy even during the Republic. This passage, which is remarkable in more respects than one, is as follows: "Nos vero, justissimi homines, qui Transalpinas gentes oleam et vitem serere non sinimus, quò pluris sint nostra oliveta, nostræque vineæ." (Lib. iii. c. 13.) The edict of Domitian, and various passages of ancient authors, (among whom I may mention Martial, who speaks of the wines of Marseilles, Vienne in Gaul, and Tarragona,) prove that this system was not uniformly enforced; local exceptions were evidently made; yet enough appears to explain the value and the cause of the permission of Probus.

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* In Probo, c. 18.

† ix. c. 17. "Vineas etiam Gallos et Pannonios habere permisit."

‡ Domitian. c. 7.

The first positive authority for the cultivation of the vine in Britain is Bede, who says, "Vineas etiam quibusdam in locis germinans*." It is important here to observe the "quibusdam in locis." Setting aside vague traditions, the next authentic testimony is that of Domesday Book, which mentions vineyards in several places. At Rayleigh in Essex, we are told "there is one park and six arpennis of vineyard, which, if it takes well, yields twenty modii of wine." (Camden's Essex.) But the very indication of a few vineyards here and there excludes the idea of any extensive cultivation, such as takes place in really wine-growing countries. At a subsequent period, many authorities, for which I may refer to the *Archæologia*, vols. i. and iii., and Miller's Gardener's Dictionary, article *Vitis*, prove the existence of vineyards in particular spots, and generally in connexion with cathedrals or religious houses. What was the success of these attempts of the monks to make wine, "in commodum et magnum honorem," as an old writer says, of their respective houses, may partly be conjectured from the accounts of a vineyard at Ely given by Miller, where the sale of verjuice forms a considerable portion of the profits of the vineyard †. Only one passage has been quoted that would at all seem to imply an extensive cultivation of the vine in ancient times, and even in that the terms are too vague to allow of any positive conclusion. William of Malmesbury (quoted by Camden) boasts of the superiority of the vineyards of Gloucestershire: "Vinearum frequentiâ densior, proventu uberior, sapore jucundior, vina etiam ipsa bibentium ora tristi non torquent acedine, quippe quæ parùm debeant Gallicis dulcedine." This passage does not imply much, it may be observed in passing, for the English wine in general. As to what he says about Gloucestershire, it is to be considered that he seems to be an inaccurate writer, and disposed to exaggeration. Daines Barrington has noticed that he speaks of the bore in the Severn as a daily occurrence, while it happens only at the equinox; and describes it in very exaggerated terms, as capable of sinking a ship, though in reality not formidable to a cock-boat.

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* Hist. Ecclesiast., i. 1. The supposition of Daines Barrington, that in this and other passages "vineæ" refers to orchards of apple-trees and currant-gardens, is too improbable and unsupported to deserve serious refutation.

† In the 12th Edw. II. the wine from the vineyard at Ely sold for 1*l.* 12*s.*, the verjuice for 1*l.* 7*s.* In 9th Edw. IV. no wine, only verjuice was made.

I repeat, that to prove a change of climate it is necessary to show, not merely the existence of vineyards in a few localities, but the extensive growth of the grape for the purpose of making wine. In fact, Plot* tells us, that in the year 1685, Dr. Bathurst, President of Trinity College, made as good claret at Oxford, "in a very mean year for that purpose," as any one could wish to drink; and Pepys says, that in the reign of Charles II. very good wine was made at Walthamstow. As far then as vineyards in particular localities prove anything, the climate of Britain has been constant from the time of Bede to the year 1685. Nor has it degenerated since; for Miller gives a list of places at which wine has been made in the course of the last century; among which are Rotherhithe, Brompton, Kensington, Hammersmith, Walham Green (wine was made at this place for 30 years), Arundel, and Pain's Hill, near Cobham. The wines of many of these places are described as being equal or superior to the French wines of the second class. That made by Mr. Hamilton at Pain's Hill is said to have been fully equal to the best Champagne, and to have sold for fifty guineas a hogshead.

While on the one hand there is no sufficient testimony in favour of the growth of wine on a large scale in ancient times, there is on the other some direct testimony against it. Petrarch, according to Miller, speaks of the people in England as not drinking wine; and Daines Barrington has quoted Lord Bacon†, who says that grapes require a south wall to ripen.

All these considerations make it difficult to admit, with Arago, that the climate of Britain was warmer formerly than at the present time. This idea rests solely on the cultivation of the vine in this island; a fact which cannot be disputed, but does not, I conceive, lead to the inferences that have been drawn from it. The testimony adduced merely indicates a very local and partial cultivation of the plant; such, in fact, as numerous experiments have shown to be practicable in recent times.

R. W. R.

XVI. *Mineralogical Notices. Communicated by* W. H. MILLER, *Esq., Professor of Mineralogy in the University of Cambridge.*

PLUMBIFEROUS ARRAGONITE.

[From Poggendorff's *Annalen*, B. xlviii.]

A VARIETY of arragonite from Tarnowitz, found by Professor Breithaupt to have a specific gravity of 2·995, contains 2·19 per cent. of carbonate of oxide of lead, according to the analysis of Professor Kersten, and 3·859 per cent. according to the analysis of M. Böttger. Hence the proportion of carbonate of oxide of lead appears to be variable, like the proportion of carbonate of strontian in common arragonite, which, according to Stromeyer, varies from 0·5 to 4·01 per cent.

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* Camden, Staffordshire.

† Cent. v. Exp. 430. 432.

ANALYSES OF PETALITE AND SPODUMENE.

[From Poggendorff's *Annalen*, B. xlix.]

Petalite analysed by M. Robert Hagen in the laboratory of Prof. Heinrich Rose, gave the following results:

	(1.)	Oxygen.	(2.)	Oxygen.
Silica	77·812	40·423	77·067	40·036
Alumina...	17·194	8·030	18·000	8·406
Lithia......	2·692	1·484	2·660	1·466
Soda	2·302	0·588	2·273	0·581

Spodumene gave

		Oxygen.
Silica	66·136	34·357
Alumina............	27·024	12·621
Protoxide of iron	0·321	0·098
Lithia...............	3·836	2·126
Soda	2·683	0·686

RESULTS OF ANALYSES OF POONALITE AND THULITE. BY PROF. C. G. GMELIN.

[From Poggendorff's *Annalen*, B. xlix.]

Poonalite.

	Observed.	Oxygen.	Computed.
Silica	45·120	23·44	45·07
Alumina............	30·446	14·22	31·33
Lime	10·197	2·86	10·43
Soda with a trace of potash	0·657	0·11	
Water	13·386	11·90	13·17

The formula to which the computed quantities refer, is

$$3\,\dot{\mathrm{Ca}}\,\dddot{\mathrm{Si}} + 5\,\underline{\dddot{\mathrm{Al}}}\,\dddot{\mathrm{Si}} + 12\,\underline{\dot{\mathrm{H}}}.$$

Thulite.

Silica	42·808	
Alumina........................	31·144	
Lime	18·726	
Soda with a trace of potash	1·891	
Protoxide of iron	2·238	
Protoxide of manganese ...	1·635	
Water..........................	0·640	99·132

The above results agree with the formula

$$(\dot{C}a, \dot{F}e, \dot{M}a)^3 \dddot{Si} + 2 \underline{\dddot{Al}} \dddot{Si},$$

and show that the mineral is chemically the same as Epidote.

RESULTS OF ANALYSIS OF BORACITE FROM LÜNEBURG. BY M. C. RAMMELSBERG.

[From Poggendorff's *Annalen*, vol. xlix.]

	Transparent Crystals.	Opake Crystals.	
	S. G. 2·955 at temp. 12°·5 C.	S. G. 2·938 at temp. 11°·5 C.	According to Arfvedson.*
Magnesia	30·748	31·124	30·3
Boracic acid . .	69·252	68·876	69·7

The quantities of oxygen contained in the magnesia and boracic acid are 12·03 and 47·26 respectively. Hence the composition of boracite is expressed by the formula

$$\dot{Mg}^3 \dddot{B}^4.$$

RESULTS OF ANALYSES OF NOSEAN, HAUYNE, LAZULITE AND ARTIFICIAL ULTRAMARINE, PERFORMED BY M. F. VARRENTRAPP IN THE LABORATORY OF PROF. HEINRICH ROSE.

[From Poggendorff's *Annalen*, vol. xlix.]

Nosean from the Lake of Laach.

		Oxygen.
Alumina	32·566	15·20
Soda	17·837	4·56
Lime	1·115	0·31
Silica	35·993	18·70
Sulphuric acid . . .	9·170	5·49
Iron	0·041	
Chlorine	0·653	
Water	1·847——99·222	

Hauyne from Nieder-Mendig.

		Oxygen.
Soda	9·118	2·33
Lime	12·552	3·52
Alumina	27·415	12·80
Silica	35·012	18·28
Sulphuric acid . . .	12·602	7·23
Sulphur	0·239	
Iron	0·172	
Chlorine	0·581	
Water	0·619——98·340	

[* The details of Arfvedson's analysis will be found in Phil. Mag. First Series, vol. lxii. p. 358.—EDIT.]

Lazulite.

Soda	9·09
Lime	3·52
Alumina	31·76
Silica	45·50
Sulphuric acid	5·89
Sulphur	0·95
Iron	0·86
Chlorine	0·42
Water	0·12
	98·11

Artificial Ultramarine.

Soda	21·476
Potash	1·752
Lime	0·021
Alumina	23·304
Silica	45·604
Sulphuric acid	3·830
Sulphur	1·688
Iron	1·063
Chlorine	a trace

TWO ANALYSES OF GMELINITE FROM GLENARM, SP. GR. 2·06 AT 12° C. BY M. CARL RAMMELSBERG.

[From Poggendorff's *Annalen*, B. xlix.]

		Oxygen.		Oxygen.
Silica	46·398	24·10	46·564	24·19
Alumina	21·085	9·84	20·186	9·43
Lime	3·672	1·03	3·895	1·09
Soda	7·295	1·86	7·094	1·81
Potash	1·604	0·27	1·873	0·31
Water	20·412	18·14	20·412	18·14

Hence the quantities of oxygen contained in the alkaline bases, including the lime, the alumina, the water and the silica, are as the numbers 1, 3, 6, 8, as in most Chabasies. The chemical distinction between the two minerals appears to consist in the relative quantities of lime and soda contained in them, most lime being contained in Chabasie. Gmelinite forms a jelly with hydrochloric acid, Chabasie does not.

XVII. *Observations on* Elæolith *and* Nepheline. *By* THEODORE SCHEERER.*

IN the 46th volume of Poggendorff's *Annalen*, p. 291, I communicated a notice on an elæolith which is found near Brevig in Norway. I considered myself justified, from the results of the analyses made with it, in advancing a new formula for this mineral, which seems to be more in harmony with its composition than the one hitherto employed. I had then no elæoliths from other localities at my disposal with which to test the truth of my assertion, and I was consequently obliged for the moment to lay all further inquiry aside. Subsequently, however, I re-commenced these investigations, partly in Norway and then in Berlin, in the laboratory of Professor Heinrich Rose, and with the kind assistance of Mr. W. Francis, I have performed a new series of analyses of elæoliths and nephelines. The results which I have obtained, as well as some other observations, I will communicate in the present paper, and at the same time I will add for comparison the analyses of these minerals, which had been previously made by other chemists.

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* Communicated by the Author.

I. ANALYSES OF VARIOUS ELÆOLITHS.

1. *Brown Elæolith from Brevig in Norway.*—I have already communicated the composition of this elæolith in the memoir above-mentioned, but for the sake of completeness I will repeat it here. It was found, from three analyses, to consist of

	1.	2.	3.
Silica	44·59	44·48	44·30
Alumina	32·14	32·03	31·60
Peroxide of iron .	0·86	1·30	1·16
Lime	0·28	0·24	0·32
Soda	15·67	15·76	20·45
Potash	5·10	5·24	
Water	2·05	2·06	2·10
	100·69	101·11	99·93

The specific gravity of this elæolith is 2·617. It occurs in smaller or larger masses in a snow-white granular albite, and is in general so much impregnated with it that it is exceedingly difficult to obtain pure fragments for analysis. Hence on dissolving the silica in carbonate of soda some powdered albite always remains. This latter mineral being scarcely in the slightest degree attacked by acids, I do not believe that it has had any sensible effect on the analyses.

2. *Green Elæolith from Fredriksvärn in Norway.*—This species of elæolith occurs, like the following, in the well-known zirconiferous syenite of Fredriksvärn. Its specific gravity was found to be 2·61. Klaproth* was the first who undertook an analysis of this mineral, and obtained the following results:—

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* Klaproth's Contributions, vol. v. p. 170.

Silica	46·50
Alumina	30·25
Peroxide of iron	1·00
Lime	0·75
Potash	18·00
Water	2·00
	98·50

After Klaproth, C. G. Gmelin* analysed the green elæolith, and found it to be composed of

Silica	44·19
Alumina	34·42
Peroxide of iron	0·65
Lime	0·52
Soda	16·88
Potash	4·73
Magnesia and manganese	0·69
Water	0·60
	102·68

The results of two analyses performed by me, were—

	4.	5.
Silica	45·31	45·15
Alumina	32·63	32·70
Peroxide of iron	0·45	0·67
Lime	0·33	0·34
Soda	15·95	15·48
Potash	5·45	5·88
Water	0·60	0·63
	100·72	100·85

Between these analyses and that of Klaproth there is very little agreement. Probably either the silica which Klaproth obtained still contained alumina, or the elæolith employed by him was impregnated with foreign minerals, insoluble in acids, which must have increased the quantity of silica, as this was not tested as to its purity. This analysis becomes still more suspicious, when we consider that elæolith not only contains potash, but also soda. From 100 grains of powder Klaproth obtained 27·5 grains of chloride of an alkali, which he took for pure chloride of potassium, and calculated as such. But if we bear in mind that it was composed of the two alkalies in a proportion corresponding to the quantities of potash and soda

* Schweigger's Journal, vol. vi. p. 82.

in my analyses, we then obtain only about 11 per cent. soda and 4 per cent. potash, which gives a loss of 4·5 for the whole. Between C. G. Gmelin's analyses and mine there prevails a satisfactory coincidence; only the silica of the former should perhaps be somewhat greater, as the small quantities of it which constantly accompany the other constituents were not separated and added to the main quantity. That M. Gmelin did not redissolve the silica in carbonate of soda can scarcely have given rise to any inaccuracy, as on employing good pieces of this elæolith the silica is always found free from foreign ingredients. Likewise in the amount of alkali this analysis differs slightly from mine, but this undoubtedly arises from the method employed by him being less perfect than the one now in use for the determination of potash and soda. The *sums* of the chlorides agree on the other hand perfectly; Gmelin obtained 39·1 per cent, and I very nearly 39·0, which proves that only the relative quantities of potash and soda were somewhat inaccurately determined in Gmelin's analysis.

3. *Brown Elæolith from the same locality.*—This variety has not, as far as I am aware, been hitherto analysed, as it was always considered to be a modification of the green elæolith, which is fully confirmed by the following analyses. I am indebted to the kind aid of Mr. W. Francis for analysis 6, made in the laboratory of Professor H. Rose:—

	6.	7.
Silica	45·51	45·55
Alumina	33·53	32·00
Peroxide of iron		1·41
Lime	0·81	trace.
Soda	15·86	16·09
Potash	4·50	5·02
Water		0·78
	100·21	100·85

The specific gravity was found to be 2·61.

4. *White Elæolith from the Ilmen Mountains in Siberia.*—The occurrence of this mineral has been described by Professor Gustav. Rose*.

The Ilmen mountains consist for the greatest part of a rock of white felspar, black uniaxal mica, and nepheline, thus exhibiting, but far more so from the other minerals it contains, such as zircon, pyrochlor, &c., great similarity to the zirconiferous syenite of Fredriksvärn. It must be considered, as it were, as a granite in which the quartz is replaced by elæolith. On account of its evident peculiarity and its large distribution, Professor G. Rose has proposed for it the name miascite (from the river Mias in the neighbourhood of its occurrence). Its specific gravity I found to be 2·60.

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* *Pogg. Annalen*, vol. xlvii. p. 377.

Two analyses of this mineral have recently been published by M. Bromeis*, who obtained the following results:—

Silica	42·51	42·33
Alumina	33·73	34·39
Peroxide of iron	trace	trace
Lime	0·20	0·47
Soda	14·01	16·26
Potash	6·91	5·95
Magnesia	0·77	0·45
Water	0·92	0·92
	99·05	100·77

Mr. Francis and I have likewise examined this mineral. The result 8 is that which Mr. Francis obtained; the result 9 that which I obtained by analysis:—

	8.	9.
Silica	44·30	44·07
Alumina	33·25	33·12
Peroxide of iron	0·82	0·57
Lime	0·32	0·26
Soda	16·02	15·70
Potash	5·82	5·69
Magnesia	0·07	trace
Water	0·00	0·90
	100·60	100·31

The results of the two last-mentioned analyses differ from those of M. Bromeis, particularly in the amount of silica. I shall subsequently return to this point.

5. *Nepheline from Monte Somma* (Vesuvius).—This mineral occurs very differently from the preceding ones, it always being found crystallized in 6-sided prisms with terminal faces perpendicular to the axis, combined with one or two hexagonal pyramids. It is situated in cavities of the dolomite, accompanied by garnet, vesuvian, anorthite, glassy felspar, &c. All these minerals present exactly the appearance of

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* *Pogg. Annalen*, vol. xlviii. p. 577.

having been sublimed into the fissures and cracks of the dolomite. The specific gravity of this nepheline I found to be 2·56. It has been analysed by Arfvedson, who found its composition as follows:—

Alumina (with some iron and manganese)	33·73
Soda	20·46
Water	0·62
Silica	44·11
	98·92

As it is very difficult to obtain the crystals of nepheline quite pure, without portions of the other minerals with which it is accompanied being mixed with them, I had the precaution to take a separate quantity of the crystals for each analysis without previously pulverizing the whole together. Now, if the result of the analysis was influenced by foreign minerals, this influence must at least vary in the different analyses, and consequently produce discordant results. Two analyses gave

	10.	11.
Silica	44·03	44·29
Alumina	33·28	33·04
Peroxide of iron	0·65	0·39
Lime	1·77	1·82
Soda	15·44	14·93
Potash	4·94	4·72
Water	0·21	0·21
	100·32	99·40

The amount of water, from want of material, was determined only in one experiment. In both analyses, notwithstanding the great precaution taken in choosing the fragments of the crystals, it was impossible to prevent a quantity of foreign minerals getting into the analysis, which was, however, not decomposable by hydrochloric acid. In the first analysis this mixture amounted to about 3 per cent; in the second to about 5 per cent. As from this cause, notwithstanding the near agreement of the results, doubts might arise as to their perfect correctness, I analysed a third portion of nepheline. Through the great kindness of Prof. G. Rose I received from him for this purpose a very fine crystalline geode in pure dolomite, containing crystals perfectly free from all foreign minerals, with a portion of vesuvian which was easily removable. I found this to be composed of

	12.
Silica	44·04
Alumina	34·06
Peroxide of iron	0·44
Lime	2·01
Potash	4·52
Soda	15·91
Magnesia	trace
Water	0·21
	101·19

The silica, on dissolving it in carbonate of soda, was found to be so pure that it only left 0·004 gr. insoluble matter. The amount of water was adopted from the first determination. The agreement of this analysis with the two former, sets it beyond all doubt that the foreign minerals which were present in both the former had not exercised any influence on the results. The quantity of alumina in analysis 12 seems, however, from some circumstance or other, to be stated too high, as it is about 1 per cent. greater than in the analyses 10 and 11, and the analysis gives 1 per cent. overplus. However this may be, it results from the three inquiries, 1st, that the nepheline contains a larger quantity of lime than elæolith; 2ndly, that soda and potash are contained in both. In these two points my analyses differ entirely from those of Arfvedson. He mentions, it is true, having found some lime, but that the quantity was too small for accurate determination. It may therefore be the case that all nephelines do not contain this quantity of lime, amounting to 2 per cent., if the reason of Arfvedson's not finding so considerable a quantity of lime is not to be sought for in the method he employed; viz. instead of adding oxalate of ammonia immediately to the liquid filtered from the alumina, first neutralizing it with hydrochloric acid. The point of neutralization might here be easily exceeded, and some oxalate of lime remain dissolved in the fluid. That Arfvedson found no potash, probably arises from his using for its detection a far more imperfect reagent than chloride of platinum, viz. tartaric acid.

6. *White Elæolith from Katzenbuchel in the Odenwalde.*—This variety is found in isolated crystals in dolerite (nepheline rock), which breaks through the sandstone of the Katzenbuchel. Prof. L. Gmelin has given an analysis of it in a paper by him and Prof. Leonhardt, which I will here cite. It gave

Silica	43·36
Alumina	33·49
Peroxide of iron (with some manganese)	1·50
Lime	0·90
Soda	13·36
Potash	7·13
Water	1·39
	101·13

I likewise have analysed a portion of this variety. It was however impossible, notwithstanding every precaution, to obtain perfectly pure crystals, as the very crystals inclosed minute parts of the rock in their interior. Their nucleus is frequently the most impure, often exhibiting a 6- or 4-sided opacity, which is environed by a more transparent frame. I found it to be composed of

	13.
Silica	43·70
Alumina	32·31
Peroxide of iron	1·07
Lime	0·84
Soda	15·83
Potash	5·60
Water	1·39
	100·74

The amount of water is here adopted from Prof. Gmelin's determination, as a sufficient quantity of pure mineral was not at my disposal. The silica separated by hydrochloric acid left about 2·5 per cent. residue, insoluble in carbonate of soda, which evidently consisted of minute parts of the rock. A considerable difference between M. Gmelin's analysis and mine will be found solely in the relative quantities of potash and soda. The source of this is without doubt to be sought in the mode of determining these two alkalies employed by L. Gmelin. He weighed the heated chlorides and determined by a solution of silver the quantity of chlorine contained in them, and thence calculated the requisite quantities of chloride of potassium and chloride of sodium which must be present to give this quantity of chlorine in the mixture of the two salts. On closer consideration, it is evident that every error of weight is greatly increased by calculation, as a small defect in the quantity of chloride of silver corresponds to a considerable difference in the relative quantity of potash and soda.

II. QUANTITIES OF HYDROCHLORIC AND SULPHURIC ACID IN ELÆOLITHS.

In the preceding analyses I have omitted two constituents which well deserve a separate consideration: these are small quantities of hydrochloric and sulphuric acids which occur in these minerals. M. Bromeis first drew attention to this subject, and I have fully confirmed his observations. To test the various species of elæolith as to the quantities of these acids, I always employed distinct quantities, which, powdered finely, were decomposed by chemically pure nitric acid. The muriatic acid was then determined by a solution of silver, and the sulphuric acid by chloride of barium. M. Bromeis found in the white elæolith from the Ilmen mountains 0·04 per cent. hydrochloric acid. I obtained the following results:—

	Hydrochloric acid.	Sulphuric acid.	
Green elæolith from Arendahl	trace	trace	2 exp.
Brown elæolith from the same locality	trace	——	
White elæolith from the Ilmen mountains	0·06	0·07	
Nepheline from Monte Somma	0·22	0·10	1 exp.
The same	0·05	trace	2 exp.

The first and second experiment with the nepheline differ very much, which probably arises from a quantity of those crystals being taken which were employed for the analyses 10 and 11, containing foreign minerals which, as for instance davyne, might easily increase the amount of the acids. As the quantities are so small, I will not vouch for perfect accuracy in the quantities stated[*]; thus much is, however, certain, that they exist in the elæoliths examined. They are not expelled by exposing the mineral to a red heat, but may be detected in the mass after heating, which is as easily decomposed by acids as before. Nor can they be extracted from the finely powdered mineral by boiling in water; it hence results that they cannot be simply combined with an earth or an alkali, but form an essential mineral combination, probably replacing a small quantity of silica. At first I suspected that the portion of silica, always remaining undissolved on decomposing the mineral with nitric acid, might possibly produce opacity with a solution of silver; but this cannot be the case, as the opacity, with different species of elæolith, under similar circumstances, differed considerably; thus, for instance, with the brown one from Fredriksvärn, there was scarcely a perceptible trace, while the white species from the Ilmen Mountains and the nepheline from Monte Somma, deposit a precipitate after some days' standing. Hydrochloric acid is probably a widely diffused constituent of volcanic minerals; I have, for instance, found distinct traces of it in Analcime.

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[*] Especially as the small quantities of the chloride of silver were always of a blackish colour, and not fusible.

III. CHEMICAL FORMULA FOR THE ELÆOLITHS.

From all the analyses above detailed, it results, to a certainty, that both elæolith, as well as nepheline, must be considered as composed of 3 atoms silica, 2 atoms alumina, and 2 atoms alkali. The alkali, however, always consists of 4 equivalents of soda to one of potash; calculated accordingly, it should be thus:

Silica	44·67
Alumina	33·12
Soda	16·12
Potash	6·09
	100·00

Here, however, no notice is taken of 1°, the amount of water, 2° the amount of hydrochlorates and sulphates. If we take both into account, we find that the species of elæoliths examined contain about from 0·5 to 2·0 per cent *foreign* constituents, which must be proportionally subtracted from the *essential* constituents. An elæolith would thus be composed of

	0·5 per cent. foreign constituents.	1 per cent. foreign constituents.	2 per cent. foreign constituents.
Silica	44·45	44·23	43·79
Alumina	32·95	32·79	32·46
Soda	16·04	15·95	15·78
Potash	6·06	6·03	5·97
	99·50	99·00	99·00

But according to the view which pre-supposed in elæolith 4 at. silica, 3 at. alumina, and 3 at. alkali, adopting the formula

$$\begin{matrix}Na^3\\Ka^3\end{matrix}\left\{\dddot{Si} + 3\,\underline{\dddot{Al}}\;\dddot{Si},\right.$$

the three corresponding compositions should be

Silica	41·58	41·36	40·94
Alumina	34·67	34·50	34·15
Soda	16·88	16·80	16·63
Potash	6·37	6·34	6·28
	99·50	99·00	98·00

Since all the elæoliths analysed approximate in the quantity of foreign constituents to one of these three cases, a comparison may be easily instituted between the compositions found and the corresponding calculated ones. It will then be seen that none of the analyses approach to the atomic proportion of the old formula, with the exception of M. Bromeis' two analyses; but even these still give 1 per cent silica more than is required by the old formula, although M. Bromeis undoubtedly took the greatest precaution not to find too much silica.

It might still be supposed by those who cling so fast to the sacred number of three in chemical formulæ, that some circumstance or other in the mode of analysis pursued by Mr. Francis and myself may have produced the constant supposed overplus of silica; I will therefore give a sketch of the method we employed. The mineral in powder was triturated in an agate mortar, and dried in a water bath, and was then treated with hydrochloric acid, with constant stirring, till the formation of a perfectly gelatinous mass occurred: this was left for some time, usually a day, in a warm place, and then in some analyses evaporated to dryness; and in others, still containing acid, treated immediately with water. The mass evaporated to dryness was again moistened with acid, and after an hour water was added to it and it was filtered. The silica, after incandescence and weighing, was tested as to its purity by boiling with carbonate of soda. Small quantities of silica were subsequently separated from the alumina, peroxide of iron, and the mixture of chloride of potassium, and chloride of sodium, and were added to the chief quantity. With this method there are four points of doubt, in which it is uncertain whether the amount of silica is influenced. 1. It may be questioned whether, when the mass of decomposed mineral, evaporated to dryness, is moistened with hydrochloric acid, and left to stand in this state for an hour or so, all the hydrochlorate of alumina, which has become basic, is re-dissolved. That this is, however, the case, is evident from there being no perceptible difference in the amount of silica, whether all the acid is expelled or the gelatinous mass, containing immediately acid treated with water. 2. On boiling the silica which had been heated to redness with a concentrated solution of carbonate of soda, there was always left an insoluble residue. Two cases must here be distinguished. This residue either consists of light flocks having the appearance of silica, or of these and a heavier powder of a sandy nature, and which is evidently some foreign mineral not decomposed by the hydrochloric acid; it must, therefore, in perfect accuracy, be subtracted from the quantity employed for the analyses. But the flocks M. Bromeis considers to be alumina, which was still retained in the silica, and was not dissolved on boiling in carbonate of soda. This, however, is not the case; for if these flocks are filtered and dried, they form a powder which, before the blow-pipe with soda, gives a perfectly clear glass. To be more surely convinced of the nature of this residue, I decomposed 10 gr. of the white, and 10 gr. of the green elæolith with hydrochloric acid. In both cases I obtained from the silica sufficient quantities of this residue to be able to examine it more accurately. I found it to contain above $\frac{2}{3}$ silica, and only a little alumina, and a trace of the peroxide of iron; it cannot, therefore, be added to the alumina without committing an error. 3. M. Bromeis conceives that alumina may still be contained in the silica, separated from the heated alumina, by long digestion, in hydrochloric acid. The quantity of it can, however, be but extremely slight, as this residue acted before the blow-pipe as pure silica. If, indeed, which is possible, it should still contain a small quantity of alumina, it is certainly balanced by the trace of silica, which is re-dissolved on boiling with hydrochloric acid; for if the alumina and peroxide of iron, dissolved in hydrochloric acid, be again precipitated by ammoniac, the precipitate again heated and dissolved still gives traces of silica*. 4. Of what does the residue consist which remains on dissolving the chlorides of potassium and sodium? In the first analyses I always added it to the silica; but in so doing I committed an error, as I subsequently found it to consist of alumina, silica, lime, and at times magnesia. The first analyses give, therefore, according to my formula, too much silica. I was deceived by the insolubility of this residue in acids, but afterwards found that it did not give a glass on being treated with soda before the blow-pipe.

In the later analyses I have attended to all these points. The powdered mineral was decomposed with dilute hydrochloric acid, which has the advantage, that the elæolith dissolves in it to a clear fluid, and the particles of foreign undecomposed mineral may easily be detected.

After all the facts quoted, it seems to me fully established that elæolith and nepheline contain 3 at. silica, 2 at. alumina, and 2 at. soda and potash. These elements can in no way be more simply combined than in the formula $\left.\begin{matrix}Na^2\\Ka^2\end{matrix}\right\} \dddot{Si} + 2\,\underline{\dddot{Al}}\,\dddot{Si}$; or, if regard be had to the proportions of potash and soda $(\dot{K}a^2\,\dddot{Si} + 2\,\underline{\dddot{Al}}\,\dddot{Si}) + 4\,(\dot{N}a^2\,\dddot{Si} + 2\,\underline{\dddot{Al}}\,\dddot{Si})$. That in these formulæ the alkali (semi-silicate) is combined with more silica than the alumina ($\frac{1}{3}$ silicate) cannot appear objectionable; it is, indeed, plain that this must always happen, if, on the formation of a mineral, there is not sufficient silica present fully to saturate all the bases; these will then divide the quantity of silica among themselves according to their affinity. Thus, for instance, feldspars, as a constituent part of granite, is always in contact with superfluous silica; potash and alumina which it contains are perfectly saturated with it. But the elæoliths, whether in syenite, miascite, nepheline-rock, albite, &c., do not occur accompanied by *free* quartz. There was, therefore, a want of silica at their formation, and the potash accordingly, on account of its great affinity, took up a greater portion of it than the alumina.

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* That a portion of the silica is re-dissolved, may be explained in the following manner. Silica which, after incandescence, is insoluble in acid, becomes soluble when fused with an alkali. But even this is not requisite; many other of the stronger bases have the property of rendering it *partly* soluble.

The proportion of the oxygen in the alkali, relative to that of the silica, as 2 : 3 is, moreover, not wholly new in mineralogical formulæ; for, according to the investigations of Walmstedt[*], repeated by Regnault[†], prehnite should have the formula $\dot{C}a^2\,\dddot{Si} + \underline{\dddot{Al}}\,\dddot{Si} + \underline{\dot{H}}$; but very probably a similar relation occurs in many other minerals where the analysis does not agree well with the formula. I will not here mention my suspicions, but only give one instance, which speaks evidently in favour of my opinion. This is, the cancrinite, recently analysed by Professor Gustav Rose[‡]. Two analyses gave

Silica . . .	40·59	40·26
Alumina . .	28·29	28·24
Soda . . .	17·38	17·66
Potash . . .	0·57	0·82
Lime . . .	7·06	6·34
Carbonic acid	6·38	6·38
	100·27	99·70

Professor G. Rose advances accordingly the following formula, of which, however, he himself adds that it does not agree well: $\dot{N}a^3\,\dddot{Si} + 3\,\underline{\dddot{Al}}\,\dddot{Si} + \dot{C}a\,\ddot{C}$.

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[*] Berzelius, Jahresbericht, No. V. p. 217.

[†] Ann. des Mines, Ser. III. T. xiv. p. 154.

[‡] See the above-mentioned Memoir on the Ilmengebirge.

The mineral might therefore be regarded as consisting of 1 atom soda elæolith (according to the *older* formula), and of 1 atom calcareous spar, and should have the following composition:

Silica	38·23
Alumina	31·89
Soda	19·41
Lime	5·89
Carbonic acid . .	4·58
	100·00

But if my formula be adopted as the correct one for elæolith, cancrinite would be expressed by the formula $\dot{Na}^2 \dddot{Si} + 2\, \underline{\dddot{Al}}\, \dddot{Si} + \dot{Ca}\, \ddot{C}$, and calculated accordingly, should have the composition:

Silica	39·11
Alumina	28·98
Soda	17·65
Lime	8·03
Carbonic acid . .	6·23
	100·00

A more satisfactory agreement of the formula with the composition found can scarcely be expected in analysis. They appear merely to differ in the formula giving the amount of lime about 1 per cent. higher; but this difference disappears almost entirely if we admit that the potash replaces a portion of the lime. That lime and potash replace one another is evident from the analyses 10, 11, and 12 of nepheline. This latter is, indeed, only essentially different from the other elæoliths by the greater quantity of lime it contains; but it seems that principally the amount of potash has been diminished by its occurrence, while the soda does not at all differ in proportion. Cancrinite, therefore, actually contains 1 at. of nepheline of a composition, such as was formerly adopted for the Vesuvian nepheline, but which has hitherto not been found in an isolated mineral.

On closer consideration, it will be found that the cancrinite affords even a better test for the correctness of my formula than the elæolith itself. The atomic value of the elæolith, according to the older formula (if we add 4 $\dddot{Si}$, 3 $\underline{\dddot{Al}}$, 2·4 $\dot{Na}$, and 0·6 $\dot{Ka}$ together), equals 5529, according to the new one (if 3 $\dddot{Si}$, 2 $\dddot{Al}$, 1·6 $\dot{Na}$, and 0·4 $\dot{Ka}$ are added) = 3877.

Now since the weight of 1 at. of carbonate of lime is 632, it results that considerable differences must originate by these different atomic values on the per centage calculation of the constituents of a combination of both atoms. In fact, were the atomic weight of elæolith = 5529 the cancrinite ought not to contain more than about 10 per cent. carbonate of lime; while, according to my formula, it should contain 14 per cent. But the latter number is likewise confirmed by the two analyses of G. Rose.

IV. COLOUR OF THE ELÆOLITHS.

Only two of the analysed varieties of this mineral possess colour, those from Fredricksvärn, the one green, the other brown. When both are finely powdered and decomposed with concentrated hydrochloric acid, the silica separated has the same colour, only in a less degree. This is especially evident in the green elæolith, which is more intensively coloured than the brown, and with which I therefore chiefly performed experiments. Even when the concentrated hydrochloric acid is evaporated, and the dry mass again moistened with acid, and treated with water, the filtered silica still retains its colour, which, however, immediately disappears on heating the silica. It is further destroyed when the silica is heated with nitric acid, or the mineral decomposed by fuming nitric acid. This latter action sufficiently proves the colour to be of organic origin. But the colouring substance must certainly be of a peculiar nature, as it withstands the action of fuming, nay, even chloriferous hydrochloric acid.

The following are the main results afforded by these examinations of the elæoliths. 1. The formula for elæolith and nepheline must be altered to $\begin{matrix}\dot{\mathrm{N}}\mathrm{a}^2\\ \dot{\mathrm{K}}\mathrm{a}^2\end{matrix}\left\{\dddot{\mathrm{Si}} + 2\,\underline{\dddot{\mathrm{Al}}}\,\dddot{\mathrm{Si}}\right.$. The atomic relation of soda and potash is as 4 : 1. Both minerals are perfectly identical, the latter being merely characterized by a somewhat greater amount of lime.

2. Elæoliths, from the most varied localities, exhibit traces of hydrochloric and sulphuric acids, especially of the former.

3. The amount of water in elæoliths varies considerably, and must be considered as accidental.

It is probably only hygrometric, and is perhaps prevented from evaporating at 80° R. by a force similar to that which several porous bodies exercise on gaseous substances.

4. The colour of some elæoliths is of organic origin.

5. The specific gravity of the elæoliths is exceedingly near to 2·6.

XVIII. *On the Theoretical Constitution of the Compounds of Ammonia.* *By* ROBERT KANE, *M.D., M.R.I.A.*

IN the course of the investigations to which I have subjected the various classes of compounds that ammonia is capable of forming, it has been my lot to submit to the consideration of chemists a great number of theoretical views to which I had been led by my experimental results, and by which I conceived that the mutual connexion of the different classes of ammoniacal compounds could be explained, and their origin and properties accounted for more satisfactorily than it was possible to do by means of the ideas that had been previously received in science. In advancing this new theory of the nature of ammonia and its compounds, I was not so sanguine as to expect that our ideas of a department of chemistry so complex and so important could be immediately or easily modified, or that the adoption of my views could take place without much conflicting reasoning and discussion. In this respect I have had cause to be very much gratified. All principles that can be considered as really vital to my theory have been adopted by the most eminent chemical philosophers, and in place of being dissatisfied that in the collateral parts of the theory some portions have been thought not positively proved, and which have hence been criticized and left for the time aside by Graham and by Rose, I was at once surprised and pleased to see how little had appeared in the eyes of these acute-minded chemists unfit for being at once adopted into science.

I believe, however, that even in those portions of the theory to which Graham and Rose have not acceded, some of the difficulties arise from a want of clearness and detail in the description of my views, into which error I fell from being too anxious to avoid prolixity. As also since that period some additional evidence has been obtained which corroborates my opinions, I shall now advert to those points which are yet debated, and perhaps place them in a clearer point of view than had been done in my former paper. So far as regards the action of ammonia without water, all my ideas have been adopted; but in the relation of the ammonia and water in the common ammoniacal salts, where the ammonium theory of Berzelius comes into question, the evidence for my theory has not appeared so perfect. In fact, in order to see the true relation of the Berzelian theory to mine, it is necessary to contemplate the common salts of ammonia under two different points of view,—1st, *their position as alkaline salts, and* 2nd, *their po-*

sition as compound bodies, without reference to any other circumstance. In the first the proper theory of the salts of ammonia is that of Berzelius, but for the second purpose it is necessary to adopt the ideas on which mine is founded.

For in fact the question is, What is sal-ammoniac? Its most striking philosophical character is its equivalence to chloride of potassium. It has the same crystalline form. It enters into combination subject to the same laws. They are two bodies formed decidedly upon the same plan. But chloride of potassium contains only two elements, while sal-ammoniac contains three. There is one common to both. The residual elements are equivalent, and $Cl\,K$ and $Cl\,N\,H_4$, as well as K and $N\,H_4$, are bodies which correspond to each other. Ammonium when isolated, as it has, in the amalgam, all but been, appears to possess the properties of an alkaline metal; it markedly resembles potassium. That is the Berzelian theory, in which to the full I believe as well as Berzelius. The equivalency of sal-ammoniac and chloride of potassium is a fact, and the equivalency of the K in the one and of the $N\,H_4$ in the other, is the natural inference from it. When therefore the equivalency of the ammoniacal and potash salts is under question, the ammonium theory is correctly used: it is not ammonia, it is not amidide of hydrogen which replaces potash, but it is to be called oxide of ammonium in comparison.

But if we for a moment cease to consider the relation of the ammoniacal and the potash compounds, and taking sal-ammoniac by itself, proceed to examine what light can be derived from other sources towards illustrating its internal constitution, the question presents itself, can we believe the ammonium to be ready-formed in sal-ammoniac? Can we consider the ammonium, which in the amalgam gives up its hydrogen spontaneously, to retain it so strongly when in contact with iodine or chlorine, and to enter into combination only as a single and perfect group? The answer to this question, in the framing of which all the classes of ammonia compounds require to be taken into account, led me to the development of my theory.

The combinations of ammonia with the anhydrous chlorides of copper, zinc, and mercury, resemble in all essential characters sal-ammoniac, and moreover, like it, those which are volatile or soluble without decomposition are found to belong to the regular system of crystallization. They further combine with the metallic chlorides of the magnesian class to form double chlorides. Thus there are

1. $\mathrm{Cl\,H_4\,N}$ and $\mathrm{Cl\,Cu + Cl\,H_4\,N}$.
2. $\mathrm{Cl\,Cu\,N\,H_3}$ and $\mathrm{Cl\,Cu + Cl\,Cu\,H_3\,N}$.
3. $\mathrm{Cl\,Zn\,N\,H_3}$ and $\mathrm{Cl\,Zn + Cl\,Zn\,H_3\,N}$.
4. $\mathrm{Cl\,Hg\,N\,H_3}$ and $\mathrm{Cl\,Hg + Cl\,Hg\,H_3\,N}$.

The completeness of the analogy thus indicated is acknowledged by Graham, who proposes to extend the Berzelian theory so as to include these cases. He assumes, that in the compound radical ammonium the hydrogen may be replaced by a metal, and thus a cuprammonium $\mathrm{N\,H_3\,Cu}$, a zincammonium $\mathrm{N\,H_3\,Zn}$, a hydrargammonium $\mathrm{N\,H_3\,Hg}$, may be capable of individual existence. If Hg and H are replaceable, then $\mathrm{Hg_2\,H_2\,N}$ is equivalent to $\mathrm{H_4\,N}$, and thus he agrees with me that the type of sal-ammoniac and white precipitate ($\mathrm{Cl\,H\,.\,H\,Ad}$ and $\mathrm{Cl\,Hg + Hg\,Ad}$ on my theory) is the same.

But why have we not $\mathrm{Cl\,Hg_3\,H\,N}$, or $\mathrm{Cl\,Cu_3\,H\,N}$, or $\mathrm{Cl\,Hg_4\,N}$ and $\mathrm{Cl\,Zn_4\,N}$, taking their place among these bodies generated by ammonia? For $\mathrm{Cu_4\,N}$ or $\mathrm{Zn_4\,N}$ would also be equivalent to ammonium. The replacement stops when there remain yet two equivalents of hydrogen to the nitrogen; and it is only by a temperature such as destroys completely the constitution of these bodies that a metallic azoturet can be produced.

The compounds containing oxygen acids are precisely similar to those chlorides just described. The bodies

$\mathrm{S\,O_3\,.\,O\,Cu\,N\,H_3}$ and $\mathrm{S\,O_3\,.\,O\,Cu + S\,O_3\,.\,O\,Cu\,N\,H_3}$
$\mathrm{S\,O_3\,.\,O\,Zn\,N\,H_3}$ and $\mathrm{S\,O_3\,.\,O\,Zn + S\,O_3\,.\,O\,Zn\,N\,H_3}$
$\mathrm{S\,O_3\,.\,O\,H\,N\,H_3}$ and $\mathrm{S\,O_3\,.\,O\,H + S\,O_3\,.\,O\,H\,N\,H_3}$
and $\mathrm{S\,O_3\,.\,N\,H_3 + S\,O_3\,.\,O\,H\,N\,H_3}$

are so obviously similar in constitution, that the one explanation of their internal structure must be admitted.

There exist thus two sorts of compounds, which are ordinary ammoniacal salts with metallic oxide in place of water, or in the words of the theory of Graham, that contain ammonium in which hydrogen is replaced by a metal; those as $\mathrm{Cl\,.\,Hg\,N\,H_3}$, in which one equivalent, and those as $\mathrm{Cl\,.\,Hg\,N\,H_2\,Hg}$, in which two equivalents have been thus replaced. The substitution stopping there shows that $\mathrm{N\,H_2}$ is fixed, and thus that even if these various sorts of ammonium be admitted, the amidogene must be considered as pre-existing in them; and as Graham admits my formula for ammonium $\mathrm{N\,H_4} = \mathrm{Ad}\left|\begin{matrix}\mathrm{H}\\ \mathrm{H}\end{matrix}\right.$, so his metallic ammoniums become

$$\mathrm{Ad}\left|\begin{matrix}\mathrm{H}\\ \mathrm{Cu}\end{matrix}\right.,\quad \mathrm{Ad}\left|\begin{matrix}\mathrm{H}\\ \mathrm{Zn}\end{matrix}\right.,\quad \mathrm{Ad}\left|\begin{matrix}\mathrm{Hg}\\ \mathrm{H}\end{matrix}\right.,\quad \mathrm{Ad}\left|\begin{matrix}\mathrm{Hg}\\ \mathrm{Hg}\end{matrix}\right. \text{ and so on.}$$

Under this form the ammonium theory is capable of being extended so as to embrace a large class of the ammoniacal compounds newly discovered,—but can it embrace all? When calomel is treated by water of ammonia there is formed the body, $Hg_2\,Cl + Hg_2\,N\,H_2$. If the ammonium theory be applied to this body, are we to make a new compound radical, and say it is $Cl + N\,H_2\,Hg_4$, and to say that the corresponding body $S\,O_3\,.\,Hg_2\,O + Hg_2\,Ad$ is $S\,O_3\,.\,O\,N\,H_2\,Hg_4$? Here, then, is no parallel whatever; these bodies lie altogether out of the possibility of replacement connecting them with ordinary ammonium; and it would be far too violent a supposition to assume the existence of a body, $N\,H_6$, in order to support the disputed existence of a more likely body, $N\,H_4$. I consider this example as being fatal to Graham's view. Sublimate $Cl\,Hg$ gives white precipitate $Cl\,Hg\,.\,Hg\,Ad$, calomel $Cl\,Hg_2$ gives black precipitate $Cl\,Hg_2\,.\,Hg_2\,Ad$; relations so simple, so natural, that it should require very strong reasons indeed to prove that they are not those consonant to truth.

If the theory of metallic ammoniums were adopted, it would be only just to give to it its proper form. A sulphate of ammonia perfectly isomorphous with sulphate of potash contains $S\,O_3\,.\,N\,H_3 + 2\,H\,O$. The replacing element of the potassium is therefore $(N\,H_5\,O)$; there is no doubt of this; it is one of Mitscherlich's best-established determinations; $(N\,H_4)$ and $(N\,H_5\,O)$ are equally isomorphous with potassium. Hence as sublimate treated in the cold with ammonia gives $Cl\,N\,H_4$ and $Cl\,.\,N\,H_2\,Hg_2$, so in boiling water we get $Cl + (N\,H_2\,Hg_4\,O_2)$ equally equivalent, and a sort of complex ammonium. The ammonia turbeth is thus: $S\,O_3 + O\,(N\,H_2\,Hg_4\,O_2)$; there is $N\,O_5 + O\,(N\,H_2\,Hg_4\,O_2)$ and $I\,(N\,H_2\,Hg_4\,O_2)$, and so on. Now the correspondence of the common oxychloride of mercury to these bodies has been proved by Ullgren, and it should therefore be looked upon as a chloride of a compound radical $Cl + (Hg_4\,O_3)$. This is truly the principle involved in Graham's idea of compound ammoniums; for no matter where we begin, we find the chain by which the common salts of ammonia and the common basic salts are connected so perfect, that whatever principle we apply to one includes the others. Already, two years ago (May, 1838, *Annalen der Pharmacie*), I started the question, Were basic salts salts of compound radicals? and Liebig, in adapting a theory to the salts of platina discovered by Gros, approached nearly to the embodying of the same idea; but further examination showed me that it is one which is at present quite unfit for science, the even partial adoption of which would throw into confusion the most positive and simplest systematic arrangements that chemistry possesses, and hence do much harm and no good. Having so far considered the degree of weight which should be attached to the extension of the ammonium theory proposed by Graham, I shall now pass to the objections which have appeared to Rose to lie against some portions of my theory.

Admitting the consistency and completeness of the arrangement which the compounds of ammonia with the dry oxygen acids and with the hydrogen acids assume according to my views, the illustrious analyst of Berlin yet considers that the assimilation of the hydrated ammonia salts of oxygen acids to those salts of the same acids which contain two equivalents of base is forced and unnatural; and he says that in place of attending to the great fact of the isomorphism of ammonium and potassium, I have neglected and suppressed that fact. This I by no means did; but this isomorphism was not the only thing to be taken into account. In fact, when all things were considered, the argument about the isomorphism of the two alkalies is of most force on the other side, and my opinion is that on the side of ammonia we have outflanked, as it were, the line of metallic bases, and that the constitution of ammonium, subamidide of hydrogen, is that which we shall hereafter find the alkaline metals to possess. The masterly researches of Rose himself on the sulphates and carbonates of ammonia are, as I believe, remarkably in favour of my view. In the carbonates of ammonia $CO_2 . NH_3 + CO_2 . NH_3 . HO$ and $CO_2\,HO + CO_2 . HO . NH_3$ what complete evidence do we obtain of the identity of type of $NH_3 = Ad\,H$ and HO? In like manner if we look upon the series

$$\begin{array}{l} SO_3 . Ad\,H + SO_3\,HO . Ad\,H, \\ SO_3 . OH + SO_3\,HO . Ad\,H, \\ SO_3 . O\,Cu + SO_3\,HO . Ad\,H, \\ SO_3 . O\,Cu + SO_3\,CuO . Ad\,H, \end{array}$$

we are driven, in order to avoid considering the recognised ammonium salt as a salt with two equivalents of base, to the adoption of the views of metallic ammoniums already sufficiently refuted.

On my theory the only hypothetic assumption is the existence of amidogen. The subsequent principles adopted that

1. Ammonia NH_3 is $NH_2 . H = Ad + H$,
2. Sal-ammoniac $Cl\,H . NH_3$ is $Cl\,H + H\,Ad$,
3. White precipitate $Cl\,Hg_2\,NH_2$ is $Cl\,Hg + Hg\,Ad$,
4. Sulphate of ammonia $\}\ SO_3 . HO\,NH_3$ is $SO_3 . OH + H\,Ad$,
5. The black substance $Cl\,Hg_4\,NH_2$ is $Cl\,Hg_2 + Hg_2\,Ad$,
6. Ammonium if ever isolated NH_4 is $H_2\,Ad$,

are all experimental and necessary results; there is nothing hypothetical about them; everything follows from experiment.

Now the existence of amidogene is one of the best-established and most universally received hypotheses in chemistry; moreover, it is adopted also by Graham and by Rose. But how many more hypotheses must they adopt for the theory of ammonium! I shall only count up a very few, or better, state that the number of hypothetical bodies necessary for the complete ammonium theory would be equal to the number of possible metallic amidides, therefore equal to the number of metallic chlorides and oxides at present known; and this without at all touching on the theory of basic salts, which, as I have shown, is a necessary consequence of the theory of complex ammoniums.

If general and simple laws can be obtained by the introduction of an hypothesis, and according as experimental research proceeds, the new facts gained are found to regulate themselves according to it, a real and important service is conferred upon science by him to whom we are indebted for it. But when a theory must change its shape and make a new assumption for each new fact discovered, as soon as the direct tendency of the theory is at an angle with that of research, and it must tack from side to side to keep the course of discovery in its line, its day has passed; and notwithstanding the services rendered to chemical theory by the hypothesis of ammonium, it is now, as I conceive, incapable of retaining its old position. Its great utility was in fixing attention on the relations of the ammoniacal and potash salts; but for explaining the immensely extended classes of compounds which ammonia is now known to form, it is insufficient.

In a paper lately published by Mitscherlich, he describes a compound of chloride and azoturet of mercury $2\,Hg\,Cl + N\,Hg_3$, and he makes an observation which, as connected with the present subject, I shall here notice. He says that the equivalent of white precipitate is not $Cl\,Hg + Hg\,Ad$, but three times that, because it requires $3\,(Cl\,Hg + Hg\,Ad)$ to give $Cl\,Hg + 2\,N\,H_3$ and $2\,Cl\,Hg + N\,Hg_3$: he says that also amide of potassium $K\,N\,H_2$ should be taken $3\,K\,Ad$, because it gives $2\,N\,H_3$ and $K_3\,N$. Now this appears to me to be a very irrational method, for then the body $Cl\,Hg\,N\,H_3$, the simple formula of which Mitcherlitch admits, should be $8\,(Cl\,Hg\,N\,H_3)$, because it gives $2\,Cl\,Hg + 3\,Cl\,Hg_2 + N + 4\,N\,H_3 + 3\,Cl\,NH_4$. The hydrate of phosphorous acid should be $4\,P\,O_3 + 3\,H\,O$, because it gives $P\,H_3$ and $3\,P\,O_5$; and a crowd of other examples might be brought forward. The equivalent formula of a body cannot be fixed thus from a single action or property.

The formula of sulphate of ammonia is not $4\,(S\,O_3 \,.\, N\,H_4\,O)$, because by heat it gives $2\,(S\,O_2 \,.\, N\,H_4\,O) + 2\,S\,O_2 + 2\,N$, but every property must be taken into account, and our idea of the body derived from a careful induction, based on a study of all the facts known of it and of its congeners.

The red substance described by Mitscherlich resembles, in fact, those obtained by Rose with sublimate and phosphuretted hydrogen, and the bodies I have myself described, containing arsenic. In fact, as Laurent and Bineau have noticed for azote, and I myself for arsenic and phosphorus, these substances replace oxygen or amidogene in the proportion of one-third of their ordinary equivalent. Phosphuretted hydrogen $P\,H_3$ does not resemble ammonia $N\,H_3 = Ad\,H$, but $\frac{P}{3} \,.\, H$ resembles $Ad\,H$ or $H\,O$. The compound of iodide and phosphuret of hydrogen resembles not the hydriodate of ammonia, which is $I\,H + H\,Ad$, but the oxy-chloride of mercury, and the compound of chloride and phosphuret of mercury is similar. There are thus

$$\left.\begin{array}{l} H\,I + 3\,H\,\frac{P}{3} \\ Hg\,Cl + 3\,Hg\,\frac{P}{3} \\ Hg\,Cl + 3\,Hg\,O \end{array}\right\} \text{all corresponding bodies.}$$

Now the ammonia compounds when decomposed by heat pass into this class, in one or two cases the action being sufficiently violent spontaneously to effect it, and the substance of Mitscherlich is

$$2\,Hg\,Cl + 3\,Hg\,\frac{N}{3} \text{ resembling the above.}$$

I discovered this body myself when analysing white precipitate; but as I did not wish to stray from the direct discussion of the amidides, I did not publish anything about it at the time. I formed also some others of the same class, which, as soon as I can obtain leisure I will complete the examination of, and give the details of their history.

XIX. *On the Composition of Inulin. By* Mr. E. A. PARNELL[*].

THIS substance, which was first discovered by V. Rose in the root of *Inula Helenium*, in 1804, has since been found by Payen and other chemists in several other roots, as

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[*] Communicated by Professor Graham.

Angelica Archangelica, *Colchicum autumnale*, *Helianthus tuberosus*, &c., and in a few lichens, as *L. fraxineus* and *L. fastigiatus*. Although known to be closely related to starch, and interesting as a probable member of the starch family (especially in being converted into gum and starch sugar by the action of dilute acids), no examination of its composition has, as far as I am aware, been made. To supply this deficiency, I have performed a few analytical experiments on inulin and its compounds, the results of which form the subject of the present communication.

The inulin analysed was prepared from the root of the dahlia, as follows: the moist root, with the skin previously removed, was sliced, macerated, and washed with cold water. This was boiled in five parts of water for about an hour and a half, and filtered. The solution was nearly colourless, and quite neutral to test paper*. It was then evaporated until a pellicle appeared on the surface, and on setting aside to cool a large quantity of inulin was deposited in the form of a white pulverulent precipitate. This was collected on a calico filter, and washed with cold water until all the salts present were removed. It was then perfectly tasteless. Dried at a gentle heat, it became gummy, transparent, and easily pulverised; very soluble in hot, but sparingly in cold water. The liquid filtered from the first deposit of inulin gave an additional quantity on evaporation, which was obtained pure by washing, resolution, and evaporation.

That used in the two first analyses was prepared by adding alcohol to a strong aqueous solution; on standing the inulin was deposited perfectly pure. That used in the third analysis was made without alcohol.

1st. — 6·70 grains gave 10·65 carbonic acid, and 3·824 water.
2nd.— 6·82 ...... 10·87 ...... 3·960 ...
3rd.— 7·38 ...... 11·72 ...... 4·260 ...

	1.	2.	3.	Mean.
Carbon . . .	43·95	44·07	43·90	43·97
Hydrogen .	6·34	6·45	6·41	6·40
Oxygen . . .	49·71	49·48	49·69	49·63
	100·00	100·00	100·00	100·00

This nearly approaches the formula $C_{24}H_{21}O_{21}$; thus

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* Payen directs chalk to be added, a free acid being present.

24 Carbon	1834·5	43·71
21 Hydrogen	262·0	6·25
21 Oxygen	2100·0	50·04
1 Equivalent inulin	4196·5	100·00

The analogy of this formula (the lowest whole numbers which can be given) to gum, starch, grape and cane sugars, will at once be apparent; each of these containing the molecular group, 24 carbon, united to hydrogen and oxygen, in the proportions to form water. But in the whole of these (with one exception, the compound of chloride of barium and cane sugar, lately examined by Peligot, $C_{24}\,H_{21}\,O_{21} + Ba\,Cl$, or $C_{24}\,H_{18}\,O_{18} + 3\,H\,O,\,Ba\,Cl$), the number of atoms admitting of division, the equivalent becomes doubtful. In crystallized cane sugar, for instance, should the formula be $C_{24}\,H_{22}\,O_{22}$, or $C_{12}\,H_{11}\,O_{11}$? While in the case of inulin this division is impossible.

To satisfy myself as to the constitution of this substance, I endeavoured to combine it with metallic oxides, but the only one attended with success was oxide of lead. Neither acetate nor subacetate of lead precipitates the aqueous solution of inulin; but on mixing its solution with the ammoniacal acetate of lead, or on adding ammonia to the mixed solutions of inulin and acetate of lead, a very bulky white precipitate occurs, which is a hydrated inulate of lead. On heating and stirring it partly dissolves, and the remainder aggregates to a heavy tenacious mass, which also dissolves if sufficient water be present. It is readily obtained pure by washing the precipitate as first produced in the cold with cold water, it being impossible to wash clean after it has aggregated. But there exist two of these inulates, possessing precisely the same physical appearances, and only distinguishable by ultimate analysis. I have procured both by the above process, and have in vain sought for the peculiar circumstances of the formation of each. Neither temperature, order of mixing the ingredients, relative proportions, nor strength of solutions, influences it in the slightest degree. The one most commonly formed possesses the composition $C_{24}\,H_{21}\,O_{21} + 5\,PbO$; the other $C_{24}\,H_{18}\,O_{18} + 3\,Pb\,O$, both being dried at 212°.

I may remark, that I have always obtained either one or the other of these compounds, and never an indefinite mixture of both.

The mean of several analyses of one is, carbon 16·65, hydrogen 2·44 per cent., with 62·43 per cent. of oxide of lead; while the other contains carbon 22·46, hydrogen 2·94, with 51·23 per cent. of oxide of lead. The following formulæ are the nearest which can be given to these numbers:

		Calculated.	Found.
24 Carbon	1834·4	16·42	16·65
21 Hydrogen . . .	262·0	2·35	2·44
21 Oxygen.	2100·0	18·81	18·48
5 Oxide of lead. .	6972·5	62·42	62·43
	11168·9	100·00	100·00

and

		Calculated.	Found.
24 Carbon	1834·4	22·81	22·46
18 Hydrogen . . .	224·6	2·79	2·94
18 Oxygen.	1800·0	22·38	23·37
3 Oxide of lead. .	4183·5	52·02	51·23
	8042·5	100·00	100·00

As first precipitated in the cold, these inulates are hydrated; they retain this water if dried in the air, give it off slowly by being kept over sulphuric acid, and readily to air at 212°. The one which contains 5 PbO gave only 2·00 per cent., the other gave 7·00. This last, with allowance for hygrometric moisture, approaches 6·4, which is the calculated number for five equivalents, making the formula $C_{24} H_{18} O_{18} + 3 PbO + 5 HO$. Thus, it would appear, that anhydrous inuline is $C_{24} H_{18} O_{18}$ (isomeric with anhydrous cane sugar), which, in the case of inulin dried at common temperatures, is combined with 3 H O. With this water it does not part without decomposition. In the two lead compounds inulin is combined, in one with $3 HO + 5 PbO$, and in the other with $3 PbO + 5 HO$. It is also possible that inulin in solution may be combined with eight equivalents of water, thus making the series

$C_{24} H_{18} O_{18} + 3 HO + 5 HO$? inulin in solution.
...... $+ 3 HO$ inulin dried at common temperature.
...... $+ 3 PbO + 5 HO$.
...... $+ 3 PbO$.
...... $+ 3 HO + 5 PbO$.

That the group of three atoms of water is more strongly attached than the group of five atoms, is proved, first, by dry inulin containing three, and not five atoms of water, in combination with $C_{24} H_{18} O_{18}$; and secondly, by the compound $C_{24} H_{21} O_{21} + 5 PbO$ giving off no water without destruction of the remainder, while the other lead compound loses five equivalents of water at 200°.

The constitution of these substances may be viewed in a somewhat different light. Water, existing as such, combined with any substance allied to starch, either basic or constitutional, as supposed above, is never known to be *so intimately combined* with the substance as to be incapable of displacement by oxide of lead, especially under such a favourable circumstance as the presence of free ammonia. When substitution appears impossible, it is therefore reasonable to conclude that the water, if it exists at all, is neither basic nor constitutional, but is essential to the substance itself.

The remarkable circumstance of inulin giving off at one time three equivalents of water, and not at another, under apparently the same circumstances, suggests the idea that two inulines exist, having the same composition, but one containing water capable of displacement, which the other does not. This substance has, indeed, been obtained by chemists in two different physical states, gummy and pulverulent (although I have never obtained but the former dry); and I would suggest the probability of these having the constitutions above-mentioned, that is, that one is $C_{24} H_{21} O_{21}$, and the other $C_{24} H_{18} O_{18} + 3\,HO$; a feeble but unknown cause being sufficient to convert one into the other.

This is not a singular instance of a double constitution. In fact, the only satisfactory means of accounting for dimorphism, is by supposing a difference in the constitution of the same substance at one time to that which it possesses at another, the ultimate composition continuing the same. In diabetic sugar, also, we have reason to suspect a double constitution, the descriptions of this substance by various authors being, as is well known, highly discordant. Some identify it with starch sugar; others, on the contrary, describe it as a peculiar substance. This is an interesting and very extensive inquiry, and would probably throw light on some of the cases of isomerism still unexplained.

University College, London,
May 16, 1840.

XX. *On certain Effects of Temperature. By* C. T. COATHUPE, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

HAVING, from the nature of my occupations, an excellent laboratory for observing the effects of temperature, I beg to offer you some experiments illustrative of some of these effects.

A modern glass-house is generally a cone built of brick, having its interior diameter at the base, varying from 58 to 60 feet, and its perpendicular height varying from 90 to 100 feet. The upper aperture through which the smoke ascends, varies from 9 feet 6 inches to 10 feet in diameter. This cone terminates at its base in substantial pillars of brick about 3 feet square, following the exterior inclination of the surface of the cone, and united above by arches which spring from pillar to pillar, and below by inverted arches beneath the ground.

Around the centre of the interior floor of this cone the furnace is erected; and around the exterior of the pillars which support the main body of the cone, the glass-house is extended by shed roofs, whose rafters bear against the exterior of the brick cone, above the arches which connect the pillars. This extension constitutes the manufacturing workshop, or space occupied by the glass-making operatives. The interior space around the furnace and within the pillars, is that occupied by the founders, or the men whose duty it is to fill the pots with raw materials for the production of glass, to urge the fire, to examine from time to time the state of fusion, and in short, to make from sand, alkali and lime, by the aid of intense heat, the material which the glass-making operatives subsequently convert by manipulation into glass.

For very many consecutive hours during the process of founding the raw materials, a thermometer placed at the greatest possible distance from the furnace, but within the area occupied by the founders, and freely suspended from a rod projecting from the interior surface of one of the brick pillars (a distance in the present instance = 20 feet 5 inches), will indicate a temperature varying from 316° to 325° of Fahrenheit. The founders have cool recesses, into which they frequently retire during their work, but the average of temperature here mentioned, viz. from 316° to 325°, and frequently very much beyond 325°, they bear without experiencing any inconvenience whatsoever. Strangers universally wonder at the possibility of human beings existing in a situation in which their clothes are continually scorched, while their naked skin exhibits no marks of the effects of fire. I had myself often wondered at the circumstance, until I made some experiments to endeavour to ascertain the cause of such an anomaly. The results of some of these experiments are curious from the extent of the ranges of the temperatures, and I have much pleasure in proffering them to those of my philosophical brethren who may feel an interest in such matters.

Exp. 1. Two silvered brass scale thermometers, having each a range of 600° Fahrenheit, were suspended from an iron pin at a distance of two inches from the interior surface of one of the brick pillars of the glass-house, at a distance of 20 feet 5 inches from the nearest point of the furnace emitting flame, and during the early part of the founding process.

They both indicated, from an average of ten simultaneous pairs of consecutive observations which ranged well together, a temperature of 194°·4 Fahrenheit.

Exp. 2. One of the thermometer bulbs was now clothed with a thin case of fine black "merino." The average of ten simultaneous pairs of consecutive observations indicated a difference of temperature manifested by the clothed bulb, in excess of that manifested by the unclothed bulb, of 23°·1 Fahrenheit upon each pair of observations. Hence 23°·1 of Fahrenheit were retained by the covered bulb, which were evidently reflected, and lost to observation, by the bright metallic surface of the unclothed bulb.

In another series of experiments wherein the temperature indicated by the unclothed thermometers averaged only 124°·5 Fahrenheit, from twelve pairs of observations, the increment shown by covering one of the bulbs with a thin bag of black "merino," amounted to 34°·66 Fahrenheit. Hence the quantity of heat that is reflected from the bright surface of a thermometer diminishes as the heat itself increases.

Exp. 3. During the latter part of the founding process and whilst the clothed thermometer suspended from the brick pillar ranged from 320° to 325° Fahrenheit, a small black iron cylindrical pan filled with water was placed upon a thin iron shelf, which had been fixed against the pillar and close by the side of the thermometer. It was reasonably anticipated that water thus placed in a temperature of 320° to 325° would boil; but after waiting until the half of it had evaporated, it showed no tendency to ebullition.

Exp. 4. The top of the iron pan was now covered with a pane of window-glass, and in a few minutes it boiled violently. This experiment demonstrated that the cooling properties of rapid evaporation can neutralize one of the direct effects of heat to a very surprising extent; and to ascertain the amount of this influence the following experiment was instituted.

Exp. 5. A clothed thermometer, whilst the mercury was indicating a temperature of 310° Fahrenheit, was immersed in a vessel of boiling water. The mercury *instantly* fell to 212°, and then *very gradually* sunk to 141°. The merino envelope had become dry, and the mercury had commenced rising when the bulb was immersed a second time into the water. The mercury rose to 202°, and then gradually fell to 139°. By a third immersion the mercury rose to 198°, whence it fell *gradatim* to 133°. The envelope was now saturated with water at about 140° Fahrenheit, but the mercury speedily reassumed the temperature of 133° Fahrenheit, and remained at this fixed point for nearly five minutes, although the real temperature of its situation was, and had been for many previous hours, 310° Fahrenheit.

The effects of rapid aqueous evaporation were thus clearly shown to influence the indications of the thermometer when placed in a dry atmosphere of 310° Fahrenheit, and under the circumstances described, to the surprising extent of 177° Fahrenheit.

We may now infer that the *copious perspiration* which exudes from the skin of glass-makers, and of those who are engaged in similar scorching occupations, is a sufficient protection from the burning effects of a *dry* atmosphere of from 300 to 400 *degrees of Fahrenheit*; and that whilst the clothes of such persons are burning to tinder, their skin may be rendered insensible to the direct effects of fire upon the inanimate matter around them, by simple natural laws, viz. those of evaporation.

Having been engaged in some delicate experiments on the subject of heat, I was surprised at the effects of comparatively moderate *dry* temperatures upon such thermometer scales as were made of ivory.

In one instance the scale became shortened two degrees in 100 in a temperature of 212° Fahrenheit.

In another, an old and "well-seasoned" ivory scale that had often endured the maximum solar heat of Jamaica and the salt waters of the Atlantic, became shortened one degree in 110° from a *dry* temperature of 130° Fahrenheit.

In a third, the scale became shortened 1½° in 120° from a short exposure to a *dry* heat of 260° Fahrenheit. Immersion in water will generally restore such scales to their original length.

I have the honour to be, Gentlemen,
yours obliged,

Wraxall, near Bristol,
April 22, 1840.

CHARLES THORNTON COATHUPE.

XXI. *Remarks on some Tide Observations, published in the Transactions of the British Association.* *By* Richard Thomas, *Civil Engineer*[*].

IN the Transactions of the British Association for the year 1838, is an account of a level line measured from the Bristol Channel to the English Channel, from which the writer draws the conclusion, "that *the mean tide must be taken as the level of the sea,*" and that this mean tide is the same, or nearly the same elevation at all places, whatever may be the differences of rise and fall.

About three years ago the British Association published the results of certain tide observations made at Liverpool, which induced me to write the following letter on the subject, which was inserted in the 'Mining Journal,' and "Falmouth Packet" Newspapers.

"In the supplement to the 'Mining Journal,' No. 23, there appears to be published under the sanction of the British Association, '*that there is one invariable mean height common to neap and spring tides, the* HALF TIDE MARK, a point from which engineers, geologists, and navigators will henceforward commence their calculations and adjust their standards of comparison.'

"I have reason to believe that however accurate the conclusion is with regard to the tides at Liverpool, where the observations were made, it is not correct as to its general application, and I mean to show that the tides generally have not the same elevation of half-tide mark, as applies to any particular locality, nor is the average half-tide mark, nor low-water mark, nor high-water mark at one part of the coast to be depended on as level with the corresponding tide marks on other parts. More than twenty years ago I had occasion to attend particularly to the tides at Falmouth, and the result of my observations showed differences as much as two feet and a half in elevation on half-tide marks. The rise and fall of ordinary spring tides there is about 17 feet, and of ordinary neaps little more than seven feet, *but the several rounds of tides differ as to the mean elevation of the sea*; the low- and high-water marks for the same difference of rise and fall being at greater elevations than others.

"At King-Road near Bristol, I observed the tides in the year 1815, and found that the difference of elevations of some half-tide marks amounted to about four feet.

"It may be possible, and I think it probable, from my observations, that these differences of half-tide levels, or rather of the mean height of the sea, may be operated on in a regular way by whatever causes it to vary at different periods: but I am not aware that any observations have yet been made for the purpose of determining the recurrence of such variations, or for ascertaining their causes; and until this be done, no mean tide-level could be determined on. But, after all, such standard level must have reference to some fixed or *bench* mark in order to make it useful, and could only be applied to one neighbourhood, as it could not be depended on with reference to tides at any distant part of the coast, which may be exemplified by a reference to the tides between the Land's-end and King-Road; and although this example may be taken as almost an extreme case, nevertheless it will serve the better to elucidate the uncertainty of tide-marks.

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[*] Communicated by the Author.

" It must be evident that the particular formation and trending of the coast as connected with the set of the tide will tend to alter the elevation of the high-water as well as of the low-water marks. The Bristol Channel, from its form narrowing upwards to the Severn, causes the tidal wave to rise much higher than at any other part of the English coast, for it may be considered that the tides at the flood have a tendency to rush from the Atlantic into the mouth of this channel with about the same velocity as through the inlets to the English Channel and the Irish Sea; but in the Bristol Channel there is not the same proportion of space as compared with the breadth of the entrance; hence in their course upward, being confined by the approximation of the shores, they are raised to a height increasing as the breadth lessens, until they attain sufficient elevation for the gravity of the water to counterbalance the impulse of the tide.

" The sea at ordinary spring tides rises and falls about 45 feet at King-Road, yet the low water there appears to be some feet above the low water of the ocean, as the great influx of the Severn at such tides occasions a great quantity of water to be coming down even at the return of the flood, so that at the time of low water below the Holmes Isles, the down tides run with such rapidity at King-Road as to indicate a great fall, and consequently there is a considerable rise in the tide before it meets the level of the water there; but it must be recollected, that then there will be some fall in the water from the Holmes to King-Road.

" At Ilfracombe, about the same time as I observed the tides at King-Road, the rises and falls were taken by my particular directions, and showed the ordinary spring tides there to rise and fall about 30 feet, and the ordinary neaps about 14 feet.

"For reasons before stated, it appears that the level of the low water at King-Road is higher than the low water further down the channel. Now the half-tide mark of ordinary spring tides at King-Road is about 22 feet above the low water there. At Ilfracombe the half-tide is 15 feet above the low water, and at the Land's-end it is eight feet and a half.

"Even supposing the low-water mark to be as low at King-Road as at the other places, then the half-tide there is seven feet above that mark at Ilfracombe, and thirteen feet and a half above that at Land's-end; but in all probability instead of 7 and 13½, the differences are really about twelve and twenty.

"The low-water marks of the corresponding tides appear to present a much better approximation to a true level than the half-tide mark, and these are taken for determining the elevations ascertained in the Trigonometrical Survey of England and Wales: but it would be desirable for many purposes that the movements of the tides should be better known, which might lead us to a knowledge of a true level, or so near an approximation as would answer all practical purposes."
"Dated November 14, 1837."

Referring to the Transactions of 1838, as before mentioned, and granting that the series of levels have been ascertained with sufficient exactness for the purpose, we must draw the conclusion, either that the low water at King-Road at spring tide is about 13 feet below the low water at Land's-end, or that there is something which affects the tides at Axmouth so as to make the mean tide there stand at a higher elevation than it does at the Land's-end, and so make it approximate to the level of the mean tide at King-Road.

It appears to me very probable that the level of the sea is kept up at Axmouth above its average height, owing to the contour of the coast of the English Channel; for it is evident that the coast of Normandy, extending from Granville northward to Cap la Hogue, presents a bar to the direct flowing up channel of the flood tide, and turns the current towards that part of the English coast on which Axmouth is placed, and that the free course downward of the ebb-tide is interrupted by the projection southward of the coast of Devon between Exmouth and the Start; both of which causes will tend to keep the water the more elevated in the bay between the Start and the Bill of Portland. At the time of high water at Axmouth the tide is setting strongly up channel, and continues running up for, I believe, more than two hours longer, and the ebb runs down strongly at the time of low water at Axmouth. Now if the currents operate in the way above stated, it is clear that the effect will be more apparent on spring tides than on neaps, owing to the greater velocity of the water, and so it appears to be.

On the south coast of Cornwall the rise of ordinary spring tides is about 16 to 17 feet, and of ordinary neaps a little more than 7 feet. At Axmouth the rise of spring tides appears to be about 10 or 11 feet, and of neaps about 7 feet. Hence we see that although the rise and fall of neaps at Axmouth nearly corresponds with that of the south coast of Cornwall, yet there is a difference of six feet in the comparison of spring tides; this appears strongly corroborative of the idea of the tide currents operating in the way I mention.

By a reference to the table of contemporaneous observations of the tides (plate 2.) accompanying the paper referred to in the Transactions of the British Association, it will be seen that the high- and low-water marks at Plymouth are so placed that the line of mean tide shall about coincide with that of the tides at Axmouth. There appears to be no authority for this, as it does not appear that any levels have been extended to Plymouth. For the reasons as above stated, I should conclude that the high-water mark at Axmouth would probably be higher than the high water at Plymouth; but supposing it the same, then in the series of tides observed, would the mean level at Plymouth be at neaps nearly one foot, and at springs nearly two feet lower than at Axmouth; and be it remembered that the greatest rises at Plymouth, as stated in this table, are only 14 feet, which I should consider as much less than the average rise of spring tides there, seeing that on the neighbouring coast of Cornwall it is nearly three feet more. I should also think that the high water at Axmouth is more elevated than at Falmouth; but if it be only at the same level, then will the mean-tide mark be three feet higher at Axmouth than at Falmouth.

In the tide-table above referred to, it appears that the mean-tide line at King-Road is about one foot above that at Axmouth. In this comparison the greatest tides observed at King-Road rise about 36 feet, in which series the greatest difference of mean-tide level does not amount to one foot. Other tides appear by the table to have been observed there, some of which had a rise of 41 feet, and the range of mean tide differed as much as two feet and a half. In the year 1815, as before stated, I observed a series of tides at King-Road, which ranged from a rise of 19 feet to a rise of 46 feet, differing in their respective elevations of mean tide as much as $3\frac{1}{2}$ feet; the greatest tide rising from 0 on the scale to 46 feet, and the smallest from 10 to 29 feet; the differences in the levels of low water being 10 feet, and of high water 17 feet.

Had the differences between the levels of low water of spring tides and of neap tides at King-Road and at the Land's-end been about the same, we might be led to conclude that all the difference of rise and fall would have been due to the increased elevation of high water, owing to the narrowing of the Bristol Channel in proceeding upwards, as before stated; but as the differences of levels of low water at King-Road amount to about ten feet and those at Land's-end only to about five feet, it may be possible that the momentum of the ebb-tide may draw the water down at King-Road at low water to below the level of low water at Land's-end. If it be so then there can be no sure means of comparisons of sea levels at either mean tide, high water, or low water; but I should consider that the low water of neap tides is most likely to be the nearest approximation to a true level, and that the mean level of the sea on the south-west coast of Great Britain is about three feet and a half above the average level of the low water of neap tides.

In order to ascertain the particular operations of the tides, and their effects in certain localities, it would be desirable to have the tide levels simultaneously taken at convenient places on both the north and south coast, extending from King-Road and from Axmouth to the Land's-end, marking the time and level of high and low water, and the elevation of the sea at every quarter or half hour throughout the day. These observations being connected at each place with some particular bench-mark, would furnish data for comparison whenever lines of levels might be extended to them. Such lines of levels might be surveyed across Cornwall in sundry places, and might ultimately be connected with the line already surveyed from Portishead to Axmouth.

Although I do not mean to impugn the correctness of the results of the leveling across the country, yet I think another line of levels should have been taken by way of verification before any conclusion should have been adopted as to comparison of tides. Such verification would prove the correctness of the relative levels of the several bench-marks, which might be applied to further observations on the tides, and the levels extended to other places. It appears that the operator took every care to observe correctly the levels, and to prove them by leveling back over the line, in which the difference of results are so trifling as not to excite notice, if it had not been for the curious anomaly of the heights of the stations constantly coming out less on returning to them than on leaving them, which the operator suspects might have happened from the alterations of refraction. But surely he could easily have detected errors arising from such source by looking again at the back staff after he had observed the elevation on the fore staff.

To say nothing of the absurdity of supposing that the refraction was always decreasing during the survey, which it must have been to have brought about such results, the distances from the instrument to the staff (44 fathoms) were so small, that even supposing every day during the hours of work the refraction should at first have amounted to an angle equal to half the contained arc of the earth's surface, and should have decreased to nothing by the time the day's work was ended, and allowing a quarter of an hour to elapse between taking the back and fore observations, the error arising from refraction would not have amounted to half an inch on the whole distance from Portishead to Axmouth; nor would the daily shrinkage in the length of the staffs account for it, as such difference could only affect the levels in a very much less degree than the difference by the survey. It is possible that there might be some very small defect in the movements or the mounting of the instrument, so as in turning it round from the back to the fore observation it might gain in elevation; but the error appears to be so very small that it cannot materially affect the results.

Falmouth, April 30, 1840.

XXII. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals. By* GEORGE GULLIVER, *F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards.* No. IV.

[Continued from page 200 of the Phil. Mag. for March 1840.]

137. A NEW species of Gibbon, (*Hylobates* —— ?) a female. The most common diameters of the corpuscles 1-3600th, 1-3555th, 1-3429th, and 1-3200th of an inch. Extreme sizes 1-5333rd, and 1-2900th. Blood from a prick of the finger.

138. White-nosed Monkey, (*Cercopithecus petaurista*,) a male, apparently an adult. The most frequent diameters of the disks 1-3555th, 1-3428th, and 1-3200th of an inch. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the tail.

139. Marikina or Silky Tamarin, (*Midas Rosalia*,) an adult

female. Common-sized disks 1-3693rd, 1-3429th, and 1-3332nd of an inch. Extreme diameters 1-5333rd and 1-2666th. Blood from a prick of the tail.

140. Ring-tailed Lemur, (*Lemur catta*,) an adult male. All the following sizes common, 1-4000th, 1-3840th, 1-3644th, 1-3600th, 1-3557th, 1-3429th of an inch. Extreme diameters 1-6000th and 1-3000th. There were several corpuscles about 1-8000th of an inch in diameter, but it is doubtful whether these were true blood disks.

The size of the corpuscles was altogether remarkably irregular. The blood was obtained from a prick of the ear: the animal was apparently not very healthy.

In some blood obtained from another full-grown male, a few hours after death, the corpuscles were also very irregular in size, but the most abundant disks were chiefly of small and large dimensions, the former being generally 1-5000th of an inch in diameter, and the latter 1-3500th. There were also a few about 1-7000th of an inch, and also some as large as 1-3000th. The majority, however, were the small and large kinds first noted, with but very few intermediate gradations. The smaller corpuscles presented a remarkably dark and distinct outline. Blood from the heart, from the coronary veins, as well as from the renal and mesenteric veins.

141. Black-fronted Lemur, (*Lemur nigrifrons*,) a full-grown male. The following diameters all common: 1-4000th, 1-4500th, 1-4662nd, and 1-4705th of an inch. Extreme sizes 1-6000th and 1-3500th.

Blood from the tail in the first trial; and in the second, made a month afterwards, from a vein of the ear.

142. Anjouan Lemur, (*Lemur Anjuanensis*,) a male, nearly full-grown. Common sizes 1-4365th, 1-4268th, 1-4000th, and 1-3555th of an inch. Extreme diameters 1-5333rd and 1-3200th.

Blood from a wound of the tail; also from a prick of the hand.

In the Lemurs the corpuscles were remarkable as being very variable in size. Whether this will be confirmed by future observation, remains to be seen. It is deserving of notice that most of the animals were more or less diseased, and died very rapidly, chiefly of tubercular consumption, in connection with which the blood particles were perhaps more or less modified.

In the black-fronted Lemur (141.) the disks appeared, from the result of two trials made at different times, to be smaller than in the other Lemurs.

In another examination of some blood obtained from the heart of an adult white-fronted Lemur (60.), the corpuscles were again observed to be very variable in magnitude, 1-4800th, 1-4000th, 1-3600th, and 1-3000th of an inch, being all equally common diameters of the disks.

143. Slender Loris, (*Loris gracilis,*) a male, apparently full-grown. The common-sized corpuscles 1-3555th, and 1-3200th of an inch in diameter. Extreme sizes 1-4600th and 1-2900th. Blood from a prick of the finger.

144. Common Ferret, (*Mustela furo,*) an adult female. Common diameters 1-4800th, 1-4500th, 1-4000th, and 1-3693rd of an inch. Extreme sizes 1-6000th and 1-3000th.

Blood from a prick of the nose. The size of the disks was extremely variable.

145. Cape Hunting Dog, (*Lycaon tricolor,*) a young male, probably about half-grown. The majority of the corpuscles 1-4000th and 1-3693rd of an inch in diameter. Extreme sizes 1-4570th and 1-3200th. Blood from a prick of the nose.

146. Spotted Hyæna, (*Hyæna crocuta,*) an old male. The most frequent diameters 1-4360th, 1-4000th, 1-3600th, and 1-3555th of an inch. Extreme sizes 1-5333rd and 1-2900th.

Blood from a prick of the upper lip.

147. Virginian Opossum, (*Didelphis Virginiana,*) a male, about two-thirds grown. Common diameters 1-3600th and 1-3530th of an inch. Extreme sizes 1-4570th and 1-2900th.

There were quantities here and there of the red granular particles, generally about 1-5000th of an inch in diameter, being as usual smaller than the regular disks.

The blood was obtained from a prick of the tail. The animal was diseased, the hind legs being paralysed.

148. Vulpine Opossum or Phalanger, (*Phalangista vulpina,*) an adult male. Majority of the disks 1-4000th and 1-3693rd of an inch in diameter. Extreme sizes 1-5333rd and 1-3000th.

Blood from a prick of the ear.

149. Black Rat, (*Mus Rattus,*) an adult male. The most frequent diameters 1-4000th, 1-3692nd, and 1-3448th of an inch. Extreme sizes 1-5000th and 1-3000th. Blood from an incision of the tail.

150. Splendid Flying Squirrel, (*Pteromys nitidus,*) a full-grown female. The common sized disks 1-4000th, 1-3600th, and 1-3555th of an inch. Extreme diameters 1-4570th and 1-3200th. The size of the disks was remarkably regular. Blood from a prick of the tail.

151. Spotted Cavy or Paca, (*Cœlogenys subniger,*) a male, apparently an adult. The most frequent diameters 1-3693rd, 1-3600th, 1-3429th, and 1-3330th of an inch. Extreme sizes 1-4000th and 1-3000th. Blood from a cut of the tail.

152. Roebuck, (*Cervus capreolus,*) an adult male. All the

following diameters common: 1-6000th, 1-5800th, 1-5332d, 1-5000th, 1-4924th, and 1-4800th. Extreme sizes 1-6400th and 1-4000th of an inch.

Blood from a vein of the ear, also from a prick of the velvet of the young growing antler. The corpuscles obtained from these different parts were identical in size and appearance.

A recent examination of the blood corpuscles of some varieties of the common sheep (42.) gave the following results.

a. A four-horned Sheep from North Africa.

b. A two-horned hairy variety from Africa, presented to the Zoological Society by the Duke of Sutherland.

c. A hairy variety from Demerara.

All the above were full-grown males, and the blood was obtained from small incisions of the lips. The corpuscles in the different varieties appeared to be of the same size; the most frequent diameters being 1-6000th, 1-5615th, 1-5331st, and 1-5028th. Extreme sizes 1-7110th and 1-4570th of an inch.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 27, 1840.—THE reading of a paper entitled, "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic processes;" by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed but not concluded.

March 5.—The reading of a paper entitled, "On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic processes;" by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed and concluded. An abstract of this paper has already appeared in vol. xvi. p. 331 of this Magazine.

A paper was also read entitled, "Remarks on the Theory of the Dispersion of Light, as connected with Polarization;" by the Rev. Baden Powell, M.A., F.R.S., and Savilian Professor of Geometry, Oxford.

Since the publication of a former paper on the subject referred to, the author has been led to review the subject in connexion with the valuable illustrations given by Mr. Lubbock of the views of Fresnel; and points out, in the present supplement, in what manner the conclusions in that paper will be affected by these considerations.

A paper was also read, entitled, "Further Particulars of the Fall of the Cold Bokkeveld Meteorite;" by Thomas Maclear, Esq., F.R.S., in a letter to Sir John F. W. Herschel, Bart., K.H., V.P.R.S., &c. Communicated by Sir John Herschel.

This communication, which is supplementary to the one already made to the Society by Mr. Maclear*, contains reports, supported by affidavits, of the circumstances attending the fall of a meteoric mass in a valley near the Cape of Good Hope. The attention of the witnesses had been excited by a loud explosion which took place in the air, previous to the descent of the aerolite, and which was attended by a blue stream of smoke, extending from north to west. Some of the fragments which had been seen to fall, and which had penetrated into the earth, were picked up by the witnesses. One of them falling on grass caused it to smoke; and was too hot to admit of being touched. The mass which was sent to England by H.M.S. Scout, weighed, when first picked up, four pounds. The paper is accompanied by a map of the district, showing the course of the aerolite.

A paper was also read, entitled, "An account of the Shooting Stars of 1095 and 1243;" by Sir Francis Palgrave, K.H., F.R.S., &c.

The author gives citations from several chronicles of the middle ages, descriptive of the remarkable appearance of shooting stars which occurred on the 4th of April, 1095, on the testimony of independent witnesses both in France and England. One of them describes them as "falling like a shower of rain from heaven upon the earth:" and in another case, a bystander, having noted the spot where the aerolite fell, "cast water upon it, which was raised in steam, with a great noise of boiling." The Chronicle of Rheims describes the appearance as if all the stars in heaven were driven, like dust, before the wind. A distinct account of the shooting stars of July 26th, 1293, is given by Matthew Paris.

March 12.—A paper was read, entitled, "On certain variations of the mean height of the Barometer, mean temperature and depth of Rain, connected with the Lunar Phases, in the cycle of years from 1815 to 1823." By Luke Howard, Esq., F.R.S.

The table given in this paper contains the results of calculations relating to the objects specified in the title; cast into periods of six, seven, or eight days, so as to bring the day of the lunar phase belonging to it in the middle of the time. The observations were all made in the neighbourhood of London. It appears from them that in the period of the last quarter of the moon the barometer is highest, the temperature a little above the mean, and the depth of rain the smallest. In the period of the new moon, both the barometer and temperature are considerably depressed, and the rain increased in quantity. The influence of the first quarter shows itself by the further depression of the barometer; but the temperature rises almost to the point from which it had fallen, and the rain still increases, but not in an equal ratio. Lastly, the full moon again reduces the temperature; while the barometer attains its maximum mean height, and the quantity of rain is the greatest. Thus it appears, that during this lunar cycle, the approach of the last quarter is the signal for the clearing up of the air, and the return of sunshine.

[* See L. and E. Phil. Mag., vol. xiv. p. 368, for an abstract of Mr. Maclear's former paper.]

A paper was also read, entitled, "On the theory of the dark bands formed in the solar spectrum from partial interception by transparent plates." By the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry in the University of Oxford.

This paper contains the mathematical investigation of the phenomena of peculiar dark bands crossing the prismatic spectrum, when half the pupil of the eye, looking through the prism, is covered by a thin plate of any transparent substance, the edge being turned from the violet towards the red end of the spectrum; and which were first noticed by Mr. Fox Talbot, and were ascribed by Sir David Brewster to a new property of light, consisting of a peculiar kind of polarity.

The author shows, that on the undulatory theory, in all cases, a difference of retardation between the two halves of each primary pencil throughout the spectrum may give bands within certain limits; and that it affords a complete explanation of the phenomena in question.

March 19.—A paper was read, entitled, "Contributions to Terrestrial Magnetism." By Major Edward Sabine, R.A., V.P.R.S.

An increased activity has recently been given to researches in terrestrial magnetism, with the definite object of obtaining correct maps of the magnetic phenomena, corresponding to the present epoch, over the whole surface of the globe. To aid these researches, and to facilitate the comparison of the general theory of M. Gauss with the facts of observation, maps have been constructed of the magnetical lines, both as computed by the theory, and as derived from observations already obtained. The theoretical and actual lines of the declination and intensity have thus been represented in maps recently published in Germany and England, as have also the lines of the inclination computed by theory; but the corresponding map or the latter element derived from observations is yet wanting. The object of the present communication is to supply this desideratum, as far as regards the portion of the globe contained between the parallels of 55° N. and 55° S., and the meridians of 20° E. and 80° W.; comprising the Atlantic ocean, and the adjacent coasts of the continents on either side.

The observations chiefly employed for this purpose are two series *made at sea*; one by Mr. Dunlop of the Paramatta observatory, in a voyage from England to New South Wales, in 1831; the other by Lieut. Sulivan of the Royal Navy, in a voyage from England to the Falkland Islands, and back, in 1838 and 1839. The observation of the magnetic dip at sea, which was commonly practised by the distinguished navigators of the last century, was unfortunately not resumed when the interest in such researches was revived on the restoration of peace: but it is by such observations only that the lines of inclination can be independently traced over those large portions of the globe which are covered by the ocean. The difficulties which attend the observation, occasioned by the motion and the iron of a ship, require the adoption of several precautions, which it is particularly desirable at this time to make generally known. The series of

Messrs. Dunlop and Sulivan are discussed in this view; and the value of results obtained under circumstances of due precaution is pointed out by their success.

The position of the lines on the land portion of the map is derived from 120 determinations in various parts of Europe, Africa, and America, between the years 1834 and 1839, of which about the half are now first communicated.

The series of Messrs. Dunlop and Sulivan contain also observations of the magnetic intensity made at sea; Mr. Dunlop's by the method of horizontal vibrations, and Lieut. Sulivan's by the instrument and method devised by Mr. Fox. The degree of precision which may be obtained by experiments thus conducted, is shown by the comparison of these observations with each other, and with the isodynamic lines previously derived from observations made on land.

The first section of this paper concludes with discussions on the relative positions of the lines of least intensity and of no dip, and of the secular change which the latter line has undergone in the ten years preceding 1837.

In the second section, the observations of Mr. Dunlop are combined with recent observations on the coasts of Australia, by Captains Fitz Roy, Bethune, and Wickham, of the Royal Navy, to furnish a first approximation to the position and direction of the isodynamic lines over that portion of the Indian ocean which is comprised between the meridian of the Cape of Good Hope and New South Wales.

A paper was also in part read, entitled, " Experimental Researches in Electricity, seventeenth series. By Michael Faraday, Esq. D.C.L., F.R.S., &c. On the source of power in the Voltaic Pile."

March 26.—The reading of a paper, entitled, " Researches in Electricity, Seventeenth Series: on the source of power in the Voltaic Pile." By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

In a postscript, the author states that he has since found a passage in Dr. Roget's treatise on Galvanism, in the Library of Useful Knowledge, published in January 1829, in which the same argument respecting the unphilosophical nature of the contact-theory is strongly urged.

" Were any further reasoning necessary to overthrow it, (namely, the voltaic theory of contact) a forcible argument might be drawn from the following consideration. If there could exist a power, having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect, or, in other words, a perpetual motion. But the electro-motive force ascribed by Volta to the metals when in contact, is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be exerted with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition the probabilities are all but infinite."—§ 113, p. 32.

April 2.—The following papers were read, viz.:

"Additional note to the Eleventh Series of Researches on the Tides." By the Rev. William Whewell, B.D., F.R.S., &c.

As an appendix to his former memoir on tide observations*, the author gives in the present paper the results of observations made at Petropaulofsk, in the bay of Avatcha, in Kamtchatka, lat. 53° 1′ N., long. 158° 44′ E., by the officers and men of the Seuivine, commanded by the present Russian Admiral Lütke; and which were conducted with great care and perseverance. The height of the surface was noted every ten minutes, both day and night, and when near its maximum every two minutes.

It appears from these observations that the high water is affected in its time by a very large diurnal inequality, reaching the enormous amount of above four hours; while its height is only slightly affected by an inequality of that kind; the greatest alternate inequalities of height were something more than a foot. In the low waters, there appears a much smaller inequality in the times, seldom amounting to more than one hour; but with regard to height, the diurnal inequality is much larger than that for high water, reaching to three, or even four feet; and this in a tide of which the whole rise, from the lowest to the highest, rarely exceeds five feet. The theory of these phænomena is then discussed.

The results of another series of observations made in July 1827, at the port of Novo-Arkhangelsk, in the island of Sitkhœ, in Norfolk sound (lat. 57° 2′ N., long. 135° 18′ W.), are also given, and their theory considered.

A paper was also in part read, entitled, "On the Nervous System." By Sir Charles Bell, F.R.S.

April 9.—The reading of a paper, entitled, "On the Nervous System." By Sir Charles Bell, F.R.S., was resumed and concluded.

The author adverting to the papers on the nervous system, which he presented to the Royal Society nearly twenty years ago†, recapitulates the train of reasoning which originally led him to the inquiries in which he has been so long engaged, on the different functions of different classes of nerves, and adduces various pathological facts in corroboration of the correctness of the views he then entertained. With regard to the spinal nerves, cases are related where, in consequence of disease of the bodies of the vertebræ, the anterior columns of the spinal chord, and anterior roots of the nerves were affected, and paralysis of the muscles to which those nerves are distributed was produced, while the posterior column of the cord was uninjured, and the sensibility unimpaired. The author next considers the respiratory system of nerves, which he regards as being both muscular and sensitive, and describes as arising from a tract of the spinal cord, on the outside of the *corpus olivare*, and anterior to the *processus ad cerebellum*; and which constitute columns having no decussations with one another, as is the case with the other systems. The conclusion he originally formed, that both the phrenic and the spinal accessory nerves are provided for motion, which he had deduced from the anatomical fact of the former taking a direct course to the diaphragm, and the latter a circuitous one for the purpose of associating the muscles of the respiratory organs with those which act on the chest, is, he thinks, amply confirmed by subsequent experiments. He concludes his paper with some remarks on the supply of blood to the respiratory system of nerves, which supply, being derived from branches of the vertebral arteries, affords an explanation of several pathological phenomena.

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* [Abstracts of Mr. Whewell's former Researches on the Tides will be found in the Lond. and Edinb. Phil. Mag., vol. xv. p. 316.]

† [See Phil. Mag., first series, vol. lxiv. pp. 41. 119. 353. 442; and Lond. and Edinb. Phil. Mag., and Annals, 2d series, vol. vi. p. 135; also Lond. and Edinb. Phil. Mag., vol. vii. p. 138.]

A paper was also read, entitled, "On the constitution of the Resins. Part IV."[*] By James F. W. Johnston, Esq., M.A., F.R.S.

This paper contains the account of the continuation of the author's previous researches into the constitution of the resins, both as they occur in nature, and as they appear when extracted from the natural products by the agency of alcohol or æther. The great difficulty in this inquiry is to determine when the resin to be analysed is obtained in its normal state; and the author has endeavoured in each case to ascertain this point by repeated analyses of the resins prepared under different conditions. He thus arrives at the conclusion, that the resin of scammony extracted from crude scammony by alcohol, and heated to 260° Fahr., is represented by $C_{40}\ H_{33}\ O_{20}$, containing the largest amount of oxygen of any resin hitherto analysed. The resin of jalap, obtained by evaporating the alcoholic extract, and afterwards boiling it in water, is represented by $C_{40}\ H_{34}\ O_{18}$, and in the amount of oxygen it contains is only surpassed by the resin of scammony. It is interesting to remark that these two resins in their effects on the animal œconomy are as nearly related as these formulæ show them to be in chemical constitution.

The resin of labdanum, extracted by alcohol from the crude labdanum and evaporated, gave the formula $C_{40}\ H_{33}\ O_{8}$; but this extract, softened in the air and water, took up from it a bitter substance of a brown colour. After boiling in water, the pure resin is represented by $C_{40}\ H_{33}\ O_{7}$.

The Berengela resin, previously analysed by the author before he was aware of the conditions necessary to be attended to in order to obtain a resin in its normal state, is expressed by the formula $C_{40}\ H_{30}\ O_7$; and the resin of Retin asphalt, also previously analysed by the author, by $C_{40}\ H_{27}\ O_6$.

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[*] [See Lond. and Edinb. Phil. Mag., vol. xv. p. 327, for the former papers.]

The resin of ammonia, extracted by alcohol from the crude gum resin, is represented by $C_{40}\ H_{25}\ O_9$; the resin of opoponax by $C_{40}\ H_{25}\ O_{14}$; and that of assafœtida by $C_{40}\ H_{26}\ O_{10}$.

A striking relation appears between the formulæ for the resins of ammonia and assafœtida, the former being $C_{40}\ H_{25}\ O_9$, the latter $C_{40}\ H_{26}\ O_{10}$, as if the latter were merely a hydrate of the former. The author considers this relation, and concludes that it is only apparent, and that probably in neither of the resins does any of the hydrogen exist in combination with oxygen in the state of water.

This leads the author to inquire into the general action of a slightly elevated temperature on the resins, and he concludes that in all cases when a resin in its normal state is heated a few degrees above its melting point, it begins to suffer partial decomposition, accompanied by the solution of water, and *always* by more or less of a volatile, generally oily compound, sometimes containing less and sometimes more oxygen than the resin which has been subjected to heat. In the case of some resins, especially such as are agreeably fragrant, and are expressed by the second of the author's general formulæ $C_{40}\ H_{21} \pm x\ O_7$, benzoic acid is one of the products of decomposition at a moderate temperature. Thus the resin of dragon's blood gives only a trace of benzoic acid, with water and a red volatile compound; while the resin of benzoin gives much benzoic acid. Some resins give off volatile matters and diminish in weight long before they reach the fusing point; as is the case with the resin of benzoin, of which the melting point is high. With regard to the special action of such temperatures in altering the atomic constitution of the resins, the author finds that each resin undergoes a change, probably peculiar to itself, and probably depending on the nature of the organic radical it contains. Thus, the formula for the resin of retin asphalt ($= C_{10}\ H_{27}\ O_6$) by prolonged heating at the melting point becomes $C_{40}\ H_{27}\ O_5$. Ammonia resin ($= C_{40}\ H_{25}\ O_9$) by heating at 270° Fahr. *approaches* to $C_{40}\ H_{24}\ O_8$; there being, however, a slight excess of oxygen, and water not being the only volatile compound driven off.

The resin of opoponax, when thus heated the hydrogen, as in that of retin asphalt, remains nearly constant $= C_{40}\ H_{25}\ O_{14}$, approaching to $C_{40}\ H_{25}\ O_{12}$. The same is the case with the resin of assafœtida ($= C_{10}\ H_{26}\ O_{10}$), which by prolonged heating at about 250° Fahr., becomes $C_{40}\ H_{26}\ O_9$. These observations when multiplied are likely to assist materially in leading to *rational* formulæ, expressive of the molecular constitution of the resins.

In reference to the general questions, with a view to the solution of which the author undertook this investigation, he concludes:

1. That the resins are not to be considered as different compounds of one and the same radical, but rather as analogous groups of compounds of analogous radicals.

2. That as far as our present knowledge extends, all the *true* resins are capable of being represented by irrational formulæ, in which C_{40} is a constant quantity.

3. That the analyses contained in the present paper render necessary a slight modification in the general formulæ previously announced. The formula for the group of which colophony is the type, being $C_{40} H_{32} \pm x Oy$; and that for the group of which gamboge or dragon's blood is the type, being $C_{40} H_{24} \pm x Oy$.

The author announces a further continuation of these researches, in which the constitution of other resins will be given, and the relations of the resins to certain chemical reagents will be explained and illustrated.

The Society then adjourned over the Easter Recess, to meet again on the 30th of April.

GEOLOGICAL SOCIETY.

[Continued from p. 74 of the present volume.]

Dec. 18, 1839.—A paper was first read, entitled "Description of the fossil remains of a mammal, a bird, and a serpent, from the London clay," by Richard Owen, Esq., F.R.S., F.G.S.

The author commences by observing, that only a few months had elapsed since the highest organic animal remains known to exist in the London clay were those of reptiles and fishes; and that the danger of founding conclusions in Palæontology from negative evidence was perhaps never more strikingly illustrated than by the fact, that the first scientifically determined relic of a warm-blooded animal from that formation proved to belong to the highest order of that class, if man be excepted; and that besides those quadrumanous remains, there have since been discovered in the London clay underlying the coralline crag, near Kyson, in Suffolk, teeth of cheiroptera, and of a species probably belonging to the marsupial order[*].

Mr. Owen then proceeds to describe the fossils, the immediate objects of the communication.

1. The portion of the mammal was discovered by Mr. Richardson in the cliffs of Studd Hill, near Herne Bay, and belongs to a new and extinct genus of Pachydermata. It consists of a small mutilated cranium about the size of that of a hare, containing the molar teeth of the upper jaw nearly perfect, and the sockets of the canines. The molars are seven in number on each side, and resemble more nearly those of the Chæropotamus than of any other known genus of existing or extinct mammalia. They present three distinct modifications of the grinding surface, and increase in complexity from before backwards. The first and second spurious molars have simple sub-compressed crowns, surmounted by a single median conical cusp, with a small anterior and posterior tubercle at the outer side, and a ridge along the inner side of its base. They are separated by an

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[*] Annals of Natural History, Nov. 1839.

interspace nearly equal to the antero-posterior diameter of the first molar. The second and remaining molars are in close juxtaposition. The third and fourth molars form the principal difference between the dentition of the present genus and that of the Chæropotamus, being larger and more complex in the grinding surface. They present a sudden increase in size and change of form. The plane of the crown is triangular, with the base outwards, and the posterior and inner side convex: it supports three principal cusps, two on the outer, and one on the inner side; there are also two smaller elevations with a depression on the summit of each, situated in the middle of the crown, and the whole is surrounded with a ridge which is developed into a small cusp at the anterior and external angle of the tooth. The three true molars closely correspond with those of the Chæropotamus. The sockets of the canines indicate that these teeth were relatively as large as in the peccari.

The bones of the head are separately described: the palatal processes of the maxillary bones are shown to be rugous, as in the peccari; the eye to have been full and large, as indicated by the size of the optic foramen and the capacity of the orbit, equalling an inch in vertical diameter: the general form of the skull is described as partaking of a character intermediate between that of the hog and the hyrax, though the large size of the eye must have given to the physiognomy of the living animal a resemblance to that of the Rodentia.

These indications, Mr. Owen says, scanty though they be, of the form of a species nearly allied to the Chæropotamus, are extremely interesting, on account of the absence of similar information regarding that genus. The resemblance of the molar division of the dental system in the new genus, for which the name of Hyracotherium is proposed, and the Chæropotamus, is sufficiently close to warrant the conclusion, that the canines and incisors if not similar would differ only in form and proportion; and that hence it may be ventured to solve analogically some of the doubts entertained by Cuvier respecting the dental characters of the Chæropotamus, and to affirm confidently that it had canines in the upper as well as the lower jaw. The incisor teeth with the ossa intermaxillaria are wanting in the specimen of the Hyracotherium, and have not been found in any fragment of the Chæropotamus.

2. The remains of birds described in the paper consist of a sternum, with other bones, and a sacrum, the former belonging to the collection of the late John Hunter, in the Royal College of Surgeons, and the latter to the cabinet of Mr. Bowerbank. Both the specimens were obtained from Sheppey. The Hunterian fossil includes the sternum nearly entire, the proximal ends of the coracoid bones, a dorsal vertebra, the distal end of the left femur, the proximal end of the corresponding tibia, and a few fragments of ribs. Mr. Owen first shows, in approximating to which of the three great groups of birds, terrestrial, aërial, or aquatic, the Ornitholite belonged, that from the length of the sternum and the remains of the primary in-

termuscular crest or keel, it could not have been a strictly terrestrial bird, though these characters do not prove that it was a bird of flight, as they occur in the Penguins or other Brachyptera, which have need of muscular forces to work their wings as paddles under water. In the present fossil, however, from the lateral extent and convexity of the sternal plate, the presence and course of the secondary intermuscular ridges, the commencement of the keel a little way behind the anterior margin of the sternum, Mr. Owen says there is no affinity with the brachypterous family. The coracoid bones or posterior clavicles, he also shows are less available in determining the habits of the Ornitholite, as they relate much more closely to the respiratory actions than to the movements of the wings, and are strongly developed even in the Apteryx. There remained consequently for comparison the ordinary birds of flight; and of these, the native species, which resemble the fossil in size, first claimed Mr. Owen's attention. Though the sternum is not complete, yet sufficient remains to have enabled him to set aside the Gallinaceous, and those Grallatorial and Passerine birds which have deeply incised sternums, and to restrict the field of comparison to such species as have the sternum either entire, or with shallow posterior emarginations. After a rigid comparison of the minor structural details and pursuing it from the sea gulls and other aquatic birds upwards through the Grallatorial and Passerine orders, omitting few British species, and no genus, he at length found the greatest number of correspondences in the skeleton of the accipitrine species. The resemblance, however, was not sufficiently close to admit of the fossil being referred to any native genus of Raptores: the breadth of the proximal end of the coracoid removes it from the owls (*Strigidæ*), the shaft of the same bone is too slender for the Falconidæ; and the femur and tibia are relatively weaker than in many of the British Hawks or Buzzards. It is with the Vultures that Mr. Owen has found the closest agreement; but he says the fossil indicates a smaller species than any known to exist in the present day, and is probably a distinct subgenus.

The professed ornithologist, Mr. Owen remarks, may receive with reasonable hesitation a determination of family affinities arrived at, in the absence of the usual characters deduced from the beak and feet; but in the course of a long series of close comparisons, he says, he has met with so many more characters, both appreciable and available in the present problem, than he anticipated, that he confidently expects, in the event of the mandibles, the bones of the feet, or the entire sternum of the bird in question being found, they will establish his present conclusion, that the Sheppey ornitholite is referrible to a member of the group of Accipitrine Scavengers, so abundant in the warmer latitudes of the present world.

The Ornitholite in Mr. Bowerbank's museum consists of ten sacral vertebræ anchylosed together, as is usual in birds with a continuous keel-like spinal ridge. Four of the vertebræ are analogous to the lumbar vertebræ in the mammalia, and they are succeeded by five others, in which, as in the Vultures, the inferior transverse processes are not developed. This character, however, Mr. Owen says, is not peculiar to the Vulturidæ. Though the part of the fossil preserved is eminently characteristic of the class of birds, yet it is not calculated to throw light on the closer affinities of the species to which it belongs: nevertheless it supports rather than affects the determination of the Hunterian specimen. For the apparently extinct bird indicated by these fossils, the name of ***Lithornis vulturinus*** is provisionally proposed.

3. Mr. Owen commences his description of the remains of an extinct species of Serpent found at Sheppey, by pointing out the essential characters by which the vertebræ of an Ophidian Reptile are distinguished.

Vertebræ joined enarthrodially by a deep anterior transversely oblong cup and a corresponding prominent posterior ball, and further articulated by projecting posterior oblique processes, wedged like the carpenter's tenon into a mortice, excavated in the anterior oblique processes of the succeeding vertebra, supporting moreover on each side of the fore part of the body an oblong convexity for the moveable articulation of the rib, can belong, Mr. Owen observes, to no other than a reptile of the Ophidian order.

One of the specimens described in this portion of the memoir, consists of about 30 vertebræ possessing the above characters; also of a number of long slender ribs, having expanded concave vertebral extremities cemented irregularly together by a mass of indurated clay, and it forms part of the Hunterian collection of fossils; another specimen, consisting of 28 vertebræ, and some others of less magnitude, belong to Mr. Bowerbank's collection. All the specimens, Mr. Owen considers, are referrible to the same species, and they were all found at Sheppey.

The vertebræ in each specimen present the same conformation, and nearly the same size, being equal in this respect to those of a Boa Constrictor 10 feet long. They belong to the ordinary dorsal or costal series, and differ from those of the Boa and Python in their superior length as compared to their breadth and height. The ridge continued from the anterior to the posterior oblique processes on each side is less developed: the oblique processes themselves do not extend so far outwards; and the spinous process is narrower in its antero-posterior extent but longer. In the first two of these differences, the fossil agrees with the Linnæan Coluber and its subgenera, but differs from the Crotalus; and in the remaining points it differs from Crotalus, Coluber, Naja and Trigonocephalus. The long and comparatively narrow spine, the outward prolongation of the upper angle of the posterior oblique processes, the uniform convexity of the costal protuberance, the uneven or finely wrinkled external surface of the superior arch of the vertebra, are characters which distinguish these Ophidian vertebræ from those of any other genus of the order with which Mr. Owen has been able to compare them. He therefore proposes to call the species provisionally ***Palæophis Toliapicus***.

The ribs are hollow as in all land serpents.

From the agreement in the configuration of the under surface of the body of the vertebræ of the fossil with that in the vertebræ of the Boæ and Pythons more nearly than with the Colubri, and in none of the differences above noticed indicating any obstacle to the entrapping and destroying a living struggling prey, as well as from the length (11 feet) which it may be inferred the creature attained, Mr. Owen concludes it was not provided with poisonous fangs. Serpents of similar dimensions exist in the present day only in tropical regions, and their food consists principally of the warm-blooded animals. Mr. Owen therefore in conclusion states, that had no evidence been obtained of birds or mammals in the London clay, he would have felt persuaded that they must have coexisted with the ***Palæophis Toliapicus***.

ROYAL IRISH ACADEMY.

Feb. 24.—A communication was read, entitled, "Justification of Mrs. Somerville's Experiments upon the magnetizing Power of the more refrangible solar Rays*." By George James Knox, Esq. and the Rev. Thomas Knox.

Professor Morichini of Rome was the first to observe that steel, when exposed to the violet rays of the solar spectrum, becomes magnetic. Similar experiments were tried by Mr. Christie in 1824; but the most accurate experiments upon this subject have been performed by Mrs. Somerville in 1825, who determined that not only violet, but indigo, blue and green, develope magnetism in the exposed end of a needle, while yellow, orange, and red produce no sensible effect. As many philosophers have failed in repeating these experiments, we were induced, in the course of the summer, to undertake the investigation of this subject, "which has so often disturbed science." Having procured several hundred needles, of different lengths and thicknesses, and having ascertained that they were perfectly free from magnetism, we enveloped them in white paper, leaving one of their extreme ends uncovered. Taking advantage of a favourable day for trying experiments upon the chemical ray, (known by the few seconds required to blacken chloride of silver,) we placed the needles at right angles to the magnetic meridian, and exposed them for three hours, from eleven to one, to the differently refrangible rays of the sun, under coloured glasses. Those beneath the red, orange, and yellow, showed no trace of magnetism, while those beneath the blue, green, and violet, exhibited, the two first feeble, but the last strong traces of magnetism.

To determine how far the oxidating power of the violet ray is concerned in the phænomena, we exposed to the different coloured lights needles whose extremities had been previously dipped in nitric acid, and found that they became magnetic (the exposed end having been made a north pole) in a much shorter time than the others, and that this effect was produced in a slight degree under the red (when exposed a sufficient length of time), strongly under white glass; and so strong under violet glass, that the effect took place even when the needles were placed in such a position along the magnetic meridian, as would tend to produce, by the earth's influence, a south pole in the exposed extremity.

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* Phil. Trans. vol. cxvi. 1826. (Reprinted in Phil. Mag. First Series, vol. lxviii. p. 168.)

Conceiving that the inactive state produced in iron (as observed by Schœnbein) when plunged into nitric acid, sp. gr. 1·36, or by being made the positive pole of a battery, or by any other means, might throw some light upon the nature of the electrical change produced, experiments were instituted to this effect, which showed that no trace of magnetism could be thereby produced.

March 16. (Stated meeting.)—The following Gentlemen were duly elected Officers and Council for the ensuing year:—

President: Sir William Rowan Hamilton, LL.D.—*Treasurer:* Thomas Herbert Orpen, M.D.—*Secretary to the Academy:* Rev. Joseph H. Singer, D.D.—*Secretary to the Council:* James MacCullagh, LL.D. —*Secretary of Foreign Correspondence:* Rev. Humphrey Lloyd, A.M.—*Librarian:* Rev. William Hamilton Drummond, D.D.—*Clerk and Assistant Librarian*: Edward Clibborn.

Committee of Science:—Rev. Franc Sadlier, D.D., Provost of Trinity College; Rev. Humphrey Lloyd, A.M.; James Apjohn, M.D.; James MacCullagh, LL.D.; Rev. William Digby Sadleir, A.M.; Robert Ball, Esq.; Robert Kane, M.D.

Committee of Polite Literature:—His Grace the Archbishop of Dublin; Rev. Joseph Henderson Singer, D.D.; Samuel Litton, M.D.; Rev. William Hamilton Drummond, D.D.; Rev. Charles Richard Elrington, D.D.; Rev. Charles William Wall, D.D.; Rev. Thomas H. Porter, D.D.

Committee of Antiquities: — Thomas Herbert Orpen, M.D.; George Petrie, Esq., R.H.A.; Rev. Cæsar Otway; Very Rev. the Dean of St. Patrick's; Rev. James Henthorn Todd, D.D.; Henry J. Monck Mason, LL.D.; Aquilla Smith, M.D.

The President then appointed the following Vice-Presidents: His Grace the Archbishop of Dublin; the Provost of Trinity College; the Rev. Humphrey Lloyd; the Very Rev. the Dean of St. Patrick's.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At the meeting of this society on Monday, June 1, Mr. Hopkins made a communication respecting certain geological phænomena of elevation, and their probable connexion with the existence of volcanos. When a district is traversed by a number of faults of which the directions are nearly parallel, and the planes of which deviate (as is generally the case) from verticality, the mass contained between any two adjoining faults will form a perfect or truncated wedge, according as the line in which the planes of the faults, or those planes produced intersect, lies within or without the elevated mass. A double system of wedges will thus be formed, of which one part will have their thicker portions upwards and the other downwards. The latter will manifestly be acted on respectively by elevating forces greater in proportion to their mass than the former, supposing the uplifting force to act uniformly over the lower surface of the mass; and consequently after the fissures are produced by the elevation and extension of the mass, the lower system of wedge will be forced upwards more than the upper one, and thus the two systems will be relatively displaced in such a manner as to bring thicker portions of them in contact than in their original positions. The two systems being thus *jammed* into each other, the uplifted mass will be supported as an arch, provided the *abutments* on which the extremities rest be sufficiently firm to support the pressure thus thrown upon them.

If we conceive a vertical line to be drawn downwards from any point in which the plane of a fault meets the earth's surface, the line will not generally coincide with the plane; and it is found that the relative displacement of the portions of the mass on opposite sides of the fault is usually such, that that portion in which the above vertical line lies appears most elevated.

Mr. Phillips has established this law by numerous observations, as a very general one. In considering the relative positions of the two systems of wedges above-described, after their displacement, it will easily be seen how simply this law is generally thus accounted for.

It thus appears, also, how great horizontal forces, and consequently great horizontal displacements may be produced, for which it would be difficult perhaps so easily to account in any other manner.

Mr. Hopkins explained the probable bearing of the above views on the theory of volcanos in the following manner. The temperature of the earth, in accordance with actual observations at comparatively small depths, may be such at the depth of twenty or thirty miles, as would under the pressure of the atmosphere fuse most of the substances composing the surface of the globe. No complete fusion, however, of the matter of the earth at the above depth can take place, because it is shown by the observed amount of precession, that the thickness of the earth's solid crust must at least be much greater than that quantity. This apparent discrepancy can only be removed, it would seem, by supposing *pressure* an antagonist cause with reference to heat considered as the cause of fluidity. This still requires the verification of experiment, but here assuming it to be true, it is easily seen how the crust of the earth may be generally solid to any depth, while portions of it may be necessarily fluid, from the removal of the superincumbent pressure by the formation over it of an *arch* or a *dome* in the manner above described.

XXIV. *Intelligence and Miscellaneous Articles.*

URINE OF THE ELEPHANT.

It is shown by the experiments of MM. John and Vogel, that the urine of the elephant does not contain benzoic, but hippuric acid. M. Brandes has examined it and has obtained the same results, and his experiments prove that it contains a notable quantity of hippuric acid, and he procured it perfectly pure.

He found in this urine:

Hippuric acid combined with urea (hippurate of urea).
An azotized extractive matter, soluble in water and in alcohol.
Traces of a fatty matter and of a black resinous substance.
Hippurate of potash.
Chloride of potassium.
Sulphate of potash.
Carbonate of ammonia.
Carbonate of lime.
Phosphate of lime.
Carbonate of magnesia.
Mucus.

The urine of the elephant does not then form an exception to the constitution of herbivorous animals; and M. Brandes concludes from his experiments, that the urea is combined with acid, as demonstrated by the researches of MM. Cap and O. Henry. The elephant's urine contains a considerable quantity of earthy salts.—*Journal de Chimie Médicale*, Dec. 1839.

DELVAUXINE.

Mr. Sandall has twice analysed a mineral named Delvauxine, which has the formula $\dddot{F}^2 \underline{\overset{\cdot\cdot\cdot\cdot\cdot}{P}} + 24\dot{H}$ attached to it, and finds its composition to be very nearly as follows:

Sesquioxide of iron	52·37
Water	40·19
Silica	3·95
Phosphoric acid	2·62
Lime	·87
	100·00

The supposed composition denoted by the preceding formula is

Sesquioxide of iron	12·82
Water	69·23
Phosphoric acid	17·95
	100·00

It will be seen, by reference to vol. xiv., p. 474, of the Lond. and Edinb. Phil. Mag., that Delvauxine, according to M. Dumont, is composed of

Peroxide of iron	30·30
Water	41·30
Silica	4·00
Phosphoric acid	13·95
Carbonate of lime	10·10
	99·65

If the discoverer of the mineral will repeat his analysis, he wil probably discover some source of error in his processes, or it may turn out that the mineral is only a variable mixture of the elements of which it is composed, and not a definite species.—R. P.

ON THE REDUCTION OF POTASSIO-CHLORIDE OF PLATINA.

M. Parisot observes that chloride of platina is employed in laboratories to ascertain the presence of potash and its salts in solution; and being consulted as to a method of easily separating the platina of the double chloride, he found that it might be effected by means of zinc.

The process consists in taking the precipitate of chloride of platina and potash, or the liquid which holds it in solution, and completely dissolving it in water, and the solution is then to have sulphuric added to it. Into this acid liquid a cylinder of zinc is to be immersed; decomposition of water occurs, attended with the evolution of hydrogen and the formation of sulphate of zinc; this remains in solution, while the platina is precipitated in the state of a black powder.

The reaction is maintained, with the disengagement of hydrogen, by adding a little sulphuric acid, until all the platina is precipitated, which is known to be the case when the liquor is perfectly colourless.

When the platina is reduced it is collected on a filter, washed with boiling water, then dried, and afterwards treated with a little hot hydrochloric acid, to deprive it of any zinc which it may retain, and lastly it is to be washed with boiling distilled water.—***Journ. de Chim. Méd.*** Avril, 1840.

DR. BARRY ON THE CORPUSCLES OF THE BLOOD.

We are requested by Dr. Martin Barry to mention, that the rapid and incessant changes in the form of the blood-corpuscles observed by him in certain altered states, described in a paper read before the Royal Society, June 4, 1840, are caused, as he has subsequent reason to believe, by contiguous cilia.

ACECHLOR-PLATINA.

M. Zeise obtains this compound by triturating chloride of platina with a sufficient quantity of acetone to form a thin paste, and leaving the mixture in close vessels from 38 to 40 hours. During the solution of the chloride heat is given out, and an irritating vapour, mixed with hydrochloric acid, is evolved. A fluid, brownish, black compound, is at first formed, but the greater part becomes a crystalline mass in about 24 hours; when the fluid portion has been separated, the mass becomes of a yellow colour when treated with small portions of acetone on a filter. The mother liquor contains some acechlor-platina, which may be obtained by distilling it to dryness with the acetone which has been used in washing; the blackish mass obtained is to be treated on a filter with fresh portions of acetone.

The properties of acechlor-platina are, that when dry it is inodorous, has a metallic astringent taste, burns on the approach of a taper, with a greenish flame, and leaves metallic platina; when heated in a glass vessel by means of an oil-bath, it supports a heat of 383° Fahr. without emitting any odour and without apparent change; but when heated to about 397°, it begins to blacken and to yield an acid and penetrating odour. At 437°, in one experiment, the whole was converted into a black mass, with the disengagement of acid vapours, and a peculiar smell. After a heat gradually increased to 572° Fahr., it appeared to yield no further odour; and when heated to redness it gave only a slight acid smell. The residue is perfectly black before contact with the air, nor do any metallic particles afterwards appear in it. It does not fuse or swell up previously to decomposition.

Water dissolves but a small portion of acechlor-platina; the solution is at first yellow, but becomes brown after a few hours; the salt also, which does not dissolve, becomes brown by remaining under water. When heated with water it becomes first deep brown, and afterwards black, yielding some peculiar products; æther dissolves but little, alcohol rather more, especially when hot and the salt crystallizes unchanged; acetone dissolves much more; the solution is yellow, it probably takes up a thirtieth part when cold and a twentieth when boiling; the solution does not redden litmus unless water be present; concentrated hydrochloric acid does not act upon it without the assistance of heat; and the solution may be boiled without alteration. A solution of potash dissolves acechlor-platina completely; the colour is brown; when heated with ammonia, or in the dry way with potash, barytes or lime, its properties are changed. If copper be digested in a solution this compound is acetone; it is covered in a few hours with a black powder, on the addition of a little hydrochloric acid; in fact, this circumstance occurs immediately, and a little gas is evolved. Mercury acts similarly, but an amalgam is produced, and afterwards a black powder separates: when phosphorus is put into a solution of acechlor-platina in acetone an insoluble black powder is formed, containing platina, carbon and phosphorus; sulphur gives a soluble compound.

If a mixture of acetone and nitrate of silver, which is slightly opake, be added to a solution of acechlor-platina in acetone, an abundant precipitate of a pure yellow colour is immediately formed; but in two or three minutes it becomes black, and the mixture remains long turbid; nitric acid produces no effect when added to a solution of acechlor-platina.

An aqueous solution of chloride of potassium or sodium dissolves acechlor-platina more abundantly than mere water; the solutions are yellow, and suffer no sensible change by ebullition, which indicates the formation of a double compound; but if this actually occurs, it is much less stable than that which results from the action of these chlorides on combustible chloride of platina (by alcohol), and they differ probably in other respects.

The mean of several experiments gave as the composition of this substance,

Platina	53·5883
Chlorine	19·1010
Carbon	19·4260
Hydrogen	2·8980
Oxygen	4·9867
	100·

which, according to M. Zeise, are equivalent to

1 atom of platina	1233·260	53·6920
2 atoms of chlorine	442·650	19·2710
6 atoms of carbon	458·622	19·6660
10 atoms of hydrogen	62·398	2·7166
1 atom of oxygen	100·000	4·3537
		99·6993

Ann. de Chim. et de Phys., t. lxxii.

NITRITES FORMED BY DIRECT COMBINATION.

M. J. Fritzche forms these salts by passing nitrous acid, procured from the action of nitric acid upon starch, into water containing finely divided oxide of lead; the oxide is quickly converted into a white mass, which, on continuing to pass the nitrous acid gas into it, completely dissolves. The deep yellow solution thus obtained, yields by evaporation with a gentle heat a considerable quantity of nitrite of lead in yellow silky scales. A very small quantity of nitrate is formed in this operation; there is, therefore, no doubt that nitrous acid can combine directly with bases. — *L'Institut*, No. 341.

METEOROLOGICAL OBSERVATIONS FOR JUNE, 1840.

Chiswick.—June 1. Very hot and dry. 2. Thunder storm, with rain in heavy showers. 3. Fine. 4. Overcast. 5. Drizzly. 6. Cloudless and hot: heavy rain at night. 7—11. Very fine. 12. Overcast and fine. 13—15. Very fine. 16. Hot and dry. 17. Fine: showery. 18. Showery in the morning: windy. 19. Slight rain. 20—24. Very fine. 25—29. Cloudy and fine. 30. Hazy :rain.

The mean temperature was within a fraction of the average for this month. The quantity of rain was moderate. Westerly winds were unusually prevalent. On the whole the weather may be considered as having been favourable.

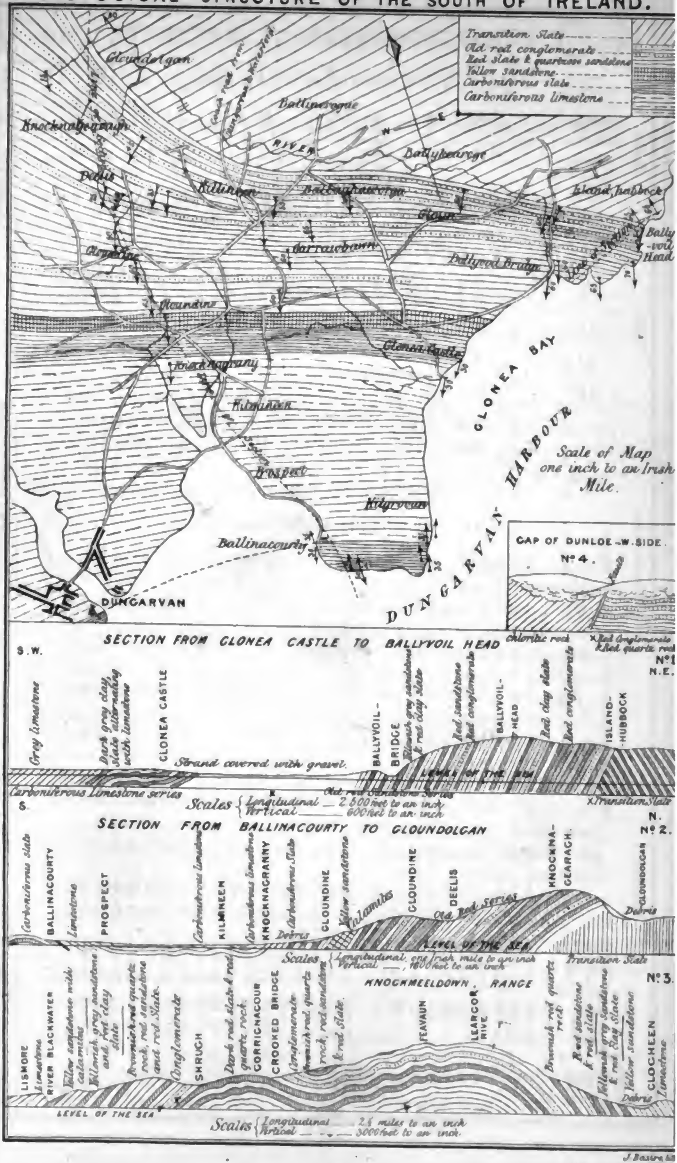
Boston.—June 1. Fine: Therm. 78° one o'clock. 2. Cloudy: Therm. at noon 53°: rain P.M. 3, 4. Cloudy: rain P.M. 5, 6. Cloudy. 7. Fine: rain early A.M. 8. Fine. 9. Cloudy: rain P.M. 10, 11. Fine. 12. Cloudy: rain P.M. 13, 14. Fine. 15. Cloudy. 16. Fine. 17. Fine: rain early A.M. 18. Cloudy. 19. Rain. 20. Fine. 21. Cloudy: rain P.M. 22. Fine: rain early A.M.: rain P.M. 23. Fine: rain P.M. 24. Fine. 25. Cloudy: rain A.M. and P.M. 26. Fine. 27, 28, 29. Cloudy. 30. Fine.

Applegarth Manse, Dumfries-shire.—June 1, 2. Mild with occasional showers. 3. Fine day: bright sunshine. 4. Cloudy but dry. 5. Rain in the evening. 6. Rain in the morning. 7, 8. Very fine day. 9. Showery all day. 10. Fine day: rain early. 11. The same, but fair. 12. Wet all day. 13. Very fine day. 14. Wet greater part of the day: thunder. 15. Wet afternoon. 16, 17. Stormy and wet afternoon. 18. Showery, but calm. 19. Wet A.M.: cleared up P.M. 20. Showery A.M.: cleared and fine. 21. Showery all day. 22. Rain A.M. 23. The same: cleared and was fine. 24. Fair all day and cold. 25. Showery. 26. Drizzling all day. 27. Showery. 28, 29. Beautiful summer day. 30. Wet morning.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. June.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·212	30·195	29·900	29·50	29·88	29·85	66·7	83·0	55·7	84	50	70	59½	50	s.	sw.	w.	sw.	...	...	...	...	58
2.	29·806	29·981	29·808	29·18	29·75	29·73	64·8	82·7	60·5	65	45	64	59	48	nw.	nw.	nw.	w.	·022	·17	...	...	60
3.	30·106	30·144	30·082	29·36	29·90	30·00	56·8	66·2	48·8	63	41	54	60	45	nw.	nw.	nw.	sw.	·294	·06	·19	...	48
4.	30·198	30·168	30·044	29·59	30·00	29·98	55·7	61·0	48·3	66	51	52·5	59½	46	nw var.	w.	calm	w.	...	·04	·06	...	50
5.	30·040	30·040	29·928	29·52	29·95	29·93	57·7	65·8	53·5	65	50	53	56	47	w.	w.	calm	ne.	·047	·07	·05	...	54
6.	29·938	29·919	29·799	29·38	29·78	29·76	61·8	66·2	52·2	77	55	59	61	47½	e.	se.	se.	ne.	·172	·28	...	0·17	54
☽ 7.	30·018	30·014	29·956	29·30	29·70	29·85	63·2	72·3	56·8	74	41	65	61½	52½	sw var.	w.	w.	s.	·333	...	·26	...	58
8.	30·072	30·045	29·949	29·45	29·84	29·78	63·7	73·8	52·5	75	57	67	67	40	se.	s.	calm	s.	...	...	...	...	53
9.	30·008	29·986	29·908	29·40	29·78	29·80	67·3	72·6	60·8	78	50	65	63	55	se.	sw.	sw.	sw.	...	...	...	...	60
10.	30·014	30·016	29·993	29·34	29·79	29·83	64·8	74·4	59·5	77	43	67·5	64	55	se.	w.	calm	sw.	...	...	·27	...	59
11.	30·100	30·082	29·974	29·42	29·82	29·77	64·3	71·7	52·8	74	59	66	64	45	s.	w.	w.	sw.	...	·01	...	...	55
12.	29·998	29·988	29·841	29·33	29·70	29·60	64·6	73·3	61·2	71	57	64	59	53	s.	sw.	calm	sw.	...	...	...	..	60
13.	29·992	30·045	29·965	29·35	29·75	29·92	63·5	71·2	59·0	77	45	59	63	49	w.	w.	n.	w.	...	...	·05	1·32	57
14.	30·096	29·997	29·970	29·48	29·73	29·65	65·7	72·8	56·5	74	47	63	62	48½	s.	sw.	n.	w.	...	...	...	...	59
○ 15.	30·054	30·027	29·884	29·43	29·74	29·60	64·8	75·3	54·4	77	56	63	62	50	ssw.	sw.	w.	sw.	...	...	...	...	59
16.	29·868	29·860	29·784	29·18	29·50	29·30	66·8	74·3	59·6	78	57	68	61	53	s.	sw.	w.	sw.	...	·09	...	...	61
17.	29·836	29·804	29·767	29·12	29·32	29·37	64·3	74·3	59·5	73	49	66	61½	51	s.	sw.	w.	sw.	·094	·22	·02	...	58
18.	29·870	29·945	29·858	29·19	29·50	29·60	61·8	71·6	52·2	70	49	61	56	45½	nw.	w.	w.	w.	·347	·01	...	...	53
19.	29·970	29·988	29·895	29·35	29·42	29·59	61·7	67·7	53·0	62	45	57	60½	49	s.	sw.	w.	sw.	...	·09	...	...	58
20.	30·150	30·201	30·103	29·50	29·82	30·00	61·2	72·6	50·6	75	52	60	61	47	w.	w.	w.	w.	·111	...	·08	0·73	54
21.	30·198	30·193	29·917	29·60	29·80	29·70	65·0	69·8	56·6	77	53	61·5	60	46½	s.	sw.	calm	w.	...	·13	...	...	56
☾ 22.	29·836	29·823	29·741	29·16	29·40	29·48	63·8	76·2	66·8	72	49	61	58	46½	s.	w.	w.	wsw.	·133	...	·71	...	55
23.	29·736	29·826	29·659	29·14	29·50	29·62	62·0	71·7	53·5	70	46	59	59	47½	sw.	w.	w.	n.	...	·06	·05	...	52
24.	29·838	29·845	29·805	29·32	29·70	29·74	54·3	67·6	49·4	64	44	53	56½	39	nw.	nw.	n.	nw.	·091	...	·03	...	50
25.	30·010	30·004	29·958	29·39	29·78	29·86	57·4	60·6	49·0	64	47	57	57½	47	nw.	nw.	w.	wnw.	...	...	...	...	48
26.	30·154	30·115	30·090	29·56	29·95	30·00	61·3	66·8	50·7	67	54	60	59	45	nw.	w.	nw.	wbyn.	...	...	·22	...	50
27.	30·196	30·146	30·056	29·60	30·00	29·97	63·7	65·4	56·2	74	57	61	61½	45	w.	w.	nw.	wbyn.	·116	...	...	0·15	54
28.	30·092	30·045	30·022	29·52	29·99	29·94	64·8	70·4	59·7	73	51	62	69	51½	nw.	e.	nw.	nw.	...	...	...	..	55
● 29.	30·100	30·041	29·993	29·54	29·94	29·87	61·4	70·6	56·6	74	51	60	67½	46½	s.	w.	calm	w—s.	...	...	...	...	58
30.	30·018	29·970	29·893	29·37	29·74	29·74	63·3	73·0	57·8	69	47	68	62	55	s.	s.	w.	w.	...	...	...	...	59
Mean.	30·017	30·015	29·918	29·68	29·415	29·761	62·6	71·2	55·5	71·96	49·93	61·5	61	48·2					Sum. 1·760	1·38	1·99	2·37	Mean. 55·5

GEOLOGICAL STRUCTURE OF THE SOUTH OF IRELAND.



J. Basire, lit

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

SEPTEMBER 1840.

XXV. *Reply to that part of* Mr. Weaver's *Paper relative to the Mineral Structure of the South of Ireland, which has appeared successively in the Numbers of the Philosophical Magazine for April, May, and June,* 1840. *By* Richard Griffith, *F.G.S. London and Dublin*[*].

[With a Plate.]

HAVING read with much care Mr. Weaver's paper published in successive Numbers of the Philosophical Magazine, relative to the mineral structure of the south of Ireland, &c., in which he endeavours to support his own views[†] in opposition to mine, respecting the order of succession of the rocks, and the geological position to which each is referable, I think it incumbent on me briefly to reply to those views and statements, which are quite inconsistent with facts carefully observed by me, and represented in my large geological map, as well as in several sections, exhibiting what appears to me to be the true order of succession of the strata of the south of Ireland: some of these sections have been published in the Journal of the Geological Society of Dublin[‡], and others in the Numbers of this Magazine for March last.

As the boundaries of the several rock districts as represented on the map, and their relative positions as exhibited in the sections, differ materially from those published by Mr. Weaver, they are considered by him to be imaginative compositions, and consequently unworthy of credit; but I hope to be able to prove that my map and sections are founded solely on a careful observation of facts, and in no case from hypothetical deductions.

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[*] Communicated by the Author.

[†] See vol. v. Transactions of the Geological Society of London, second series.

[‡] See vol. ii. part 1.

It is quite true that in the counties of Waterford, Cork and Kerry, I have made considerable changes in the colouring of my large geological map of Ireland, as compared with the small one appended to the Second Report of the Irish Railway Commissioners; but I should observe that, at the period of the publication of that map, and of the condensed Outline of the Geology of Ireland which accompanied it, I entertained doubts as to the accuracy of my views regarding the true position in geological sequence of some of the arenaceous and schistose strata of our southern districts. From the Wernerian character of my geological education, I found it almost impossible to conceive that quartz-rock and clayslate could be newer than the old red sandstone, or even belong to that series; and consequently when I found that the coarse conglomerates of the counties of Tipperary and Waterford, which rest *unconformably* on undoubted transition slate, alternated with, and were succeeded in an ascending order by red clayslate and red and gray quartz-rock, I was induced to refer the whole series to the transition class; but, judging from their unconformable position in regard to the older clayslate, I considered them to belong *to a newer transition series*, and described and coloured them as such.

Soon after the publication of the Railway Report I made a careful *re-examination* of our southern counties, and on comparing the well-characterized old red sandstones of the Slieve-naman and Galties mountains of the county of Tipperary, and of the Cahirconree or Slieve Meesh range of the county of Kerry, with the conglomerate series of the county of Waterford, I found them to be *identical in geological position*, and to present no difference in mineral character beyond a superior fineness in the grain of the upper members of the red slate series which occur in the valleys of the rivers Blackwater and Lee in the counties of Waterford and Cork. Under these circumstances it became evident that I should either obliterate the old red sandstone from my map, and class the whole with the transition series; or assuming the *unconformable position of the red conglomerate and slate* with regard to the older clayslates as an indication of a distinct period of formation, arrange them with that series of rock to which the name of old red sandstone has been given by British geologists.

In colouring the large geological map of Ireland, I adopted the latter principle, and thereby removed the anomaly which occurred in the small one, in which the conglomerate series of the mountains of Clare, Tipperary, and the north of Kerry were coloured as old red sandstone, while rocks occupying the same geological position, and presenting the same mineral character and composition (as in the Monavoullagh and Knockmildown mountains, and other portions of the counties of Waterford, Cork and Kerry) were coloured as belonging to the upper portion of the transition series. The investigation which led to this change having been very extensive and minute, enabled me to make numerous corrections in the detail of the outlines of the rock districts throughout the southern counties, and I think it right firmly to assert that every one of those subdivisions to which Mr. Weaver has applied the term *gratuitous*, has been the result of careful observations laid down on the spot, on a map on a large scale. Whether the subdivisions of the strata I have made may or may not eventually be considered as indicative of the boundary of distinct *formations*, still they will be valuable as indicating the boundaries of different rocks presenting distinct mineral characters and fossil remains. Thus, in my map and sections I have endeavoured to represent with accuracy the relative positions of the several rocks as they occur in geological sequence, and when a more correct nomenclature shall be established, it can be applied to both without difficulty and without *altering a single line*.

Mr. Greenough, in his introductory memoir to the new edition of his geological map of England, has classed the old red sandstone with the graywacke formation, and has quoted from Dr. M'Culloch's System of Geology, the characters of the formation as exhibited in Scotland, which he thinks are equally just when applied to the west of England, and in my opinion correspond very accurately with those of Ireland, particularly of the south; but I still adhere to the propriety of separating these strata from the transition class on account of their *almost universal unconformability**, which forms a stronger line of distinction between the transition slate and the old red series than we find between the old red and the strata which succeed it, where no unconformability is observed, each suite, in the ascending series, succeeding the other in regular succession without any break, as far up at least as the new red sandstone; consequently, there is really more difficulty in determining the line between the old red sandstone and the carboniferous limestone series, than between it and the transition slate†.

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* The only examples of conformability that I am acquainted with, occur in the west of the counties of Cork and Kerry. See paper by me in the Lond. and Ed. Phil. Mag. for March last, pp. 161—175

† The lower beds of the carboniferous limestone in many parts of Ireland, alternate with yellowish gray sandstone, which graduate into old red.

As to the term old red sandstone, I agree with most others in thinking it objectionable; but if this rock occurs in similar positions and presents similar mineral characters in the west of England, in Scotland, and in Ireland, a local name, as "Devonian," cannot be considered suitable, and the provincial term *killas*, recommended by Mr. Greenough, though preferable, as not indicating any particular locality, does not appear to be applicable to a general series of rocks.

But I am forgetting the object of this communication, and must return to Mr. Weaver. In common with all geologists connected with Ireland, I feel much indebted to him for his laborious investigations in that country; and although I differ from him in many most material hinging points, still I fully admit that he has effected much that is accurate and valuable. Entertaining such feelings towards him, I regret to find that he has not entered upon the discussion of the differences between our respective geological labours with the fairness I should have expected, as, in making his strictures on my geological maps and papers, he has compared the explanation which accompanied the production of *my large geological map*, with the *small one* and the "Geological Outline" appended to the Railway Report. As my large geological map was exhibited at the meeting of the British Association at Newcastle in 1838, at the time I read my paper on the geological structure of the south of Ireland, and as in that paper I mentioned the change that I had made in relation to the old red sandstone series of the southern counties, Mr. Weaver in considering that paper should have referred to the map which it was meant to illustrate, and not to the small geological map, the discrepancies in the colouring of which, as already expressed, it was intended to correct. Similar observations are applicable to the papers, also referred to by Mr. Weaver, read before the Geological Societies of London and Dublin, in May and June, 1839, both of which, as mentioned in the commencement of those papers, were communicated on the presentation of a copy of my large geological map, published in March of the same year.

Instead of comparing these several papers, all of which refer to the geological structure of the south of Ireland, with the large geological map, Mr. Weaver has compared them with the small map, and the "Geological Outline" which accompanied it; we need not therefore be surprised at his observing differences and incongruities, and through them attempting to throw discredit on everything which I have done.

It is true that Mr. Weaver has introduced notes in several places referring to the large geological map, in the first of which he states, "that his paper was drawn up before he had seen another geological map on a large scale, published in 1839;" and he observes, "that as the alterations that have been made in the new map do not *materially* interfere with the course of his argument, which in the first instance bears on the map attached to the outline, he has left the text unaltered." Now in his text Mr. Weaver does not confine his observations to the outline and small geological map, but refers equally to the three papers read by me before the British Association, and the Geological Societies of London and Dublin, the whole being compared with the small geological map, though at the period of the publication of his paper in April 1840, my large geological map had been in the possession of the London Geological Society for eleven months, and was to be seen exhibited in Mr. Gardner's, in Regent Street, for upwards of a year; so that it does appear extraordinary, that Mr. Weaver, who in the commencement of his paper alludes to my several communications above-mentioned, in each of which my large geological map is mentioned, should not have seen it when he drew up his paper for the Philosophical Magazine.

With this explanation I will conclude my observations on the subject of the discrepancies noticed by Mr. Weaver between my small geological map, and the sections and papers intended to illustrate the large one.

In comparing Mr. Weaver's geological map of the south of Ireland with mine (the large one), it must be admitted that the differences *are very considerable*, both in the great scale and in the detail. The latter is not to be wondered at, as I possessed much better maps, and had many opportunities of examination in almost every locality; but in regard to the great features, some explanation appears to be necessary. Mr. Weaver considers the whole of the schistose strata situated to the south of the river Suire in the county of Waterford, and thence to the south coast of the county of Cork, to belong to the transition series, on the northern part of which, particularly on the summits of the Knockmildown and Monavoullagh mountains, strata belonging to the old red sandstone series are superimposed in an unconformable position; and that the limestone of the valleys of the rivers Suire and Blackwater belong to the carboniferous series, while that of the valleys of the rivers Bride, Lee, &c. is transition, and alternates with the transition slate.

According to my view, the transition slate within the abovementioned limits is confined to the dark gray slate district south of the conglomerate of the valley of the Suire and east of that of the Monavoullagh mountains; while the conglomerate of these mountains rests unconformably on the slate strata to the north and east, and *dipping* to the southward, forms the substratum or base of the whole of the arenaceous, quartzose and schistose strata to the southward; and consequently, as these conglomerate beds are admitted by Mr. Weaver to belong to the old red sandstone series, the strata which rest upon them cannot belong to the transition class. I am also decidedly of opinion that the limestones of the valleys of the Suire, the Blackwater, the Bride, and the Lee, all occur in the same geological position, being placed on the top of the series, and that as the limestone of the valleys of the Suire and Blackwater are admitted by Mr. Weaver to be carboniferous, all the others must likewise belong to the same series.

In proof of the accuracy of these views, I formed with great care a section passing through this district nearly in a north and south direction; and if this section exhibits a correct representation of the relative positions of the several rocks, Mr. Weaver's views must be erroneous.

This section was exhibited at Newcastle in 1838; at the Geological Society of London, in May, 1839, in illustration of my paper; and subsequently at the Geological Society of Dublin, in June, 1839, and has since been published in the Journal of that Society.

I shall now proceed to consider Mr. Weaver's objections to my sections, both as to the principle of their construction and their accuracy.

He objects to the principle of making the scale of the height much greater than the scale of length, as he observes that "without considerable labour it leads to inaccuracy, as such sections, unless done correctly, instead of conveying precise information, tend rather to mislead the judgement." I quite agree to the position that an inaccurate section will tend to mislead; but if a section be made with care, and the relative positions of the rocks be accurately laid down according to the respective scales, no error can arise, though the angle of the dips will be necessarily increased.

The direct advantage to be derived from making the scale of height greater than the scale of length is, that supposing the scale of 8 to 1 be taken, the length of the paper on which the work is laid down is but $\frac{1}{8}$th of the length which it would be, if equal scales of length and height were adopted; and in long sections this is most important, by bringing the whole subject under the eye at one time, instead, as must otherwise be the case, of having it in a long roll, or different portions placed one beneath the other, by which means the continuity is broken.

Suppose the scale of height adopted for a section of a mountainous district be 2000 feet to an inch, and the distance between the termini be 100 miles, on equal scales, the length of such a section would be 22 feet; while by adopting a scale of 8 to 1 for the height, the length would be but 2 feet 9 in., and if carefully constructed the latter would give as clear a representation of the structure of the country as the other.

In regard to the second point, namely, the *accuracy* of my sections, Mr. Weaver observes, " that they appear to him in many respects drawn rather according to the conceptions of their author, than the occurrences in nature." This is certainly not complimentary; but as my sections through the same district differ in many important points from those made by Mr. Weaver, I cannot be surprised at the opinion: my consolation is, that should I prove mine to be correct, his opinion of my work will be applicable to his own.

The part of my section near the east coast to which Mr. Weaver objects, is from the valley of the river Suire to the south coast in the county of Cork; and first, as he observes, respecting " that part which lies between the valley of the Suire and the vale of Dungarvan, which latter extends westward to the Blackwater."

As far southward as the conglomerate of the Monavoullagh mountains, which rests unconformably on the old clayslate of the county of Waterford, there is no difference of opinion between Mr. Weaver and me; we both consider the limestone trough of the Suire to be carboniferous limestone, and the red slate and conglomerate to be the old red sandstone; but Mr. Weaver states that I am incorrect in making the old clayslate to the south of the Suire dip north; he says, the dip is to the *south*. Now I have examined the stratification with great care, both previously and subsequently to the publication of his paper, and I must state, that in the line of my section from the hill of Carrick southward by Millvale to Rathcormuck, the general dip of the *cleavage* in the old slate series is to the south, but the dip of the *strata*, as determined by the sedimentary lines, is to the northward; consequently Mr. Weaver in this case must have mistaken *cleavage for stratification*. It should be observed in this place, that although the slate strata dip towards the north, they are not conformable with the overlying conglomerate of the valley of the Suire, the latter dipping to the north at an angle of 80°, while the ends of the slate beds, where the junction is clearly visible, abut obliquely against the conglomerate, and dip 30° to the west of north at angles varying from 35° to 60°.

We may next consider the Monavoullagh conglomerate, which in his map Mr. Weaver shows as an insulated tract, surrounded by transition slate; I, on the contrary, show in my section a precipitous escarpment to the north, but a gradual declivity towards the *south* and south-west. If Mr. Weaver be correct, his conglomerate, which he represents as a mountain cap, must rest unconformably on the transition slate on the south as well as on the north side; now, I positively assert that such is not the fact, but that the alternating series of red clayslate and conglomerate, which presents so striking an escarpment on the north face of the mountain range, *forms the lower part of the red slate series*, and that proceeding from the summit of Crotty's rock to the southward we regularly ascend in the series, till at length in approaching the Blackwater near Lismore, we meet the yellow sandstone with calamites, which in so many localities alternates with the carboniferous limestone, that I have been induced to consider it rather as the lowest member of the carboniferous limestone than the upper part of the old red sandstone series*. Now if I am right in this position, what becomes of Mr. Weaver's mountain cap, or of the occurrence of the old transition slate to the southward of the Monavoullagh mountains? As an additional proof of the correctness of my views on the subject, I have made a careful examination of the district *within the last month*, and have prepared a plan which has been constructed with great care, and which, I think, will set the matter at rest.

The plan represents that portion of the county of Waterford extending from Ballyvoil Head in a western direction, along the valley of the Ballyvoil river to Gloundolgan, a distance of four miles, and southward from thence to Dungarvan. In this district we have the unconformable outgoing of the conglomerate of the Monavoullagh mountains, extending in an eastern direction *without interruption* to the coast at Ballyvoil Head. Now Mr. Weaver states† in his paper, "that there is no apparent connexion whatever between the horizontal sandstone and conglomerate of the Monavoullagh range and those beds of conglomerate and red slate of the coast which continue eastward from Dungarvan in several separate discontinuous bands interstratified with the other transition rocks." In opposition to this opinion, I beg to state, that the alternating strata of red clayslate and conglomerate of the Monavoullagh escarpment, does extend uninterruptedly from Crotty's rock to the shore at Ballyvoil Head, where they rest equally in an unconformable position on the transition slate.

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* See Lond. and Ed. Phil. Mag. for March, 1840, p. 173.

† See Lond. and Ed. Phil. Mag. for April, 1840, p. 279.

The contact of the conglomerate with the transition slate is clearly visible at Island Hubbock, about a quarter of a mile north of Ballyvoil Head, and their unconformability is unquestionable, as the conglomerate rests upon the upturned ends of the slate, and strongly adheres to them. In illustration of this fact, I beg to refer to the section No. 1, which gives an accurate representation of the red slate and conglomerate strata as they occur, resting unconformably on the dark gray transition slate, from Island Hubbock westwards. The succession is as follows:

1. Unconformable base composed of alternations of blackish gray and reddish-gray clayslate with chlorite slate and gray quartz-rock, the blackish-gray clayslate greatly predominating; dip of strata from 10° to 20° east of north, at angles varying from 60° to 75°.

2. Alternations of reddish-gray and red conglomerate, reddish-gray compact sandstone, and dark-red slate, the conglomerate predominating; dip of strata about 35° west of south, at angles varying from 60° to 80°. The thickness of these alternating strata is about 300 feet.

3. Dark reddish-gray clayslate, reddish-gray quartz-rock, and sandstone, the slate predominating: thickness about 300 feet.

4. Dark red micaceous sandstone, and red clayslate, with occasional beds of conglomerate, the sandstone predominating; dip 25° west of south at angles varying from 60° to 85°: thickness about 750 feet.

5. Dark-red slate, red quartz-rock and sandstone, the slate predominating: thickness about 300 feet.

6. Brownish-red quartzose rock, red clayslate, and yellowish-gray sandstone, the sandstones predominating: dip 20° west of south, on an average at an angle of 65°: thickness about 600 feet.

To the south of the yellowish-gray sandstone and red slate the strata are concealed from view by diluvial matter for a distance of about half a mile, beyond which alternations of dark-gray carboniferous slate and limestone are visible, both of which contain abundance of fossils belonging to the carboniferous limestone series.

I also give a section of the strata as they occur, from the unconformable contact of the red slate and conglomerate with the transition slate at Gloundolgan, to the coast at Ballynacourty in the harbour of Dungarvan, in which the succession of the strata between the lower conglomeritic base, the yellow sandstone, the carboniferous slate, and superincumbent carboniferous limestone are clearly shown.

Now if we compare these sections with that originally published by me as extending southward from Crotty's rock in the Monavoullagh mountains to Lismore, we find exactly the same suite interposed between the conglomerate base and the carboniferous limestone of the valley of the river Blackwater.

It is true, that the distance between Crotty's rock and Lismore, is greater than between Gloundolgan and Knocknagranny, in section No. 2, or between Ballyvoil Head and Ballyvoil bridge, in section No. 1; but it will be observed by referring to the plan, that the conglomerate and red slate at the coast dip to the south at an angle of 70°; while at Gloundolgan, four miles west of Ballyvoil Head, they dip south at an angle of 45°; further to the westward the dip is south at an angle of 15°; and as we approach the summit of the Monavoullagh mountains at Crotty's rock, the strata effect a nearly horizontal position. Owing to this circumstance, though the section is complete at Ballyvoil Head in the horizontal distance of one mile and a half, yet between Crotty's rock and Lismore the same suite occupies a horizontal distance of eighteen miles.

Having stated these facts, I think it unnecessary to do more than observe that Mr. Weaver's argument in proof of the impossibility of the red slate and conglomerate at Crotty's rock composing a part of the same series with the red slate and conglomerate of Ballyvoil Head, namely, *the nearly horizontal position* of the one, and the highly inclined angle of the strata of the other, is untenable; and I confess I am surprised at such an argument being used, as every practical geologist must be aware of its weakness.

The northern part of Mr. Weaver's section between the Suire and the Blackwater does not take the same line as mine, as his crosses over the Knockmildown mountains, and mine over the Monavoullagh range situated to the east; but to enable me to test the accuracy of his section, I have made one nearly in the same line. (See section, No. 3. in the plate.) If we compare this section with Mr. Weaver's, nothing can be more dissimilar. Mr. Weaver's section represents a base of graywacke slate, which supports unconformably a cap of old red sandstone; but according to my section, it is evident that e entire mountain range belongs to the old red sandstone series. The anticlinal axis is exposed to view in the valley of the river Ownashad, at Corrignagour, three miles north of Lismore: it consists of alternations of dark-red slate and dark-red quartzose rock, the slate predominating. These strata are succeeded both on the north and south sides of the axis by beds of brownish-red quartz-rock, red sandstone, and red clayslate, occasionally associated with fine-grained conglomerate, a thin bed of which appears at the surface on the south side of the axis at Shrugh, and on the north side at Crooked Bridge on the road from Lismore to Clogheen. In the lower part of this series the quartz-rock and sandstone predominate, but in the upper part the red clayslate is the prevailing rock.

Descending the hill on the north side towards Clogheen, and on the south towards Lismore, we find the last-mentioned rocks succeeded by alternations of gray sandstone and red clayslate, beyond which we have the limestone series alternating in the commencement with dark-gray clayslate. There is much diluvial matter at the base of the declivity on the north side, and in consequence the yellow sandstone with calamites is not visible there; although to the north of Lismore, at Reaf, and in other parts of both valleys, it may be observed associated with the dark-gray slate, which alternates with the lower beds of the carboniferous limestone.

If we compare the succession of rocks which form the stratification of the Knockmildown mountains, as above described, with those that occur at Ballyvoil Head and Gloundolgan, it would appear that the lowest visible rocks of the Knockmildown range belong to the *upper part* of the old red sandstone suite, which will account for the absence of the thick beds of conglomerate which abound in the lower part of the series in the Monavoullagh range, as at Crotty's rock, Gloundolgan and Ballyvoil Head.

In my paper printed in the Journal of the Geological Society of Dublin*, I have entered so fully into my reasons for considering that the limestones of the valleys of the Suire, the Blackwater, the Bride, the Lee, &c., occupy the same geological position, both by the order of succession of the strata and by fossils, that I do not think it necessary to discuss the subject a second time; and conceiving that I have shown that the red slates and conglomerates which overlie the conglomeritic base at Crotty's rock, at Gloundolgan, and at Ballyvoil Head, occupy the entire space between the dark-gray transition slate and the carboniferous limestone, I think I have substantiated my case, and shown that the strata situated to the south of the unconformable junction with the transition slate of the county of Waterford, do belong to a *newer series*, to which, for reasons already given, I have applied the name of old red sandstone in my large geological map, and in the papers referable to it. I shall, however, reply to one statement of Mr. Weaver's in regard to the stratification of the ridge interposed between the valley of the river Blackwater and the Bride, which, he observes†, "are said by Mr. Griffith to partake of a similar composition to that of the northern side of the Blackwater near Lismore, &c., forming in the centre of the ridge an anticlinal axis." He further observes, "The anticlinal axis I have not seen, the dip which I observed *being throughout to the south*," &c.

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* See vol. ii. part 1.

† See Lond. and Ed. Phil. Mag. for April, 1840, p. 228.

Now I must assert that the anticlinal axis *does exist*, and is clearly visible in the section of the strata of this ridge, which is exposed to view on the west bank of the river Blackwater, which in this locality takes a southern course, and cuts through the ridge between Killahally, opposite to Dromana Castle on the north, and Camphire on the south, as may be clearly seen by reference to my large geological map. The anticlinal axis is visible nearly in the centre between Camphire and Killahally, the strata at the axis and on either side consisting of alternations of red quartzose rock and red clayslate, which are succeeded both on the north and south by alternations of gray quartz-rock and red slate, those to the north dipping north at angles varying from 60° to 85°, and those to the south dipping at angles varying from 35° to 40°. These strata are succeeded on the south side by yellow sandstone containing calamites, and lastly, by alternations of limestone and black clayslate. On the north side the yellow sandstone is not visible, owing to a covering of diluvial matter, but the limestone alternating with black clayslate is visible immediately to the north of Killahally, dipping to the north at an angle of 60°; consequently, there can be no doubt of the identity of the limestone of the valley of the river Blackwater at Lismore, and thence to Dungarvan, with that of the Bride at Tallow, Camphire, &c.

It is to be observed, that the limestone of the valley of the Blackwater immediately to the south of Lismore, as represented in my section, published in the Journal of the Geological Society of Dublin, dips to the south at a very high angle; but fortunately this is not *universally* the case, as at Killahally, three miles south-east of Lismore, the strata, as above-stated, dip to the north, which in regard to the trough-shape of the limestone, proves by observation what I had previously supposed to be the case by induction.

In like manner I am prepared to follow my section in detail from the valley of the Bride to the south coast at Cork Head, and to show that at each point the *strata do actually dip* in the direction exhibited on the section; but I must remark in reference to the section published in the Journal of the Geological Society of Dublin, that in one point, namely, on the west side of Cork harbour, the lithographer has represented the beds of the carboniferous limestone as abutting against the subjacent carboniferous slate, while in the original drawing, as in nature, they are conformable.

I shall now make a few remarks with regard to Mr. Weaver's section as compared with my own, and particularly with reference to the position of the limestone of the several troughs in which it occurs.

We agree with respect to the limestone of the valleys of the Suire and the Blackwater, both considering it to be carboniferous; but what are the geological circumstances under which these limestones occur? They both rest conformably on the red slate and conglomerate series, which in the valley of the Suire Mr. Weaver calls *old red sandstone*, but in the valley of the Blackwater *transition slate*. In respect to the latter, I have shown that he is mistaken by means of my original section, and the sections from Ballyvoil Head and Gloundolgan; but Mr. Weaver in his section exhibits the carboniferous limestone of the Blackwater *at Lismore* in the form of a trough, the north side dipping to the south, and the south side to the north, which is not the fact; as in that locality, as already mentioned, the carboniferous strata dip to the south on both sides of the valley, on the north side at an angle of 30°, and on the south at an angle of about 60°.

If Mr. Weaver had carefully examined the dips of the strata, he must have observed this fact; and, following the principle he has adopted in other places, he should have included this limestone in his transition suite; but knowing this was not the fact, from its connexion with the great limestone field of Ireland, he has represented this rock at Lismore as a trough having reverse dips on the opposite sides of the valley.

We next come to the limestone of the valley of the Bride at Tallow, which Mr. Weaver in his section truly represents as a trough having dips in opposite directions; but this limestone, though undoubtedly the same in geological position, in lithological character, and in fossils, as the limestone of the Blackwater, he makes transition, though from its position and true trough shape, he must have considered it to rest on the top of the red slate series which lie beneath it on the north side of the valley dipping to the south, and on the south side to the north.

I shall now make a few observations respecting the extensive district represented by me as carboniferous slate, which occupies the greater portion of the south of the county of Cork, and which Mr. Weaver has included in his transition district. In geological position it rests upon the red slate and conglomerate series; near its commencement it is usually interstratified with gray or yellowish-gray sandstone and arenaceous slate, which frequently contain calamites, and in some cases, as at the Old Head of Kinsale, where the strata are unusually compact, the sandstone passes into quartz-rock, but it still exhibits the calamites.

In many localities the carboniferous slate includes beds of limestone, which, together with the slate, contains fossils similar to those which occur in the lower beds of the carboniferous limestone. This is the case at Blackball Head*, on the north-western extremity of Bantry bay; also at Brickeen island, near Killarney; at Kenmare†; at Clonea Castle on the coast of the county of Waterford, south-west of Ballyvoil Head; at Goat island in Ardmore bay on the same coast; at Kilnamack, near Knocklofty bridge; on the south side of the valley of the Suire above Clonmel, and many other localities.

The following are a few of the fossils which occur in the beds of carboniferous slate which are interstratified with the limestone at the following different localities:

Fossils which have been discovered in the carboniferous slate beneath and interstratified with the admitted carboniferous limestone at the undermentioned localities.			Fossils which have been discovered in the carboniferous slate beneath and interstratified with the limestone of Killarney and Cork Harbour, supposed by Mr. Weaver to belong to the transition series.	
Ballinacourty, Dungarvan Bay.	Clonea Castle, Dungarvan.	Kilnamack, near Knocklofty Bridge, valley of the Suire.	East of Brickeen Bridge, near Killarney.	Below the Cork limestone at Renniskiddy, Shanbally, and Killingly.
Calamopora tumida				
Retepora tenuifila				
	Retepora laxa			
	Ret. membranacea	Ret. membranacea		Ret. membranacea
Turbinolia fungites	Turb. fungites		Turb. fungites	Turb. fungites
Amplexus Sowerbii	Amplexus Sowerbii			
Producta lobata	Producta lobata		Producta lobata	
	P. depressa	P. depressa	P. depressa	P. depressa
	P. scabricula			
	P. aurita			P. aurita
	P. martini			
	Leptæna lata			Leptæna lata, (Sow.)
	Spirifera attenuata	Sp. attenuata		Sp. attenuata
	Sp. bisulcata	Sp. bisulcata	Spirifera bisulcata	Sp. bisulcata
	Sp. lineata			Sp. lineata
	Sp. resupinata			Sp. resupinata
Spirifera glabra	Sp. glabra			
	Sp. symmetrica			
	Sp. crenistria	Sp. crenistria		Sp. crenistria
	Sp. semicircularis			
	Sp. filiaria	Sp. filiaria		Sp. filiaria
		Sp. laminosa		
		Sp. arachnoidea		Sp. arachnoidea
			Sp. imbricata	
				Sp. cuspidata
Pleurorhynchus elongatus				
Pl. minax				
Nucula tumida	Nucula tumida			
Nu. undulata				
	Asaphus gemmuliferus			As. gemmuliferus
	As. truncatulus			
	As. quadrilimbus			

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* In the dark-gray clayslate of Blackball Head I found *Spirifer semicircularis*, Phillips.

† *Retepora membranacea* occurs in the carboniferous slate at Roughty bridge above Kenmare, where it rests conformably on yellow sandstone containing calamites.

I shall next advert to Mr. Weaver's observations respecting my section which extends from Brandon bay, at the extremity of the peninsula of Dingle in the county of Kerry, in an eastern direction across the Cahirconree or Slieve Meesh range of mountains, thence traversing the limestone valley of Castle island, and terminating in the great millstone grit of Munster*.

Of this section, Mr. Weaver observes, "That the portion which more immediately claims attention, is that which extends from the summit of the old red sandstone of the Slieve Meesh range, to the carboniferous limestone of Castle island. The former is represented as constituting nearly a cap or sheet, formed on an inclined plane, from west to east, the strata corresponding and succeeding each other in that direction to the junction with the carboniferous limestone." Mr. Weaver further observes, that he "knows of no such arrangement; on the contrary, the strata of the old red sandstone are accumulated to a great depth, and certainly, in some quarters, at least to the level of the sea, being disposed in a gently arched form from north to south."

In the latter observation Mr. Weaver is perfectly correct: in fact, the Slieve Meesh or Cahirconree range may be compared to a semicone, having its base to the west and apex to the east; the western base presents a precipitous escarpment, the lower region of which is occupied by highly-inclined strata, consisting of dark gray clayslate, which on the outer edges alternates with purple clayslate; the nearly upright ends of these strata are covered by a series of unconformable beds of compact red sandstone and red conglomerate, alternating with coarse red slate; near the summit, these strata present a nearly horizontal arrangement, in a north and south direction, but they *dip to the eastward* at a *moderate angle*. On approaching the declivities of the cone, both to the north and south, the conglomerate strata dip rapidly, on the one side, towards Tralee bay, and on the other, towards the bay of Castlemaine. From the summit, the eastern dip is continued towards the apex of the cone at Currens; but the lower bed, which rests on the ends of the transition slate, does not continue to form the surface, but dipping more rapidly to the eastward than the ridge of the hill, it is succeeded by a number of beds of conglomerates and coarse slate, each cropping out to the westward. At the eastern extremity of the range, or the apex of the cone, the upper portion of the red slate and conglomerate series is succeeded in a conformable position by beds of fine-grained yellowish-gray sandstone of the carboniferous series, some of which contain calamites and many obscure casts of bivalves, one of which was named by Mr. Sowerby as the *Avicula modiolaris* [*]. The upper beds of the sandstone alternate with a dark-gray, and occasionally blueish-gray quartzose-rock, and they are succeeded by dark-gray clayslate, alternating with carboniferous limestone. These strata, at Riversville quarry, which I have lately visited, dip to the east at an angle of 15°. It was here that Mr. Weaver could discover traces only of the graywacke formation; now the upper part of the quarry just mentioned contains thin beds of carboniferous limestone; and immediately to the south and east we have large quarries of that rock partaking of the same strike and dip as the schistose beds beneath it, which rest conformably on the strata belonging to the old red sandstone series of the Slieve Meesh range. How Mr. Weaver can consider beds in such a situation to belong to the transition series, I cannot understand; for, as to his idea of there being a protrusion [†] of graywacke from beneath the old red sandstone, it cannot be sustained, there being no reverse dip; on the contrary, the yellow sandstone and dark-gray slate rest conformably on the old red slate, and are succeeded by strata of limestone having the same strike and dip [‡].

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* See Journal of the Geological Society of Dublin, vol. ii., part 1.

In regard to fossils in the yellow sandstone and carboniferous slate of this locality, as I mentioned in my paper which has been quoted by Mr. Weaver, they contain numerous imperfect casts of *Producta*, *Spirifera*, *Terebratula*, *Crinoidea*, and *Retepora*; but though I lately sought, with much care, I did not discover any varieties of *Orthis* or *Favosites*, supposed by Mr. Weaver to occur there.

The foregoing description of the structure of the Cahirconree, or Slieve Meesh range, is similar to that contained in my paper just alluded to, which has been verified by recent observations; but as Mr. Weaver was not convinced of the inaccuracy of his views respecting the carboniferous slate at the eastern base of the Slieve Meesh range, which, notwithstanding my section and description, he still considered to belong to the graywacke series, I do not expect that what I now repeat will have the effect of changing his opinion. But it should be observed, that this carboniferous slate, which underlies and alternates with the undoubted carboniferous limestone of the valley of Castle island, is precisely similar to the carboniferous slate which underlies and is interstratified with the lower beds of the carboniferous limestone at Clonea Castle, on the east coast of Waterford, near Dungarvan; it is likewise similar to the rock which alternates with the limestone of Cork harbour, of Killarney, of Kenmare, and of many other localities in the south of Ireland; consequently, as Mr. Weaver persists in the opinion that the limestone of Cork harbour, of Killarney, of Kenmare, &c., belongs to the transition series, it would be fatal to his argument to class the carboniferous slate of Clonea Castle, or of Currens, with the carboniferous limestone series. But I will observe, that in the localities just mentioned, beds of undoubted carboniferous limestone alternate with slate, precisely similar in fossils, as well as in lithological character, to that of Cork harbour, &c.

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[*] Journal of the Geological Society of Dublin, vol. ii., part 1.

[†] See Lond. and Ed. Phil. Mag. for April, p. 291.

[‡] In my section already alluded to, the lithographer did not make an accurate copy of the original, and has made the limestone strata to rest unconformably upon the carboniferous slate, while in nature these strata are conformable.

I do not think it necessary to pursue this argument further than to observe, that in endeavouring to form a distinction between the admitted carboniferous limestone of the valley of the river Laune, and that of Killarney, and also between the admitted carboniferous limestone of the valley of the Blackwater, below Mallow, and that westward of Clonmeen Castle in the same valley, Mr. Weaver has involved himself in an untenable dilemma. There is no difference in geological position, in mineral character, or in fossils, between the limestone of Killarney, and that of the valley of the Laune between Beaufort bridge and Killorglin, which are all contained in the same valley, and all repose on the same base; and a similar statement may be made in regard to the limestone of the valley of the river Blackwater, above Clonmeen Castle, and that at and below Mallow; yet Mr. Weaver considers the limestone of Killarney and that above Clonmeen Castle to be transition, and that below Beaufort bridge and Mallow to be carboniferous.

After what has been said, I hardly think it necessary to reply to the observations contained in the postscript to Mr. Weaver's paper*, in which he endeavours to show that I am incorrect in considering the conglomerate and red sandstone of the Gap of Dunloe, and that to the south of the Lower Lake of Killarney generally, as identical with the red sandstone, the conglomerate, and red slate of the Cahirconree or Slieve Meesh range.

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* Published in the Philosophical Magazine for June last. [L. and E. Phil. Mag. vol. xvi. p. 471.]

It should be observed, that the red conglomerates and red slates of the district of Killarney are situated on the *south side* of the carboniferous limestone valley of Castlemaine, while those of Cahirconree are on the *north side*; that the upper beds of both graduate into the carboniferous limestone series, at Currens on the north side, and at Brickeen island in the Lower Lake of Killarney on the south side of the valley. The unconformability of the conglomerate beds with the transition series on Cahirconree, and their conformability on McGillacuddy's Reeks, the Purple Mountain, &c., is no proof that the rocks are not identical, as, in England, the old red sandstone graduates both into the Silurian and mountain limestone series.

In respect to the fault described by me, the occurrence of which is *doubted* by Mr. Weaver, I shall observe, that it is clearly visible at the Gap of Dunloe and at Brickeen island; and I will assert, that the positions of the old red sandstone strata on one side, and the chloritic rocks on the other, in both those places, are as clearly indicative of a fault as any I have ever seen. On the west side of the Gap of Dunloe, we have a perpendicular cliff upwards of 200 feet in height, which is traversed by a nearly upright cut or crack about 20 feet in breadth. On the south side of this cut we find strata of dark-green chloritic quartz-rock dipping to the south at an angle of 30°, while the strata on the north side dip to the west at an angle of 10°, and are composed of rather fine-grained conglomerate and a red quartzose-rock or compact sandstone identical with that which lies beneath the red conglomerate of Cahirconree. I am of opinion that these appearances do prove that there has been a fault.

Figure No. 4 in the plate is an accurate representation of the fault as above described.

Similar observations are applicable to the appearances at Brickeen island. There also, the strata on the south side of the fault consist of green chloritic rock, having rather a slaty structure, which dip to the south; while on the north side, we have in succession, red quartzose-sandstone and red slate, red limestone, and yellowish-green slate containing calamites, abutting obliquely against the chloritic rock on the north side.

Towards the conclusion of his paper, Mr. Weaver observes, "Proceeding now to the Dingle peninsula, the succession given also by Mr. Griffith from *north to south*, namely, from Brandon bay to Foylaturrive, is as follows: 1st, dark gray clayslate," &c. The above sentence is *a misquotation* from my paper. My words are, "If we make a section across the

Dingle peninsula, from Foylaturrive on the *south* to Brandon bay on the *north*, we find that the strata consist of a base of dark blackish-gray clayslate," &c. Now by reversing the points, Mr. Weaver has made the dark gray clayslate to occur on the north side of the peninsula, namely, at Brandon bay, while it really occurs on the south: the misquotation was doubtless unintentional, but as Mr. Weaver's argument was founded on this misconception, it is unnecessary to reply to it.

In concluding my observations, I cannot avoid expressing my regret that Mr. Weaver was not present either at the meeting of the British Association at Newcastle, or at the Geological Society of London, when I communicated my views relative to the geological structure of the south of Ireland; for, *viva voce* discussion tends more to clear up geological differences than lengthened written descriptions; and where both parties are in search of truth there is little difficulty in attaining it.

Dublin, July 8, 1840.

XXVI. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* By M. Dumas.

[Continued from vol. xvi. p. 505, and concluded.]

Organic Radicals.

For some years organic chemistry has so frequently used what we call *organic radicals*, that it will appear singular to see, if not their existence, at least the reality of the absolute function which they have been made to play, here put in doubt.

We know that by the term organic radicals we mean to designate certain compound bodies which might fulfil their functions in the manner of simple bodies, and which might enter, as they do, and following the same laws, into combination with the various bodies of nature.

If by organic radicals, bodies analogous to cyanogen, to amidogen, to the oxalic or benzoic radical be intended, there is no doubt that there, in fact, compound bodies perform the function of simple bodies, like those analogous to them in mineral chemistry, the oxide of carbon, sulphuric acid, the binoxide of azote, and nitrous vapour.

But if by the term organic radicals we must, as M. Berzelius wishes, designate certain invariable compounds which would fulfil the function of the metals, the theory of types, while admitting their concurrence, cannot allow their permanency.

Thus to fix our ideas, in the theory of types the essence of bitter almonds is a type in which we can substitute for an equivalent of hydrogen an equivalent of chlorine, of bromine, of iodine, of oxygen, or of amidogen, without the type being altered,

$$
\begin{gathered}
C^{28}\,H^{12}\,O^{2} \\
C^{28}\,H^{10}\,O^{2} \\
Ch^{2} \\
C^{28}\,H^{10}\,O^{2} \\
O \\
C^{28}\,H^{10}\,O^{9} \\
S \\
C^{28}\,H^{10}\,O^{2} \\
Az^{2}\,H^{4}.
\end{gathered}
$$

But whilst admitting that an element might be substituted for the system $C^{28}\,H^{10}\,O^{2}$, the theory of types does not consider it as an invariable group. It believes that hydrogen may be taken from that group, that chlorine may take its place, or that it may be made to undergo every other modification without its fundamental nature being altered by it.

In a word, by a reciprocity easy to foresee, and which to receive all its development would require a detail of formulæ which I cannot enter upon here, we arrive at the conclusion, that in the same manner that it is possible in an organic compound to substitute sulphuric acid, which fulfils the same function for hydrogen, so we may in certain organic matters substitute a simple body for a group of molecules representing a compound body.

To say that nitrous vapour takes the place of hydrogen in nitrobenzine, is the same as if we said that in æther potassium may take the place of æthyle.

But we must not conclude from this that æthyle is a permanent, immutable (*immuable*), unchangeable compound, for experience proves the contrary. Only by losing some hydrogen and gaining chlorine everything leads us to suppose that it preserves its character, as does the æther, of which it makes a part.

But I admit that in a given type there are certain compound groups for which simple bodies may be substituted, and which in so far would deserve the name of radicals. They fulfil the same function as ammonium, which takes the place of the potassium in alum, for example.

Thus I cannot consider these groups as immutable bodies, for experience has pronounced the contrary, and every theory which would absolutely rest on this basis would go too far.

Amongst the researches which contribute the most to modify the opinion on the function of the organic radicals, we should cite in the first place the important observations of M. Laurent on the essence of bitter almonds, and those not less remarkable of M. Piria on the hydruret of salicyle*. To resume; nothing hinders me from retaining the name of organic radicals for certain molecular groups capable of being substituted for elementary bodies which may reciprocally be substituted for them, but these groups may in their turn be modified by substitution, like the other bodies which do not perform this function.

I had a memoir of M. Gehrardt put into my hands, but too late for me to make use of it in the present notice, in which these questions are examined in a manner which appeared to me very worthy the attention of chemists.

Nomenclature.—Amongst the questions which are presented to us as being the immediate consequence of the point of view which we have just set forth, there is one which deserves particular attention; it has relation to the principle itself of our chemical nomenclature, and to the modifications which the progress of the science has led us to make it undergo.

At the memorable period when the French Academicians, under the influence of the immortal discoveries of Lavoisier, conceived and unfolded the project of a reform in the old chemical nomenclature, they grounded themselves upon the view which Lavoisier himself had just established, that is, upon the existence of those undecomposed substances which were recognized as the material elements of all bodies.

Seeing that by the aid of these elements all the bodies of nature could be produced, that in associating them two and two binary bodies were formed, that in combining these one with another salts were produced, and that in combining these salts in their turn double salts were obtained, the nomenclature had to follow the philosophical principle in all its developments. It required that the names of the elements should be set forth in those of the binary compounds, that they should reappear in the names of simple salts, in those of double salts, &c.

What strikes us in the chemistry of Lavoisier, and in the nomenclature which was the consequence and the expression of it, is the antagonism of the elements which combine to form the binary compounds; it is the antagonism of the acids and of the bases which combine to form salts; it is the antagonism of the salts which combine to form double salts, &c.

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* [See Lond. and Edinb. Phil. Mag., vol. xvi. p. 210, 211.]

The chemistry of Lavoisier and its nomenclature seemed then to have foreseen and prepared the electro-chemical theory, which has had nothing else to do than to call one of these antagonist bodies the positive element, and the other the negative element.

But let us not lose sight of the great discovery of Lavoisier; it is the discovery of the elements. This is the fundamental principle by which he revived chemistry and natural philosophy. Not a truth of this order is discovered without leaving its impress on all our thoughts; and for the same reason that Lavoisier had established that all the bodies of nature might be formed by means of some elements, he would be led to define the compound bodies by the elements which compose them, and there, in fact, is the principle that our nomenclature has appropriated.

Now not only is the nomenclature of Lavoisier no longer sufficient for us, but it expresses a system of ideas quite contrary to that which we seek to cause to prevail.

It is no longer sufficient for us, because in organic chemistry thousands of combinations are produced with three or four elements, and consequently those could not lend themselves to name all the compounds which result from them.

It is positively contrary to the system of ideas explained above, in this, that it derives the notion of the bodies from the nature of its elements, whilst the latter have only what may be called a secondary interest in the classification.

Each type must have a name, and this name should be found in the numerous modifications which it may undergo, so that it should never disappear so long as the type itself is not destroyed.

It is on this principle that I have already formed the following names: *acetic acid* and *chloracetic acid*, *æther* and *chloræther*, *olefiant gas* and *chlorolefiant gas*; names, the object of which is to set forth, as may be seen, the permanency of the types, notwithstanding the intervention of chlorine in the compounds.

The theory of types views these bodies in some degree as *casts from the same mould, with different materials.* It would have the nomenclature always recal their fundamental molecular arrangement, and that it should be put in the first line, whilst the nomenclature of Lavoisier applies itself to the material, brings out the nature of it, and places this notion first.

The theory of types tells you, here is alum of chromium;

the nomenclature of Lavoisier sees in it sulphate of potassa and of chromium under the form of alum.

Alum is a type; all the alums are cast in the same mould; their form is what the theory of types would set forth especially; that which essentially defines each of them. It acts as an artist, who in seeing the statues consisting of different materials cast from the same mould, will say to you, "Here is the Venus of Milo in brass, in lead, in plaster." The artistic type strikes him before he dreams of the material, and he will never think of saying that he is about to show you brass, plaster, or bronze in the form of the Venus of Milo.

An entire reform of the organic nomenclature and of some parts of the mineral nomenclature, appears to me, then, both urgent and possible.

Electro-chemical theory.—We just now saw how the principle of dualism, introduced by the chemistry of Lavoisier in the definition of every chemical combination, was favourable to the conception of what is called the electro-chemical theory. We have also understood how the theory of molecular types swerves from this order of ideas, for it does not suppose two antagonist elements present in the bodies, acting as would two masses endowed with different electricities, and held in combinations by the mutual action of these two electricities.

Does a chemical combination constitute a simple edifice or a double monument? this is the question. In the theory of types, the formulæ combine, and are written without attending to the reduction (*dédoubler*) of each body into two others. In the electro-chemical theory they combine, and are written in such a manner as always to paint to the mind these two principal divisions of the edifice which they represent.

This is the manner in which the theory of types has been driven to separate itself from the electro-chemical theory, or rather that in which this latter has been led to combat the other from its first appearance. The question, however, is given in the clearest way in the following letter from M. de la Rive. The skilful Genevese philosopher, whose name will always be united with the history of electro-chemistry, wrote to me on the 25th of October last (1839):—

"I have read your researches on *substitutions* with very great interest. They interested me the more as I have been occupied for more than a year upon a rather large work on the electro-chemical theories. I dare not, I must confess, go as far as you; and without believing in the theory of Berzelius such as he has presented it, yet I cannot help thinking that there is something well-founded in the table of the relative chemical powers of bodies. Now, that hydrogen can perform the function of chlorine exactly, is what I can hardly admit.

"Allow me to ask you if chemists are not rather easy (*facile*) when they group their symbols in every way. There is in this facility of permutation something which does not completely satisfy us physicists, and which appears to lend itself rather too complacently to all combinations. Is there not something arbitrary in the manner in which chemists make these choices? To attack the electro-chemical theory you group your formulæ in a certain manner; immediately to defend this theory M. Berzelius groups them in another manner; where is the law of nature?"*

I shall be pardoned for quoting this letter; it depicts the opinions of philosophers upon questions still new to many minds, and in all cases very obscure to those persons who have not followed them step by step in their development.

Those who have taken a part in the experimental researches of which we are speaking, know well that the electro-chemical theory guided my first studies, that I professed and admitted it for a long time on the faith of its inventors. They also know, that it is the force of circumstances, that it is a clear and convincing experience, the production of chloracetic acid, which has led me to admit that hydrogen and chlorine perform the same function in certain compounds. I constructed my formula according to pure chemical experience, my mind being free and disengaged from every view of general theory.

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* [It is proper to add here, that M. de la Rive, in the *Bibliothèque Universelle* for February 1840, p. 193, after reciting the passage extracted from his letter by M. Dumas, as above, makes the following remarks on the subject of it and the comments of M. Dumas:—

"In writing these lines to M. Dumas, I sought, as he himself remarked, to satisfy myself concerning a question which becomes every day more obscure. On the one hand, we cannot help recognizing that in organic chemistry, especially the electro-chemical theory, or rather the chemistry which connects the development of electricity with the play of the affinities with which it is always accompanied, has on its side powerful arguments, even when we do not admit on this point all the views of M. Berzelius. On the other hand, there are certainly some phænomena, especially in organic chemistry, in which the function of the same elements in the formation of compounds seems to change its nature in a manner so complete and so extraordinary, that we cannot admit of their possessing a previous predisposition to conduct themselves in such and such a chemical manner, or what comes to the same thing, an absolute electro-chemical power. Would not the result of this seem to be that the electro-negative or electro-positive properties do not previously exist in bodies? That they do not exist until the bodies are presented to each other, and that from that time instead of being *absolute* they are *relative*, that is to say, depend for the same body on the relations which exist between its own nature and that of other bodies in the presence of which it is found? This point of view can only be thoroughly examined by means of direct experiments. I shall return to it when I have finished bringing together a number of facts sufficiently considerable for its justification, if, as I presume, I find it to be founded. I shall be glad to try thus to reconcile, at least in part, the function which M. Berzelius attributes to electricity in the chemical phænomena with the very remarkable laws at which M. Dumas seems to have arrived."]

But to admit that chlorine may take the place of hydrogen and perform the same function, was to separate oneself from the chemists who would explain all the phænomena of combinations by means of what is called the electro-chemical theory. I understood it thus, and I found it necessary to explain myself in a direct manner. Besides, how could we believe that this consequence would have escaped the penetration of M. Berzelius, when we see all the value he attaches to giving an immediate explanation, according to the electro-chemical theory, of each of the facts which daily enrich the theory of substitutions, and when we are able to appreciate the high talent he displays in the combination of the formulæ which his theory requires?

It was not necessary to say to M. Berzelius, that in the views of electro-chemistry the *nature* of elementary particles should determine the fundamental properties of bodies, whilst in the theory of substitutions it is from the *situation* of these particles that the properties are especially derived.

We have, however, on this head decisive facts in the domain of mineral chemistry itself. Thus oxygen, sulphur, selenium, tellurium, chromium, iron, manganese, magnesium, and hydrogen constitute a series of bodies capable of taking each other's places, without the form or essential properties of the compounds being changed by it. Thus M. Berzelius attributes to the nature of the elements the function (*rôle*) which I attribute to their position: this is the ground of our respective opinions: let us now come to the point where they separate in practice.

Amongst the consequences of the electro-chemical theory, one of the most immediate consists in the necessity of viewing all chemical compounds as binary bodies. We must always find in each of them the positive particle and the negative particle, or the whole of the particles to which these two functions are attributed. Never was view more capable of shackling the progress of organic chemistry. All the difficulties which we have felt for some years in the inquiry concerning the fundamental formulæ of bodies, the discussions, the misconceptions, the errors, spring from prepossessions which this view had given rise to in our minds.

Some examples will make these two points of view easy of comprehension.

Carbon can combine with oxygen, and thus form carbonic oxide and carbonic acid. In its turn carbonic oxide combines with chlorine, and produces the acid gas discovered by Dr. John Davy. The electro-chemical theory should see in this last an acid chloride of oxide of carbon. The theory of types views it, on the contrary, as carbonic acid, in which for half the oxygen chlorine is substituted. Thus the bodies CO^2, $COCh$, CS^2 are modifications of the same type*.

Oxygenated water is a type, and one of the neatest and best-defined (*nets*) that chemistry possesses. Supply the place of the hydrogen by a metal, and you will have the binoxides of calcium, barium, strontium, and in general the simple (*singuliers*) oxides. For these, substitute in its turn for half the oxygen, chlorine, as is the case in chloro-carbonic acid, and you will produce the decolorating chlorides. Thus oxygenated water, the simple oxides and the decolorating chlorides, belong to the same type, to which must also be added the compounds which binoxide of azote forms with the alkaline oxides, so, for example, that we may have the following series:

$$\begin{array}{l}
\left.\begin{array}{r}\mathrm{H\ O}\\ \mathrm{O}\end{array}\right\}\\
\left.\begin{array}{r}\mathrm{Ba\ O}\\ \mathrm{O}\end{array}\right\}\\
\left.\begin{array}{r}\mathrm{Ca\ O}\\ \mathrm{O}\end{array}\right\}\\
\left.\begin{array}{l}\mathrm{Ca\ O}\\ \mathrm{Ch}\end{array}\right\}\\
\left.\begin{array}{l}\mathrm{Ca\ O}\\ \mathrm{Az\ O^2}\end{array}\right\}
\end{array}$$

These compounds of oxides and of chlorine have received all kinds of definitions in the electro-chemical system. Chlorides of oxides, of chlorites, of hypochlorites, have been made of them, as people were guided by the pretended necessity of always putting together in the formula of a compound two antagonist bodies, the positive and the negative.

This is precisely the character of the differences at the present time between the electro-chemical school and the school of molecular types.

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* Here is what I said of phosgene gas in 1828:—" It is easy to see that chloro-carbonic acid corresponds to carbonic acid itself. In fact, in all its combinations one volume of chlorine takes the place of one half-volume of oxygen; it is then as if the oxide of carbon had been changed into acid, by substituting for the half-volume of oxygen which it was necessary to add, a volume of chlorine."—See my *Traité de Chimie*, vol. i. p. 513.

If acetic acid is deprived of all its hydrogen, and chlorine substituted for that hydrogen, we say that acetic acid and chloracetic acid possess the same molecular arrangement, and that they should possess the same general actions so long as their molecule is not destroyed. Urged by the convenient principles, the electro-chemical system, M. Berzelius, on the contrary, makes of chloracetic acid a separate body, in which he arranges the elements into two groups, which he supposes to be combined with each other. In his opinion the chloracetic acid becomes a compound of oxalic acid and chloride of carbon, a formula which is in no way justified, for chloracetic acid treated with potassa should give chloride of potassium and oxalate of potassa, whilst, according to my experience, it really gives carbonic acid and chloroform.

It is just the same thing as when it was said that the binoxide of calcium and the chloride of lime belonged to the same type, that which the interesting and decisive experiments of M. Millon have so well proved; whilst M. Berzelius, relying on ingenious researches, was induced to assert that lime when uniting with chlorine gave rise to a chlorite.

If M. Malaguti takes two equivalents of chlorine from æther, the theory of types foresees and explains that in their place there must have entered into the new product two equivalents of chlorine. It sees æther in the new product, as to the molecular constitution and the fundamental properties.

But M. Berzelius, on the contrary, as might have been supposed, disposes the elements of this new body, and those of the products of which it makes a part, in such a way as to make them constitute binary compounds, which according to these formulæ would possess actions quite opposed to those which have been recognized by M. Malaguti.

In all cases in which the theory of substitutions and the theory of types see *single molecules* losing some of their elements and substituting others for them, without the edifice being modified in its form or its exterior actions, the electro-chemical theory reduces (*dédouble*) these same molecules, solely we must say to find those *two antagonist groups* which it afterwards supposes combined, in virtue of their reciprocal electrical action.

Thus in my opinion the electro-chemical theory has been drawn out of the circle which experience traces for us, when it would have explained the new facts of organic chemistry. But is it to be asserted that the electrical properties of bodies are without influence on chemical phænomena? Unquestionably not: only it must be agreed, that it is at the moment when the combinations are made, at the moment when they are destroyed, that the function (*rôle*) of electricity may be observed.

But when the elementary molecules have taken their equilibrium, we know not any longer how to define the influence that their electric properties may exercise, and no one has put forth views on this subject which agree with experience.

I have then been induced to declare that the facts which I have just discovered were irreconcilable with the electro-chemical theory of M. Berzelius, who considers hydrogen as always positive and chlorine always negative, whilst we see them supply each other's place, and perform the same function.

But I am far from denying, on that account, that the chemical and electrical forces may be the same, and there is no reason to take up the defence of the general function of electricity in chemical phænomena, when it is simply a particular electro-chemical theory which is under discussion. What I wished to say, what I said, is, that when we have endeavoured to represent the electric state of the combined molecules, pure hypotheses have been attained without any result for the science.

When, on the contrary, as has been done so happily by our colleague M. Becquerel, an endeavour has been made to take advantage of this electricity which shows itself at the moment of chemical combinations or decompositions, results the most important and the most fruitful have been obtained.

It is in this class of facts that the beautiful discoveries of Davy may be classed, those which M. Becquerel pursues with so much perseverance and success; in fact, the experimental law with which Mr. Faraday himself more recently enriched chemical philosophy.

All the discoveries of these great physicists have reference to the phænomena of the chemical action, and are quite independent of the views which they may have expressed on the function of electricity in compound bodies.

In the course of this memoir I have several times made use of the actions (*réactions*) of bodies, as being the only method quite proper for unfolding their real nature. There is notwithstanding an objection in the experiments themselves, to which I have often referred the reader, thus:—

A chemist who, without knowing the origin of it, had had to study the body $C^8 H^6 Ch^4 O$, seeing that under the influence of potassa this body is changed into chloride of potassium and acetic acid, would certainly have seen in it either a chloride of the acetic radical, or acetic acid, in which for a portion of the oxygen chlorine had been substituted; yet this body is nothing but the chlorinated æther of M. Malaguti.

Just so, when I was occupied with the study of chloroform $C^4 H^2 Ch H^6$, the manner in which it acts with the potassa, the formation of the chloride of potassium, and of the formic acid which result from it, led me to consider it as being formic acid, $C^4 H^2 O^3$, in which equivalent quantities of chlorine supplied the place of the oxygen; yet M. Regnault has lately shown that chloroform is nothing but some hydrochloric æther of methylene, in which chlorine has supplied the place of a portion of the hydrogen, the body $C^4 H^6 Ch^2$ being changed by this substitution into $C^4 H^2 Ch^6$.

The result of these examples, which might be multiplied, would be, that the actions of bodies are not a faithful guide, for they lead us to refer to acetic acid, a body derived from æther, and to formic acid, a body which represents hydrochloric æther of methylene. But in looking nearer, we see, in fact,

Æther	$C^8 H^{10} O$
Chlorinated æther	$C^8 H^6 O$ Ch^4
Acetic acid	$C^8 H^6 O$ O^2,

belong really to the same molecular grouping, and that in saying that chlorinated æther is derived from æther, and that it produces acetic acid, nothing really contradictory has been affirmed.

On the other hand,

Methylic æther	$C^4 H^6 O$
Formic acid	$C^4 H^2 O$ O^2
Chloro-methylic æther	$C^4 H^6 Ch^2$
Chloroform	$C^4 H^2 Ch^2$ Ch^4,

constitute bodies of the same molecular grouping, so that chloroform may be viewed as anhydrous formic acid, or as bichlorinated chloro-methylic æther, without these two ways of regarding it at all contradicting each other.

The result of this is, that chemical actions, without possessing the absolute character which has often been given to them, deserve a confidence which may have been momentarily shaken, but which a profound examination again establishes in its true place in our minds.

In fact, we have admitted that substitutions may unveil the molecular grouping of bodies by furnishing a set of equations of condition which the general formula ought to satisfy. Now it is evident that the metamorphosing actions are often nothing but means for operating substitutions, by taking advantage of affinities more complicated than those which are made use of in the ordinary substitutions.

It is therefore more than ever requisite to apply to the study of the actions of bodies, and not to trouble ourselves about the distance which often separates the point from which we started from that which we reach; for it may well happen, that these two points, so unlike in their properties, are really united to each other by the theory of substitutions, and belong to the same molecular grouping.

The law of substitutions expresses, then, a simple experimental relation; it is limited to the expression of a relation often observed between the hydrogen lost and the chlorine absorbed, by a hydrogenated body submitted to the action of chlorine. This law establishes only, that if the substance loses 1, 2, 3 equivalents of hydrogen, it will gain 1, 2, 3 equivalents of chlorine; but it does not explain this fact.

The theory of types goes further; it explains what the law of substitutions is content to determine. It considers organic bodies as being formed of particles, which may be displaced and have their places supplied by others without the body being destroyed, so to speak. In the cases above-quoted, the molecule of acetic acid, that of æther, may lose hydrogen and take chlorine, without ceasing to constitute an acid or basic molecule, formed of the same number of equivalents and endowed with the same number of fundamental properties.

It is then because, that on pain of being destroyed, the molecule of acetic acid must take an equivalent of chlorine to stand for the equivalent of hydrogen which it loses, that this substitution, this *remplacement* is effected. Thus it is that the theory of types explains the law of substitutions.

The substitution of one element for another, equivalent for equivalent, is the effect; the preservation of the type is the cause. The organic molecule, the organic type, constitute an edifice, in which a course (*assise*) of hydrogen can have its place supplied by a course of chlorine, of bromine, or of oxygen, without the exterior relations of that edifice being thereby modified. But it is necessary, when the course of hydrogen is taken away, to put something in its place; if not, the edifice crumbles or is transformed.

The law of substitutions was hardly put forth before it became the subject of severe criticisms in Germany, to which I thought it useless to reply. If this law was just, it was for experience to teach us; if it was false, it was experience which would pronounce its falsity. In all cases it was necessary to leave time to determine its place in science.

The theory of types was scarcely published when the same criticisms were reproduced, at least by M. Berzelius; and notwithstanding all my devotion to the interests of the science, I would again have left to time and experience the care of pronouncing on these debates.

But when I reflected, it seemed quite evident to me, that as a consequence of the researches of organic chemistry, general chemistry had reached one of those periods of crisis, when every one owes to science the testimony of his convictions.

We cannot conceal from ourselves that two systems of ideas are before us:—one, which is supported by all the authority of the past, the rights acquired by quiet possession now for nearly a century, the tacit assent of a great number of chemists, and which reckons amongst its defenders and at their head, a philosopher illustrious amongst the most illustrious, M. Berzelius; the other, which consists in asserting that the bodies formed of the same number of chemical equivalents placed in the same manner, belong to the same molecular type, and often to the same chemical type.

This latter attributes to the number and arrangement of the particles an influence of the first order, which in the ideas of the received chemistry belongs especially to the nature of those particles. The law of substitutions would be the experimental demonstration of this new system, and would have led some of its partizans to adopt it. I do not claim its invention, for it does but reproduce and give precision to, under a more general form, opinions which are to be found in the writings of great chemists, and particularly MM. Robiquet, Mitscherlich, Liebig, Laurent, Persoz, Couerbe, &c. It is precisely this coincidence between the numerous facts, to the discovery of which the law of substitutions has led, and the opinions already known relative to the influence of certain preexisting molecular arrangements, that has given me the confidence necessary for their adoption in my turn when I proposed the admission of organic types.

Here we have, then, before us two systems: one which attributes the principal agency to the nature of the elements, the other which reserves it for the number and arrangement of the equivalents.

Pushed to an extreme, each of them in my judgement would be found to lead to an absurdity. Regulated by experience and kept by it within prudent limits, each of them must take a large share in the explanation of chemical phænomena; and to explain by a last word the meaning which I attach to their respective functions, I shall say that in chemistry the nature of the molecules, their weight, their form and their situation, must each exercise a real influence on the properties of bodies.

It is the influence of the nature of molecules that Lavoisier has so well defined, it is that of their weight which Berzelius has characterized by his immortal labours. It might be said that the discoveries of Mitscherlich relate to the influence of their form, and the future will prove whether the present labours of the French chemists are destined to give us the key to the function which belongs to their position.

We subjoin to the preceding memoir by M. Dumas, a translation of an extract of a letter from M. Baudrimont, published in the *Comptes Rendus*, for March 16.

" M. Dumas says, that the law of substitutions, and the theory of chemical types, are unconcerned in the reclamations of M. Baudrimont, who does not admit them. This requires an explanation from me.

" I cannot admit M. Dumas's law of substitutions; first, because it has not the character of a physical law; secondly, because it is but the strict expression of an order of facts, much more extended than M. Dumas supposes; *but I admit chemical substitutions*; *for substitution is only one of the modes by which bodies may enter into combination.*

" I should without doubt do more than M. Dumas in saying to the Academy: *Chemical compounds are produced, either by direct combination or by displacement, or by substitution, or lastly, by several of these modes united. Substitution may be non-equivalent, equivalent, isotypic, isorhythmic, or isomorphic*; let me be pardoned this neologism. But this formula, which is true, has not the character of a law; it is but the general expression of facts which are within the knowledge of all men, ever so little versed in chemistry; for chemical substitutions have been known ever since we arrived at the knowledge that one metal can precipitate another, taking its place in a saline solution; ever since hydrogen was first obtained by displacing it by iron or zinc in the pretended sulphuric and chlorhydric acids diluted with water; ever since we knew that chlorine displaces bromine and iodine; ever since we knew isomorphism by substitution; and ever since M. Beudant made more than a thousand applications of them to the calculation of the composition of minerals.... I admit, then, chemical substitution; but I repudiate the pretended law of M. Dumas, for the reasons I have just set forth.

"As to what relates to chemical types, which M. Dumas says I do not admit, I dare hope that the Academy, and all enlightened men will not participate in this opinion; for it will without doubt be allowed that he who first classed chemical types, must necessarily have admitted them, even before M. Dumas had acquired any notion of them, as his memoir appears to show, since he makes this notion only go back to his experiments on chloracetic acid, the discovery of which is posterior to my thesis."

M. Dumas replies, that his memoir is conceived in such terms, that it should have spared the Academy all the reclamations of which it has been the subject. In a historical note which he intends soon communicating to the Academy, he will show in what the views which are represented as identical differ, and to whom belongs the discovery of each of the principal points of the theory.

XXVII. *On the Ferrosesquicyanuret of Potassium.* By ALFRED SMEE, *Esq., Surgeon.*[*]

THE action of chlorine upon the ferrocyanate of potassium is a subject of much interest to the chemist, and has not been examined to any extent in this country. It therefore has been my endeavour to investigate this action carefully, and to see under what circumstances the change from the ferrocyanate into the ferrosesquicyanuret takes place; and the methods which are here detailed to obtain this latter salt uncontaminated with impurities, will be found free from the difficulties and uncertainties attending on the present mode of preparing it.

When a current of chlorine is passed through a solution of ferrocyanate of potassa, or an aqueous solution of that gas is added to it in certain quantities, the persalts of iron are not precipitated. This solution has no smell of chlorine, and is changed from a yellow colour to a dark red, and deposits on evaporation red crystals. A similar change takes place when bromine is added to the ferrocyanate, and in both cases the weight of the entire red mass is equal to that of the yellow ferrocyanate, plus the weight of the chlorine or bromine used, but minus the quantity of water which the yellow crystals are known to contain. This indicates, first, that the red crystals are anhydrous; and secondly, that the chlorine or bromine is actually absorbed by the salt. The former fact is confirmed by heating the red precipitate in a test tube, when no water is given off; and the latter fact is also proved by the evolution of chlorine or bromine, on the addition of two or three drops of strong heated sulphuric acid to a few grains of red salt.

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[*] Read before the Royal Society, June 18, 1840; and now communicated by the Author.

When heated alcohol is added to this red mass a small portion is dissolved, which is again deposited when the spirit is evaporated. This salt by its characters is known to be either the bromide or the chloride of potassium. By this method the red ferrocyanate of potassa, which is insoluble in alcohol, becomes purified; but this is a troublesome and expensive process, as the bromide or chloride is but little soluble in the spirit, and therefore a large quantity must be used.

About half an equivalent of chlorine or bromine is required to effect this change, and great care must be employed to prevent excess of these substances, as they are apt to react upon a portion of the salt. The liquid in this case contains Prussian blue dissolved, which materially discolours the salts, and it can only be precipitated from the solution by the addition of neutral salts, as sulphate of soda, which renders the red ferrocyanate impure. In a similar manner, chloride of soda, as might be expected, forms the red ferrocyanate of potassa.

From the foregoing details a knowledge is obtained of the action of chlorine and bromine upon the ferrocyanate, for we have seen that chloride and bromide of potassium is formed, and that one half an equivalent of these substances is necessary for this change. Now it is manifest that half an equivalent of potassium is removed from the ferrocyanate, so that the new salt, instead of consisting of iron one equivalent, potassium two equivalents, cyanogen three equivalents, contains iron one equivalent, potassium one and a half equivalent, cyanogen three equivalents; and therefore it is rightly named the ferrosesquicyanuret of potassium: that half an equivalent of potassium has been removed from the salt, two or three experiments have verified.

The acids as a class will not effect a similar change, because as they combine not with potassium but with potassa, water must be decomposed, the oxygen uniting with the metal, and the hydrogen passing to the ferrocyanate, forming hydroferrocyanic acid.

A question naturally arises whether the potassium may not be removed from the ferrocyanuret by other processes, and we are led to try the action of the anions, and of these I attempted to add oxygen to the salts by the use of nitric acid. This acid, when added in small quantities to the yellow ferrocyanate, acts as the other acids by liberating hydroferrocyanic acid, which is speedily decomposed into a pale blueish cyanuret of iron. When, however, further additions of this acid are made, the potassium takes oxygen, forms potassa, deutoxide of nitrogen is evolved, and the solution becomes dark coloured. This liquor, when neutralized with potassa, is found to give no precipitate with the persalts of iron, but forms Prussian blue with the protosalts of that metal. The rapidity of this change depends upon the heat of the solution, for when warm the effect takes place immediately, whilst on the contrary, two or three days are required at a low temperature. When evaporated, a large quantity of nitrate of potassa is deposited; and lastly some red crystals are formed. When acid is more used, the ferrocyanate is totally decomposed; the black mass which is the result has at first a sweet, but afterwards leaves a disagreeable metallic taste upon the palate. This process can never be used advantageously to form the ferrosesquicyanuret, from the quantity of acid which is required, the degree of nicety which must be employed to effect the change, and the impurity of the salt when obtained.

The next highly oxygenated acid which we have to examine is the iodic; this when added to ferrocyanate of potash becomes decomposed, the oxygen passes to the potassium to form potassa, free iodine is evolved, and the potassa passes to another portion of iodic acid, and is precipitated as the iodate of potassa. The free iodine can be readily removed by agitation with a little æther, and in this way a tolerably pure ferrosesquicyanuret of potassium can be extemporaneously obtained, for the solution contains but little iodate of potassa from its insolubility*.

Chloric acid operates in the same way as iodic acid, but is more difficult of decomposition, and it requires the action of heat before the smell of chlorine is exhaled and the red ferrocyanate formed.

If chlorate of potassa be added to the ferrocyanate, and dilute sulphuric acid be dropped into the solution, red ferrocyanate of potash will also be formed.

Bromic acid will not act upon the ferrocyanate with the production of the ferrosesquicyanuret, but acts as other acids in forming Prussian blue.

A great variety of other oxyacids have been tried, but none were found to part with their oxygen.

When a large quantity of peroxide of manganese in fine powder is added to a solution of the ferrocyanate of potash, and the mixture digested for a considerable time, the ferrocyanate becomes converted into the ferrosesquicyanuret, and on evaporation crystals of the most beautiful ruby red are obtained. The salt thus procured appears to be very pure.

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* This elegant process can be employed with advantage when a small quantity of the salt is suddenly wanted, as it scarcely requires a minute to effect.

If a little dilute sulphuric acid be added to the solution in conjunction with the peroxide of manganese, the action takes place more quickly, but sulphate of potassa is formed, which is a great disadvantage.

The last process in which nascent oxygen contributes to the formation of ferrosesquicyanuret of potassium, is, perhaps, one of the most elegant, efficient, and simple processes in the whole range of chemistry. This mode I was induced to follow from the consideration, that as nascent oxygen effects a change of the yellow to the red ferrocyanate of potassa, a similar change must be produced by its being subjected to a galvanic current. Accordingly some solution of the salt was placed in a tube bent like a syphon, and at the bottom a piece of tow was thrust, in order that a separation might so far be effected, that the solution on one side could not readily pass to the solution on the other. Having thus completed the arrangement, a galvanic circuit was passed through the fluid; when at the cathode, hydrogen was evolved, and at the anode no oxygen, on the contrary, was given off, but the solution became of a dark colour. The dark solution was found to precipitate only the protosalts of iron, and on evaporation deposited red crystals of the ferrosesquicyanuret, but at the cathode potash was discovered. The rationale of this change may be deduced from circumstances attending slight alterations of arrangement; for if on the zinc side of the bent tube a saturated solution of the ferrocyanate be placed, and on the platinum side distilled water, and then the galvanic circuit be completed, potash will appear at the platinode, and red ferrocyanate at the zincode. On the contrary, if the distilled water is placed at the zinc side and the ferrocyanate at the platinum side, potash is left at the platinode, whilst at the zincode no red ferrocyanate is found, but a substance which does not redden litmus paper, and which speedily decomposes into Prussian blue; this is probably ferrocyanogen. Thus it appears that one equivalent of the yellow ferrocyanate is decomposed, the free potash travels one way and the hydroferrocyanic acid the other; the oxygen unites with the hydrogen of the acid and sets ferrocyanogen at liberty; this again unites with an equivalent of ferrocyanuret of potassium to form the ferrosesquicyanuret.

Various other attempts were made to form the red ferrocyanate by oxygen, such as heating it with nitrate of potassa, but the mixture exploded at a temperature below redness.

When a mixture of powdered ferrocyanate and peroxide of manganese were heated together no ferrosesquicyanuret was formed. Several other oxides, as those of mercury, silver, tin, iron, &c. &c., were digested with ferrocyanate of potassa, but none that were tried, except the peroxide of manganese, formed the red ferrocyanate; many of them were converted into cyanurets.

A current of oxygen gas passed through the solution of the salt produces no alteration, showing that the gas must be in a nascent state to cause the change.

The next substance we have to examine is phosphorus, and its action is somewhat remarkable; for little or no change is effected by the addition of an alcoholic or ætherial solution of phosphorus. When a piece of phosphorus is also placed in a solution of the ferrocyanate, or when phosphorus is heated with powdered ferrocyanate, the sesquicyanuret is not produced; but if a stick of phosphorus is placed in a bottle containing a solution of the salt, and only a portion of it is covered with the liquor, the phosphorus gradually burns away, the solution becomes sour and red, and ceases to precipitate the persalts of iron. This change takes place with a rapidity exactly proportionate to the wasting of the phosphorus; for if the temperature is below 45°, but little action takes place, but above 60° the reddening is very speedily produced. The red solution is not to be tested with the salt of iron whilst it is acid, for in that case a copious greenish-white precipitate is produced of phosphate of iron; but after it has been neutralized with potassa a solution of baryta is to be added, to throw down the phosphate, and a drop of dilute sulphuric acid may then be added to remove any excess of baryta.

The solution will now be found not to precipitate persalts of iron, but, on the contrary, a large quantity of Prussian blue is produced with the protosalts. The actual combustion of the phosphorus seems essential to this change; for if the water in which phosphorus has been allowed to burn, be added to the solution of the ferrocyanate, a similar change will not be produced. The cause of this change appears paradoxical, for phosphorus has in other instances a deoxidizing agency, so that a piece placed in a solution of either gold, silver, platinum, or copper, has the metal precipitated upon it. Perhaps it depends upon decomposition of water and the formation of phosphuretted hydrogen; for a narrow bottle, to which air has but limited access, is more favourable to the change than a wide vessel. If this explanation is correct, the action of phosphorus must be classed with the other oxygenating substances, for oxygen, and not phosphorus, removes the potassium*.

No mode of abstracting the half equivalent of potassium by sulphur is known, for if half an equivalent of sulphur be heated with powdered ferrocyanuret, the ferrosesquicyanuret is not produced, and the alcoholic or terebinthine solution of sulphur, added to a solution of the ferrocyanuret, also failed to produce this change. Even nascent sulphur arising from the decomposition of sulphuret of potash by an acid did not produce any effect†.

A current of cyanogen gas passed through a solution of the salt is gradually absorbed, and it becomes of a very dark colour, but red ferrocyanate is not formed.

Doubtless many may be surprised that the action of iodine has not been adverted to before, and more especially that it should not have been mentioned with chlorine and bromine, as to these it has a striking analogy in most of its properties; but in reality little resemblance exists between the action of iodine on the ferrocyanate of potassa, and that of chlorine and bromine, as we shall immediately see. If iodine is added to a solution of the salt it speedily becomes dissolved, the solution turning to a dark red, and gives a blue precipitate with salts of either oxide of iron. One equivalent of ferrocyanate of potash dissolves about one equivalent of iodine, which remains in great part uncombined in solution. If the solution is allowed spontaneously to evaporate the free iodine passes off, and a whitish uncrystallized mass is obtained which has no free iodine, but hydriodate of potassa in its composition. This gives a precipitate with both oxides of iron. Now there is a ready method of ascertaining how much iodine the ferrocyanate will not only dissolve, but combine with, and for this purpose a definite quantity of the salt is to be dissolved in a small quantity of water, and then placed in a phial. Upon the solution æther is to be poured, then the iodine is to be added gradually, when as soon as the æther is discoloured the saturation is known to be effected. Brisk and continued agitation must follow each addition of the iodine, in order that the æther may part with any iodine previously to the point of saturation. When evaporated to dryness more of the iodine is evolved, but still hydriodate of potash may be abstracted from the mass by alcohol. When all the iodine is removed from the mass, a result which is known by its not discolouring starch upon the addition of nitric acid, it still retains its power of forming Prussian blue with salts of either oxide of iron, and still presents the same indisposition to crystallize, for it neither shows itself as the yellow nor the red ferrocyanate of potash, but as a compound having properties intermediate with both.

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* No change takes place if the phosphorus is completely under the solution of the salt.

† It is foreign to this page to describe the sulphocyanuret of potassium.

When iodide of potassium is added to the ferrosesquicyanuret, iodine is evolved, the solution loses its red colour, and the salt possesses the characters similar to the mass obtained by the action of iodine on the ferrocyanate of potash. Thus it is evident that if a solution of persulphate of iron be treated with the red ferrocyanate whilst an iodide is present, Prussian blue will be formed.

Whether this is really a mixture of the ferrocyanuret and ferrosesquicyanuret or a distinct compound, it is difficult to determine, but the latter is rendered probable from its generally presenting itself as an amorphous mass; yet, however, when the purified mixture is dissolved two or three times in water, a dark mass is deposited, and at last crystals of the yellow salt are formed.

Every method which has been discovered of converting the ferrocyanate of potassa into the ferrosesquicyanuret has now been detailed, and we have seen that they may each be referred to the class of anions, for of the cathions the powerful agency of potassium was unable to effect this change.

Upon the first formation of the ferrosesquicyanuret the colour will occasionally be a very dark red, but this is an adventitious, not a necessary property; for when prepared by peroxide of manganese or chloride of soda, it does not possess this dark colour. If the red crystals be carefully picked and re-dissolved, in no instance is this seen, and in every case where the dark red exists it yields to liquor ammoniæ or potassæ, with the production of a small quantity of the ferrocyanate.

The ferrosesquicyanuret, however prepared, has the same peculiar properties. It has been already mentioned that the protosalts are precipitated blue, whilst the persalts are not effected by this agent; however, the solution in the latter case is always much darkened, and after a time a small quantity of dark-coloured substance is deposited. The mode of preparation of the ferrosesquicyanuret does not influence this result.

With almost every acid, especially if heat be applied, Prussian blue is formed and hydrocyanic acid is given off, and thus upon testing for minute quantities of metal, care must be taken to prevent any excess of acid, as in that case the chemist would find iron in everything he examines. With excess of alkali, on the contrary, no precipitate of Prussian blue is produced; and therefore if search be made for that most useful of all metals, the experiment would declare that iron had no real existence; but if the golden mean be employed, or the solution be but very slightly acid, the ferrosesquicyanuret, as well as the ferrocyanuret, become most valuable and delicate tests, the one for the peroxide, the other for the protoxide of that metal.

The change by chlorine and bromine has been shown to result from the abstraction of the half equivalent of potassium by the formation of chloride or bromide of that metal, and therefore the ferrosesquicyanuret is impure till that is removed by alcohol. We have seen also that the change may be effected by the iodic, nitric, and chloric acids, but by these methods the salt is also contaminated to a great extent by the nitrate of potash, but to a much less extent with the chlorate, and scarcely at all with the iodate; with phosphorus the salt in a very impure state may still be made. With peroxide of manganese, however, and the galvanic current, it may be made of absolute purity.

This last mode will probably supersede entirely every other mode of preparation, as with a galvanic battery a large quantity can be readily made. The battery which I have used for these experiments is the platinized silver, which from its simplicity is so well adapted for general purposes, and suitable for long-continued action.

Bank of England,
Feb. 12, 1840.

Table of Decompositions.

By Chlorine and Bromine.

$$\left.\begin{array}{l}\left.\begin{array}{r}\text{1 eq. Ferrocyanate}\\ \text{of Potassa}\end{array}\right\}\left\{\begin{array}{l}\text{Iron 1}\\ \text{Cyanogen 3}\\ \text{Potassa 2}\end{array}\right.\\ \tfrac{1}{2}\text{ equivalent of chlorine}\end{array}\right\} = \begin{array}{l}\begin{array}{r}\text{1 eq. Red fer-}\\ \text{rocyanate}\end{array}\left\{\begin{array}{l}\text{Iron 1.}\\ \text{Cyanogen 3.}\\ \text{Potassium 1.}\end{array}\right.\\ \tfrac{1}{2}\text{ equivalent chloride of potassium.}\end{array}$$

Bromine acts in the same way.

By the Galvanic Current.

$$\begin{array}{l}\left.\begin{array}{c}\text{1 eq. Ferrocyanate}\\ \text{of Potassa}\end{array} = \right\}\left\{\begin{array}{l}\text{Iron 1}\\ \text{Cyanogen 3}\\ \text{Potassium 2}\end{array}\right.\\ \tfrac{1}{2}\text{ eq. of Oxygen.}\\ \tfrac{1}{2}\text{ eq. of Hydrogen from decomposition}\\ \qquad\text{of water.}\end{array} = \left\{\begin{array}{l}\begin{array}{l}\text{1 eq. Red}\\ \text{Ferrocyanate}\end{array}\left\{\begin{array}{l}\text{Iron 1.}\\ \text{Cyanogen 3.}\\ \text{Potassium }1\tfrac{1}{2}.\end{array}\right.\\ \tfrac{1}{2}\text{ eq. of Potassa.}\\ \tfrac{1}{2}\text{ eq. of Hydrogen evolved.}\end{array}\right.$$

The action of the acids, &c. have been already sufficiently adverted to.

Table of Precipitates with the Iodo-ferrocyanate of Potassa pure.

Gold	Solution red, no precipitate.
Platinum	a little white deposit.
Mercury, bichloride	white, becoming green.
Lead	white, abundant.
Silver	white, with a little reddish tinge.
Bismuth	white, afterwards yellow.
Zinc	white.
Copper	dark brown.
Iron protosalts . . .	Prussian blue.
Iron persalts	Prussian blue.

Table of Precipitates with the Red Ferrocyanate of Potassa.

Gold . . .	chloride . . .	solution darker, no precipitate.
Platina. . .	chloride . . .	solution darker, small crystals deposited.
Palladium .	nitrate	red-brown precipitate.
Silver. . . .	nitrate	deep orange.
	sulphate . . .	
	acetate	
Nickel . . .	nitrate	red brown.
Copper . .	sulphate . . .	yellow brown.
	ammoniuret .	deep-greenish brown.
Mercury .	protonitrate .	at first yellow brown, then white, then green.
	bichloride . . .	none.
Bismuth . .	nitrate	pale yellow brown.
Tin	protochloride	white, gelatinous.
Iron	protosulphate	Prussian blue.
	persulphate .	none, with iodide, potassium, Prussian blue.
Antimony .	potassio-tartrate	none.
Manganese	chloride. . . .	sepia.
Cobalt . . .	chloride	chocolate brown.
Zinc	sulphate . . .	buff.
Cadmium .	sulphate.	pale yellow.
Curanium .	nitrate	deep red brown.
Lead . . .	acetate	solution brownish, none.
Alumina . .	acetate.	none.
Baryta . .	muriate	none.
	nitrate	
Strontia . .	nitrate.	none.
Lime . . .	muriate	none.

XXVIII. *Mineralogical Notices. Communicated by* W. H. Miller, *Esq., Professor of Mineralogy in the University of Cambridge.*

[Continued from p. 105.]

ANALYSIS OF MONAZITE.

[From Poggendorff's *Annalen*, vol. xlvii. p. 385.]

ONE hundred parts of Monazite, the Mengite of Mr. Brooke, analysed by M. Carl Kersten, gave

Oxide of cerium	26·00
Oxide of lantanium	23·40
Thorina	17·95
Oxide of tin	2·10
Protoxide of manganese	1·86
Lime	1·68
Phosphoric acid	28·50
Traces of potash and titanic acid.	

ANALYSES OF OCTAHEDRAL COPPER PYRITES. BY M. PLATTNER.

[From Poggendorff's *Annalen*, vol. xlvii. p. 351.]

	From Condurrow Mine near Camborne, in Cornwall.	From the Woitzki Mine near the White Sea.	From the Mårtan mountain in Dalarne in Sweden.
Sulphur	28·238	25·058	25·804
Copper	56·763	63·029	56·101
Iron	14·843	11·565	17·362
Silica			0·120

	From Eisleben.	From Sangehausen.	Unknown locality. (Analysed by M. F. Varrentrapp.)
Sulphur	22·648	22·584	26·981
Copper	69·726	71·002	58·199
Iron	7·539	6·406	14·845

XXIX. *On the Use of Hydriodic Salts as Photographic Agents. By Mr.* Robert Hunt.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I HAVE been engaged more than twelve months in studying the peculiarities of the salts of hydriodic acid, when used for the production of those photographic pictures which are formed by one operation, having their lights and shadows correct as in nature; and the results of my very numerous experiments are, the establishment of fixed principles, which remove most of the uncertainty attending the use of the hydriodates, an explanation of many of the anomalous results they give, and the discovery of some very remarkable properties not before noticed.

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* Communicated by the Author, whose former papers on Photography will be found in vol. xvi. p. 138, 267: see also our report of the proceedings of the Royal Society for the past session.

The uncertainty attending the application of the hydriodates, has greatly circumscribed their use, and it is the desire of forwarding the progress of a beautiful art, which now makes me solicit a few pages of your valuable Journal.

Sir John Herschel, in his very excellent memoir "On the Chemical Action of the Rays of the Solar Spectrum," &c.* particularly notices the inconstancy of the effects exhibited by the hydriodates. "Nothing," says that talented and indefatigable inquirer, "can be more variable and capricious than the results obtained according to the different intensities of the solutions applied; the qualities of the paper; the degree of darkening induced on the paper before the application of the ioduretted solution, the state of the paper as to moisture or dryness, and other circumstances."

That the various positions I wish to establish may be completely understood, and to ensure the same results in other hands, it will be necessary to enter into a somewhat detailed account of several kinds of paper which have been used, and to give tolerably full directions for successfully using the same, either in the camera obscura, or for drawings by application.

1. *The preparation of the paper.*—The variable texture of even the finest kinds of paper occasioning irregularities of imbibition, is a constant source of annoyance, deforming the drawings with dark patches, which are very difficult to remove; consequently my first endeavours were directed to the formation of a surface on which the photographic preparations might be spread with perfect uniformity.

2. A variety of sizes were tried with very variable results. Nearly all the animal glutens appear to possess a colorific property, which may render them available in many modifications of the processes published by Mr. Talbot, but they all seem to protect the darkened silver from the action of the hydriodic solutions. The gums are acted on by the nitrate of silver and browned, independent of light, which browning considerably mars the effect of the finished picture. It is a singular fact that the tragacanth and acacia gums render the drawings much less permanent. I therefore found it necessary for general practice to abandon the use of all sizes, except such as enter into the composition of the paper in the manufacture.

[* An abstract of Sir J. Herschel's paper appeared in vol. xvi. p. 331.]

3. It occurred to me that it might be possible to saturate the paper with a metallic solution, which should be of itself entirely uninfluenced by light, on which the silver coating might be spread without suffering any material chemical change. The results being curious, and illustrative of some peculiarities to be explained when the hydriodates come under our examination (65.), I shall record a few of them.

4. *Sulphate and Muriate of Iron.*—These salts, when used in certain proportions, overcame many of the first difficulties, but all the drawings on papers thus prepared faded out in the dark.

5. *Acetate and Nitrate of Lead.*—The salts of lead I have since perceived have been used by Sir John Herschel with success in some of his *negative* processes. I found a tolerably good result when I used a *saturated* solution of the above-named salts; but papers thus prepared required a stronger light than other kinds to give good results; when I used weaker solutions the drawing was covered with black patches. On these a little further explanation is required. When the strong solution has been used, the hydriodic salt which has not been expended in forming the iodide of silver, which, it is well known, is the lights of the photograph, goes to form an iodide of lead. This iodide is soluble in boiling water, and is thus easily removed from the paper. When the weaker solution of lead has been used, instead of the formation of an iodide, the hydriodate exerts one of its peculiar functions in producing an oxide of the metal (65—67.).

6. *Muriate and Nitrate of Copper.*—These salts, in any quantities, rendered the action of the hydriodates very quick, and when used in small portions appeared to promise much assistance in quickening the process; but experience has shown their inapplicability, the edges of the parts in shadow being destroyed by chemical action.

7. *Chloride of Gold.*—I did not anticipate much from the use of this salt. On trial it was found to remain inactive until the picture was formed, when a very rapid oxidation of the gold took place, and a consequent darkening of all the bright parts (5.) (65—67.).

8. Chloride of platina was found to act in all respects similarly to the chloride of gold, the re-darkening of the lights being much more rapid and intense (5. 7. 67.).

9. A very extensive variety of preparations were tried with like effects, and I was at length convinced, that the only plan by which a perfectly equal surface could be obtained, without impairing the sensitiveness of the paper, was careful manipulation with the muriated and silver solutions.

By attention to the following directions, simple in their character, but arrived at by a long series of inquiries, any one may prepare photographic papers, on which the hydriodic solutions shall act with perfect uniformity.

10. Soak the paper for a few minutes in a muriated wash, removing with a soft brush any air-bubbles which may form on it. The superfluous moisture must be wiped off with very clean cotton cloths, and the papers dried at common temperatures. When dry, the paper must be pinned out on a board, and the silver solution spread over it boldly but lightly, with a very soft sponge-brush. It is to be instantly exposed to sunshine, and, if practicable, carried into the open air; as the more speedily evaporation proceeds, the less does the silver penetrate the paper, and the more delicate it is. The first surface is very irregular, being made up of blue streaks, which are parts on which a true chloride is formed; and of brown ones, which appear to be the chloride of silver combined with a portion of undecomposed nitrate. As soon as the surface appears dry the silver solution must be again applied as before, and the exposure repeated. It must now be exposed until a fine chocolate-brown colour is produced equally on all parts of the surface, and then, until required for use, carefully preserved from the further influence of light.

11. In darkening these papers, the greatest possible attention must be paid to the quantity of light to which they are submitted, everything depending on the rapidity of the blackening process. The morning sun should be chosen, it being very evident that some portion of the violet rays are absorbed by the atmosphere after the sun has passed the meridian, which permeated it freely before he had arrived at that point.

A perfectly cloudless sky is of great advantage. The injurious consequence of a cloud obscuring the sun during the last darkening process, is the formation of a surface which has the appearance of being washed with a dirty brush. This is with difficulty removed by the hydriodates, and the resulting pictures want that clearness which constitutes their beauty. Papers darkened by the diffused light of a cloudy day are scarcely, if at all acted on by these salts.

12. The kind of paper on which the silver is spread, is an object of much importance. A paper known to the trade as satin-post, double-glazed, bearing the mark of J. Whatman, Turkey Mill, is decidedly superior to every other kind I have tried.

The demy printing papers are many of them bleached by chlorine, after an artificial substance has been given them by lime. These reverse the photographic process, and the parts on which the light acts with the most power become the darkest of the drawing, while the shaded parts are whitened.

The dark specks which abound in some kinds of paper must be avoided, and the spots made by flies very carefully guarded against. These are of small consequence, indeed are not noticed during the darkening action; but when the hydriodic wash is applied they form centres of chemical action, and the bleaching process goes on around them independent of light, deforming the drawing with small rings, which are continually extending their diameters.

13. *The Muriated Solutions.*—These saline washes may be considerably varied, and combined to an indefinite extent with a continued change of effect, which is singularly interesting. In their application I am invariably guided by the combining proportion of the salt; for having tried solutions of all strengths, I am at length satisfied no other proportions give such certain results; consequently I always work with my scale of equivalents at hand. The following is a list of the salts I most frequently use, selected from upwards of seven hundred combinations which I have tried. They are placed in the order of sensitiveness they appear to maintain, when used under as nearly as possible the same circumstances.

	Colour of Picture.
a. Muriate of ammonia	Red, changing to black in sunshine.
b. Chloride of sodium	Ditto.
c. Muriate of strontia	Brown, changes but slightly.
d. Muriate of baryta	A rich brown inclining to purple, darkens slightly.
e. Sol. chloruret of lime	Very red.
f. Sol. chloruret of soda	Red, changes a little.
g. Iodide of potassium	Yellow brown.
h. Chlorate of potassa	Variable, sometimes yellowish, often of a steel blue.
i. Phosphate of soda	Mouse colour.
k. Urate of soda	Yellow brown.
l. Muriate of iron	Deep brown, blackens.
m. Bromide of sodium	Red brown.

The change I mention in the colour of the finished picture is that which arises from a fresh exposure to the solar rays; where no change is mentioned, it is too slight to be worth notice. This phænomenon will, however, occupy our attention presently (38.). In addition to the salts named I sometimes use

	Colour of Picture.
n. Hydrochloric acid	Red which blackens.
o. Hydrochloric æther ...	Black.
p. Aqueous chlorine	Red, deepens a little.
q. Phosphoric acid	Very variable.

14. When papers prepared with any of the above, except *i* and *q*, are soaked for a little time in water, and dried in the sunshine, the picture produced—it matters not what hydriodate is used—is rendered peculiarly red, and does not change by re-exposure: washing either of the papers *b*, *c*, or *d* with weak solution of ammonia, occasions this peculiarity in a striking manner.

15. *The Solution of Silver.*—Take of crystallized nitrate of silver 120 grains, distilled water 12 fluid drachms; when the salt is dissolved, add of alcohol 4 fluid drachms, which renders the solution opake. After a few hours a minute quantity of a dark powder—oxide of silver?—is deposited, and must be separated by the filter.

16. The addition of the alcohol to the solution was adopted from an observation I made of its influence in retarding the chemical action of the hydriodates on the salt of silver, which goes on in the shade. Its use is therefore to make the action depend more on luminous influence than would be the case without it.

17. Nitric æther and acetic æther not only check the bleaching process in the shade, but actually act with the hydriodic salts in exalting the oxidation of the silver. In copying lace or feathers, they are very valuable agents, but for any other purposes they are useless, as all the faintly lighted parts are of the same tint.

18. The hydrochloric æther, which I use as the solvent of the silver, and apply without any saline wash, has a similar property to the nitric; but as it is readily affected by faint light, it is of greater value. However, papers prepared with it must be used within twenty-four hours, as after that they quickly lose their sensitiveness, and soon become nearly useless.

19. *The Hydriodic Solutions.*—To fix with any degree of certainty the strength of the solution of the hydriodic salts, which will in all cases produce the best effect, appears to me impossible; every variety of paper, either as regards its composition, or the intensity of light to which it has been exposed to darken, requiring a solution of different specific gravity.

20. *Hydriodates of Potassa and Soda.*—The former of these salts being more easily procured than any other of the

hydriodates, is the one generally employed. The strength at which I use these salts for most kinds of paper is thirty grains to an ounce of water. The following results will exhibit the different energies manifested by these solutions at several strengths, as tried on the same paper by the same light.

120	grains of salt	to an ounce of water took to whiten		12	minutes.
100	do.	to	do.	10	—
80	do.	to	do.	9	—
60	do.	to	do.	7	—
40	do.	to	do.	6	—
30	do.	to	do.	4	—
20	do.	to	do.	6	—
10	do.	to	do.	12	—

The other hydriodic salts correspond nearly with these in their action; a certain point of dilution is necessary with all.

21. Hydriodate of ammonia, if used on unsized paper, has some advantage as to quickness over either the salts of potassa or soda. This preparation is, however, so readily decomposed, that the size of the paper occasions a liberation of iodine, and the consequent formation of yellow-brown spots.

22. *Hydriodate of Iron.*—This metallic hydriodate acts with avidity on the darkened paper; but even in the shade its chemical energy is too great, destroying the sharpness of outline and impairing the middle tints of the drawing. It also renders the paper very yellow.

23. *Hydriodate of lime* acts similarly to the iron, but less energetically, and the paper is not rendered yellow by it.

24. *Hydriodate of manganese* answers remarkably well when it can be procured absolutely free of iron. When the manganesic solution contains it, even in the smallest quantities, light and dark spots are formed over the picture, which give it a curious speckled appearance.

25. *Hydriodic acid*, if used on paper which will not decompose its aqueous solution, acts readily on the darkened silver. It is difficult, however, to procure a paper which does not liberate the iodine. A portion of hydriodic acid, free, in any of the saline solutions, greatly quickens the action.

26. *Hydriodate of baryta* possesses advantages over every other simple hydriodic solution, both as it regards quickness of action, and the sharpness of the outline in the photograph.

27. I find, however, the quickness of this solution may be much increased. Forty grains of the hydriodate of baryta being dissolved in one ounce of distilled water, thereto should be added five grains of pure sulphate of iron, and allowed slowly to dissolve. Sulphate of baryta is precipitated, which

should be separated by filtration, when the solution is composed of the hydriodate of baryta and iron. By now adding a drop or two of very dilute sulphuric acid, more baryta is precipitated and hydriodic acid left free. The clear solution must be decanted off, as the filtering through paper decomposes the acid. By this means a photographic fluid of great value is formed. It should be prepared in small quantities, as it suffers decomposition under the influence of the atmosphere and of light. It is always easy to set hydriodic acid free by precipitating sulphate of baryta.

28. *Directions for taking Photographs.*—For drawings by application less care is required than for the camera obscura. With a very soft flat brush apply the hydriodic solution on both sides of the prepared paper until it appears equally absorbed, place it in close contact with the object to be copied, and expose to sunshine. The exposure should continue until the light parts of the picture (iodide of silver (54.)) are seen to brown. The observance of this simple rule will be found of very great advantage in practice. Immersion for a short time in soft water removes the brown hue, and renders the bright parts of the picture more clear than they would otherwise have been.

29. If the paper is intended to be used in the camera, it is best to soak it in the hydriodic solution, until a slight change is apparent from the chemical action on the silver; it is then to be stretched on a frame, and not allowed to touch in any part but at the edges; placed in the dark chamber of the camera at the proper focus, and submitted to luminous influence.

If the wetted paper is placed upon any porous body, it will be found, owing to the capillary communication established between different points, that the solution is removed from some parts to others, and different states of sensitiveness induced. Another advantage of the frame is, the paper being by the moisture rendered semi-transparent, the light penetrates and acts to a greater depth, thus cutting out fine lines which would otherwise be lost. However, if the camera is large, there is an objection to the frame; the solution is apt to gather into drops, and act intensely on small spots to the injury of the general effect. When using a large sheet, the safest course is to spread it out when wetted upon a piece of very clean wet glass, great care being taken that the paper and glass are in every part in close contact. The picture is not formed so quickly when the glass is used as when the paper is extended on a frame, owing to the evaporation being slightly retarded; the additional time required, about one-sixth longer, is however in most cases of small consequence. It is somewhat singular that if the glass plate is interposed between the paper and the lens, the action is not more retarded than if it had been placed behind it. The interference of a transparent plate is little felt in the hydriodic process.

30. *On fixing these Photographs.*—The picture being formed by the influence of light, it is required, to render it unchangeable by any further action of the luminous fluid, not only that the hydriodic salt be entirely removed from the paper, but that the iodide of silver which is formed be also dissolved out of the drawing.

31. By well washing the drawing in warm water the hydriodate is removed, and the pictures thus prepared have been stated to be permanent; and if they are kept in a portfolio, and only occasionally exposed, they are really so; for I shall show presently (54.) that they have the property of *being restored in the dark to the state in which they were prior to the destructive action of light.* I have now before me the first drawing of this kind I ever executed, bearing the date June 17, 1839. This drawing has been kept loosely in my table drawer, and has often been exposed for many successive days to the action of the sun; yet the most delicate venations of the rose leaves are as perfect as at first. Thus prepared, however, these photographs will not bear continued exposure without injury, about three months in summer, or six weeks in winter being sufficient to destroy them.

32. For a long period I was under the impression that two iodides of silver existed, the one sensitive to solar influence, but the other not so; and in my paper published in your Magazine for April, I stated such to be my opinion. I have, however, since that period seen reason sufficient to question the correctness of my conclusion. Under the former impression, not being successful in removing the iodide from the paper without also injuring the oxidized or dark portions, I endeavoured to effect a chemical change in the iodide of silver. Some of the results being curious, I shall give them.

33. By washing the photograph with a hot saturated solution of the acetate of lead, the yellowness of the lights was at first increased, but eventually considerably whitened, and the dark parts assumed a peculiar crimson hue. The drawing faded out entirely by the action of light in three weeks.

34. When these drawings are dipped into a solution of the bichloride of mercury, they fade out in precisely the same manner as Sir John Herschel discovered the photographs on Mr. Talbot's principle were obliterated, and in like manner

are they restored by a liquid hyposulphite; the paper, instead of being completely white, being altogether of a full rich yellow. When these photographs are restored by the hyposulphite, they are even less permanent under the influence of light than those washed with the salt of lead.

35. The ferrocyanate of potassa exerts no action on these photographs in any way remarkable, unless they have been formed by the agency of the hydriodate of iron (22.) or of baryta and iron (27.). They are then obliterated by it, but on exposure, the light parts of the picture are darkened, changing thus to a *negative* photograph, the originally dark parts being now a light blue.

36. With much attention, I have tried the hyposulphites of soda, ammonia and potassa. But I have failed to remove all the iodide of silver, without destroying at the same time the dark parts, and the minute portion which remains in the paper is very soon darkened by light to a tint similar to the lighter shades of Indian ink. When first done the drawing is much improved in appearance, but it is difficult to remove the hyposulphite so completely as is necessary to prevent the formation of the sulphuret of silver.

37. Sulphuretted hydrogen gas, which has the singular property of blackening the iodide of silver, when in that state which is easily darkened by light, *but of bleaching it in the less susceptible state,* acts on these photographs in a manner similar to the hyposulphites; but the oxidized portions of the picture are first destroyed and then restored by light. The light parts are, however, rendered brown.

I have tried a great variety of other agents, diversifying my method of using them in almost every possible way, but as yet I have discovered no material which effectually removes the iodide of silver alone; consequently I satisfy myself with well washing my photographs in hot water.

[To be continued.]

XXX. *On the Anthracite Coal of South Wales.* *By* Samuel Woods, *Esq., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING received from a friend at Paris the accompanying analysis of some of the anthracite coal found abundantly in this neighbourhood, I transmit it to you in the belief that it may be worthy of record in your pages; and the quality and uses of this coal having lately excited so much attention and inquiry, a few preliminary observations may not prove unacceptable.

This deposit of anthracite coal forms a part of the large Welsh coal basin extending from Brides-bay to Pontypool, —the western limit of the anthracite (or stone coal) deposit being the coast of Pembrokeshire, and its eastern terminating not far beyond the Neath Valley: the seams towards Merthyr and beyond, gradually losing their anthracetous and assuming the bituminous character, the width till it reaches Llandshipping in Milford-haven is inconsiderable, from whence it slowly expands towards Tenby. In this district there do not appear to exist more than three or four distinct veins, all of which seem to have been exposed to intense heat, and to disturbing forces of great violence, these veins abounding in faults and tossed about in various directions, very difficult to follow, a large proportion being reduced into small particles known by the denomination of culm, employed chiefly in lime-burning: the larger pieces, however, possess a very uniform compact character and superior purity, and are in great reputation and demand for drying malt, for which purpose they have been long used. The veins then appear to pass under Carmarthen-bay, and again to emerge in the Gwendraeth Valley beyond Kidwelly, where a vast quantity is procurable. From the southern edge of the basin from Pembrey to Swansea and Neath, the coal is chiefly bituminous, but occasionally of a mixed character, more or less approaching to anthracite, the latter becoming more pure and distinct as we advance northward through the collieries of Trimsaran, Brondyny, Llangennych and Pont-twrch in the Swansea Valley. To the north of all these points the quality rapidly improves, and all the coal north of a line drawn from Pont Yales to Aperpergwm, may be considered real anthracite. The Gwendraeth Valley from near Pont Yales to the Big mountain, a distance of about eight miles trending N.E. is known to possess thirteen distinct veins, somewhat varying in purity and thickness, the latter in the aggregate amounting to about 40 feet, and the greatest depth hitherto worked, or in contemplation to work, about 65 fathoms; the dip from 30° to 45°. Seams of argillaceous iron ore are also found between the coal veins.

The use of this coal has hitherto been chiefly confined to maltsters; it has been more recently adopted for Dr. Arnott's and other stoves, and is now successfully applied to the reduction of iron ores in the vicinity of Swansea; and its freedom from smoke, joined to the durability of its heat, offer the strongest recommendations for its employment in locomotive engines either on land or water, and there is little doubt will be ultimately adopted for these purposes. There seem at present some difficulties as to the best mode of using this combustible; since by sudden exposure to an incandescent furnace it is apt to split or exfoliate into minute fragments, which choke up the draught of air, or are thrown out like dust by any rapid motion. Dr. Ure assigns as the cause of this, I believe truly, that being a bad conductor of heat, the superficial parts expand and break off from the cooler internal portions; the remedy for which appears to be some contrivance for gradually heating the coal before it comes into contact with the fire, and of supplying a constant current of hot air; these precautions are not required for common fires. Mr. Player has secured a patent for such a process, which is exhibited on the Thames in the steamer called the Anthracite, the action of which every one is disposed to praise, yet no one adopts: for this no reason appears but the difficulty of overcoming prejudices, or the fear of engaging in novelties in the first instance requiring some additional expenditure.

With regard to its application to domestic use it has many and forcible recommendations; it gives out a clear, steady and durable heat; requires but little attention when once lighted; and the absence of all annoyance from smoke, soot or dust, renders it very desirable for culinary purposes and for bedrooms; in the parlour it may be thought deficient in the bright and cheerful character which belongs to the Newcastle coal; besides which, as the anthracite never cakes, it requires no aid from the poker, the employment of which on the contrary extinguishes the fire, and therefore may be deemed objectionable.

Analysis of the vein of Anthracite coal called the Gwerdd (Green) vein from Coalbrook in Carmarthenshire near Pont y berem, in the vale of the Gwendraeth, the property of the Gwendraeth Anthracite Company; by Mons. Jacquelain, of the *Ecole des Arts* at Paris.

Ultimate analysis.	
Carbon ...	89·43
Hydrogen	3·56
Oxygen ...	3·66
Azote......	0·29
Ashes	1·70
Hygrometric moisture........	1·36
	100

Manufacturing analysis.			
	Carbon	89·80	Coke 91·50
	Ashes .	1·70	
	Water	1·35	
Volatile substances	slightly carburetted hydrogen, ammonia and oxygen	7·15	
		100	

One gramme of the coal reduces 33·3 grammes of lead, consequently the heating power may be thus estimated: 1 kilogramme is capable of raising 76·54 kilogrammes of water (quere from 32°) to the boiling point, or of evaporating 11·55 kilogrammes. All the samples of coal placed in my hands were of a brilliant black, very compact, and of a lamellar structure; the cross fracture rough and uneven; they do not soil the fingers, and break easily under the hammer; the hardness is nevertheless considerable, and it is difficult so to reduce it to powder as to destroy its brilliancy.

The double carbonate of lime and iron occurs between the laminæ of this coal, and sometimes agglomerations of carbonaceous matter possessing the appearance, lightness and friability of wood charcoal.

The specific gravity of this coal is 1·27; it burns in the furnace without flame; a small quantity reduced to powder consumes slowly without inflaming; ignited masses do not lose their form, but when separately exposed to the air become extinguished, whereas in mass the combustion succeeds well in the reverbatory furnace.

I incinerated some of this coal reduced to powder in a muffle furnace charged with the same coal, and beginning with a stratum of lighted charcoal: the combustion continued for five hours, while every part of the interior of the furnace was incandescent.

The residue of this incineration upon a small scale, consists of slightly ferruginous and calcareous ashes; that of combustion on the large scale is similar, but mixed with some small pieces of coal having suffered incipient exfoliation. The ready combustion in mass is undoubtedly to be attributed to a slight separation of the laminæ of the coal produced by the high temperature, and to the emission of a gas*.

The quantity of gas which this coal is capable of supplying in close vessels has been found to be 24 litres for 100 grammes of coal, equivalent to 240 litres for a kilogramme. It will be seen that these results approach those obtained on the large scale in gas-works, where that quantity yields 180, 200, and 250 litres of gas; unfortunately the gas produced from this anthracite does not give more light than pure hydrogen; it is now well known that illuminating power may be communicated to hydrogen gas by causing it to circulate in reservoirs over the surface of oil of schist or of tar.

From what has been stated, it is evident that coal having a uniform character similar to that which I have analysed, would be much in demand on account of its remarkable purity, both for domestic consumption and for blast furnaces, and especially for the latter, on account of the extremely high temperature it is capable of producing.

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* I do not exactly comprehend how this separation of the laminæ promotes combustion. I should have thought its tendency would have been to choke and check the fire.—S. W.

I repeat, that if this combustible prove homogeneous and of equal quality in every part of the deposit, the discovery is one of great importance for the reduction of ores and the quality of the resulting products.

Dr. Schafhaeutl of Munich, now or lately residing at Swansea, has analysed for the Kilgetty Company, two samples of the Pembrokeshire coal; the average specific gravity he states to be 1·413.

	1.	2.
Carbon........	92·42	94·100
Hydrogen......	3·37	2·390
Oxygen.........	1·43	1·336
Nitrogen.......	1·05	·874
Sulphur........	·12	——
Earthy matter	1·61	·068
Alumina.........		·478
Silica		·190
Iron		·264
Water.		·300
	100	100

Pembrey, Carmarthenshire.

XXXI. *On the Tension Spark from the Voltaic Battery. By* Andrew Crosse, *Esq. Communicated in a Letter to* John P. Gassiot, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

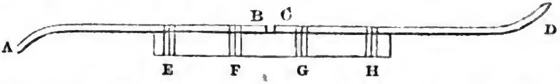
THE inclosed communication from my friend Andrew Crosse, Esq., may perhaps be interesting to the readers of the Philosophical Magazine; the paper he alludes to is one I communicated to the Royal Society, and which was honoured by a place in their Transactions of this year.

I am, Gentlemen, yours, &c.

Clapham Common, Aug. 3, 1840. John P. Gassiot.

My dear Sir,

I return you my best thanks for your paper on the voltaic spark, &c.; I have read it with attention, and was much interested by its perusal, particularly by that part which relates to Zamboni's pile. I once had a cork ball kept vibrating between the poles of four columns of De Luc's pile, without cessation, for upwards of twelve years! Now for the possibility of obtaining a spark between the poles of a voltaic battery before the circuit is completed. Were you to see the action of my unfinished water battery of 1626 pairs of zinc and copper cylinders, you would allow the question to be set at rest. I take a small glass stick, and tie on it, with waxed silk thread, very securely, two wires of platina, with the two extreme ends ready to be plunged into two cups of mercury connected with the opposite poles of the battery: the two other ends of the wires are brought to the distance of about $\frac{1}{100}$th of an inch from each other, as below.



A B, C D, two platina wires secured on the glass stick E, F, G, H at the parts E, F, G, H. The two nearest ends of the wires approach each other at B, C to about the distance of $\frac{1}{100}$th of an inch. I say about, for I have no instrument to measure it with accuracy, nor is it of any consequence, as the cells of that battery are not by any means so well insulated, as that the above distance should be taken as a test of intensity of the battery. The moment the connexion is made with the poles of the battery, a small stream of fire takes place at the interval between B and C, which I have kept up for many minutes, nor did it appear inclined to cease. This experiment never fails, but with a much greater number of plates, each pair not being separately insulated, it would never succeed. To expect to produce a spark or visible current under similar circumstances with the above would be hopeless, except with a considerable number of pairs of plates, each pair being separately insulated. With 1200 pairs I have succeeded, and with 10,000 or 20,000 the distance at which it would strike would be very great, comparatively speaking.

I showed a friend the other day about twelve inches of iron chain illuminated pretty strongly by the passage of repeated shocks of my large electrical battery through it, charged by the water battery alone. The intensity was so great as to keep up a constant dance of pieces of silver-leaf between two plates connected with the opposite poles. The reason why Professor Daniell's water battery, which you employed in some of your experiments, failed, was first, that the cells were not separately insulated, and secondly, that they were too few in number. I presume it was the water battery which I once saw used at the Royal Institution, the intensity of which was very feeble.

I do wish you could manage to pay me a visit, and see the action of my battery. I mean, as soon as I have time to add about 850 pairs of cylinders to it. Woe then to the unfortunate wretch who comes between the poles, when connected with the electrical battery!

P. S. Please to observe, that when I procured the stream of electricity in the interval between the platina wires, I used the water battery alone, without other apparatus, and not connected with the electrical or any other battery.

Broomfield, near Taunton, July 17, 1840.

XXXII. *On the Process of Daguerreotype, and its application to taking Portraits from the Life. By* JOHN WILLIAM DRAPER, *M.D., Prof. Chemistry in the University of New York.*

VERY soon after M. Daguerre's remarkable process for Photogenic Drawing was known in America, I made attempts to accomplish its application to the execution of portraits from the life. M. Arago had already stated, in his address to the Chamber of Deputies, that M. Daguerre expected, by a slight advance, to meet with success, but as yet no account has reached us of that object being attained.

More than one hundred instances are recorded in Berzelius's chemistry, in which the agency of light brings about changes in bodies; these are of all kinds: formations of new compounds, re-arrangements of elements already in union, changes of crystallographic character, decompositions, and mechanical modifications.

The process of the Daguerreotype is to expose a surface of pure silver to the action of the vapour of iodine, so as to give rise to a peculiar iodide of silver, which under certain circumstances is exceedingly sensitive to light. The different operations of polishing, washing with nitric acid, exposure to heat, &c., are only to offer a pure silver surface; the operation of hyposulphite of soda, and the process, which I shall presently describe, of galvanization, are to free the plate from its sensitive coating, and in no wise affect the depth of the shadows, as some of the French chemists at first supposed.

There is but one part of the Daguerreotype which does not yield to theory: on one point alone there is obscurity. Why does the vapour of mercury condense in a white form on those portions of the film of iodide, which have been exposed to the influence of light?—condense to an amount which is rigidly proportional to the quantity of incident light?

Even on this point there are facts which appear to have a bearing.

(*a*.) It has long been known, that if a piece of soapstone or agalmatolite be made use of as a pencil to write with on glass, though the letters that may have been formed are invisible, and though the surface of the glass may subsequently have been well cleared, yet they will come into view as soon as the glass is breathed on.

(*b.*) I have often noticed, that if a piece of very clear and cool glass, or what is better, a cold polished metallic reflector, has a little object, such as a piece of metal, laid upon it, and the surface be breathed over once, the object being then carefully removed, as often as you breathe again on the surface, a spectral image of it may be seen, and this singular phænomenon may be exhibited for many days after the first trial was made.

(*c.*) Again, in the common experiment of engraving on glass by hydrofluoric acid, if the vapour has been very weak, no traces will be perceived on the glass after the wax has been removed; but on breathing over it, the moisture condenses in such a way, as to bring all the object into view.

(*d.*) In a former number of this Journal I described a phænomenon which relates to the crystallization of camphor on surfaces of dry glass, on which moveable traces have been made by the pressure of a glass rod; this also appears to belong to the same class of effects.

Berzelius (Traité, vol. ii. p. 186.) has attempted to explain (*a.*) and (*c.*) on this principle, that the changed and unchanged surfaces radiate heat unequally. There may be strong doubts with some as to the correctness of this, but is not the Daguerreotype due to the same cause, whatever it may be?

We must separate carefully the chemical changes which iodide of silver undergoes in the sunbeam, from the mechanical changes which happen to the sensitive film: iodide of silver turns black in the solar ray, the whole success of the Daguerreotype artist depends on his checking the process before that change shall have supervened.

The coating of iodine is not *immediately* necessary to the production of images by the mercurial vapour. The condition seems to be traceable to the metallic surface. If you take a Daguerreotype, clean off the mercury, polish the plate thoroughly with rottenstone, wash it with nitric acid and bring it to a brilliant surface, yet if it has not been exposed to heat, the original picture will re-appear on exposure to the mercurial vapour. Is not this a result of the same kind as those just referred to?

As a polishing material for the Daguerreotype plate, common rottenstone and oil answer very well. The plate having been planished by the workman, is to be rubbed down to a good surface, and as high a polish given to it as possible;

it is to be heated and washed with nitric acid, as indicated in the French account, and finished by being rubbed with whiting (*creta præparata*), in the state of a very dry powder, going over it for the last time with a piece of clean dry cotton; this gives an intensely black lustre, which cannot be obtained by rottenstone alone, and thoroughly removes any film which nitric acid may have left.

To coat with iodine, I make use of a box about two inches deep, in the bottom of which that substance in coarse flakes is deposited; no cloth intervenes, but the silvered plate, with a temporary handle attached to it, is brought within half an inch of the crystals, and it becomes perfectly coated in the course of from one to three minutes; no metallic strips are necessary to ensure this effect; if the edges and corners are thoroughly clean, the golden hue will appear uniformly.

M. Daguerre recommends, that the plate, after being iodized, shall be placed in the camera without loss of time. The longest interval, he says, ought not to exceed an hour. "Beyond this space the action of the iodine and silver no longer possesses the requisite photogenic properties."

There may be something peculiar in the preparation of the plate as I have described it, but it is certain that this observation must be received with some limitation. A plate, which has been iodized, does not appear so quickly to lose its sensitiveness. On the other hand, by keeping it in the dark for twelve or twenty-four hours, its sensitiveness is *often remarkably increased.* Other advantages also accrue. Those who have made many of these photogenic experiments, will have had frequent occasion to remark, that the film of iodine is not equally sensitive all over, that there are spots or cloudy places which do not evolve any impression, and often the whole is in that condition, that the bright parts alone come out, while the parts that are in shadow do not evolve correspondingly, nor can they be well developed, except at the risk of solarizing the picture. Now, a plate that has been kept for several hours, is by no means so liable to these effects: I do not pretend to give any reason for this, but merely mention it as a fact, of considerable importance to the travelling daguerreotyper; he will find that the iodine does not lose its sensitiveness in many days.

In a paper read before the Royal Society, of which an abstract is given in the April number of this Journal for the present year (p. 333.), Sir John Herschel states, that there is an absolute necessity of a perfect achromaticity in the object-glass of a photographic camera. M. Daguerre appears to have been under the same impression, and recommends in his published account such an object-glass.

All the rays of light, with *perhaps* the exception of the yellow, leave an impression on the iodide of silver. The less refrangible rays, however, act much more slowly than those which are at the opposite end of the spectrum. In the common kinds of glass, the most energetic action takes place in the indigo, or on the boundary of the blue. Now the retina receives an impression with equal facility from each of the different rays, the yellow light acting as quickly upon it as the red or the blue. Vision is therefore performed independently of time, the eye catching all the colours of the spectrum with equal facility and with equal speed. But it is not so with these photogenic preparations. In the action of light upon them, time enters as an element; the blue ray may have effected its full change, whilst the red is yet only beginning slowly to act; and the red may have completed its change before the yellow has made any sensible impression. On these principles, it is plain that an achromatic object-glass is by no means essential for the production of fine photographs; for if the plate be withdrawn at a certain period, when the rays that have a maximum energy have just completed their action, those that are more dispersed but of slower effect, will not have had time to leave any stain. We work, in fact, with a temporary monochromatic light.

Upon these principles I constructed the camera which I am in the habit of using, with a double convex non-achromatic lens. Some of the finest proofs were procured with a common spectacle lens, of fourteen inches focus, arranged at the end of a cigar-box as a camera; a lens of this diameter answers very well for plates four inches by three, reproducing the objects with the most admirable finish, copper-plate engravings being represented in the minutest particulars, and the marks of the tool becoming quite distinct under the magnifier.

In this instance, it is true, owing to the magnitude of the focal length compared with the aperture, but little difficulty ensues from chromatic aberration; but when with the same focal length the aperture is increased to three or four inches, then the dispersion becomes very sensible, and yet good proofs can be procured, by working in the method here indicated, the chief difficulty then arising from spherical aberration.

It has already been stated, that the ray of maximum action for the Daguerreotype, when colourless French plate-glass is used, lies probably within the indigo space: it therefore follows, that the length of the camera should be diminished, after arranging it to the luminous focus. The importance of this is pointed out in a paper by Mr. Towson, inserted in this Journal last year; I was, however, in the habit of using this adjustment before reading the suggestions contained in that excellent communication. The amount of shortening which should be given to the camera, where the lens is fifteen inches focus, does not commonly exceed three-tenths of an inch. If the luminous focus be used, the proof comes out indistinct.

In the subsequent process of mercurializing, it is of little importance what is the angular position. Several experimenters were for a time under the idea that an angle of 45° or 48° was a necessary inclination, in order that the plate should take the vapour; this arose from a misinterpretation of the printed account. Plates mercurialize equally well in a horizontal as in any other position; perhaps a slight inclination may be of advantage, in allowing the vapour to flow with uniformity over the iodized surface, but the chief use of an angle of 45°, is to allow the operator to inspect the process through the glass.

Sometimes it is advantageous to heat the mercury a second time, when the proof is not distinctly evolved at first. Indeed, it occasionally happens, that a proof which did not evolve at all at first, will come out quite fairly on raising the temperature of the mercury again.

M. Daguerre recommends two methods of removing the sensitive coating from the plate, by washes of hyposulphite of soda, and a solution of common salt. The former answers perfectly, the second only indifferently well. There is, however, another process, which is very simple, and has an advantage over the former of these in cheapness. It adds not a little to the magic of the whole operation, in the eyes of those who are unaccustomed to chemical results. The plate, having been dipped into cold water, is placed in a solution of common salt, of moderate strength; it lies without being acted upon at all; but if it be now touched on one corner with a piece of zinc, which has been scraped bright, the yellow coat of iodide moves off like a wave and disappears. It is a very pretty process. The zinc and silver forming together a voltaic couple, with the salt water intervening, oxidation of the zinc takes place, and the silver surface commences to evolve hydrogen gas; whilst this is in a nascent condition it decomposes the film of iodide of silver, giving rise to the production of hydriodic acid, which is very soluble in water, and hence instantly removed.

This process, therefore, differs from that with hyposulphite. The latter acts by dissolving the iodide of silver, the former by decomposing it. It is necessary not to leave the zinc in contact too long, or it deposits stains, and in large plates the contact should be made at the four corners successively, to avoid this accident.

After the proof is washed, all the defects in the preparation of the plate become apparent. If a film of mercury has existed on it, due to its not having been burnt sufficiently long, there will be found a want of distinctness in the shadows; or if the plate has not been burnt at all, perhaps the former impressions which have been obtained will re-appear. This accident frequently happened in my earlier trials, when care had not been taken to give a due exposure each time to the spirit flame. Spectral appearances of former objects, on different parts of it, emerged,—an interior with Paul Pry coming out, when the camera had been pointed at a church.

There is no difficulty in procuring impressions of the moon by the Daguerreotype, beyond that which arises from her motion. By the aid of a lens and a heliostat, I caused the moonbeams to converge on a plate, the lens being three inches in diameter. In half an hour a very strong impression was obtained. With another arrangement of lenses I obtained a stain nearly an inch in diameter, and of the general figure of the moon, in which the places of the dark spots might be indistinctly traced.

An iodized plate, being exposed for fifteen seconds only close to the flame of a gas light, was very distinctly stained; in one minute there was a very strong impression.

On receiving the image of a gas light, which was eight feet distant, in the camera, for half an hour, a good representation was obtained.

The flame of a gas lamp was arranged within a magic lanthorn, and a portion of the image of a grotesque on one of the slides received on a plate; a very good representation was procured.

With Drummond's light, and the rays from a lime-pea in the oxy-hydrogen blowpipe, the same results were obtained.

In the first experiments which I made for obtaining portraits from the life, the face of the sitter was dusted with a white powder, under an idea that otherwise no impression could be obtained. A very few trials showed the error of this; for even when the sun was only dimly shining, there was no difficulty in delineating the features.

When the sun, the sitter, and the camera are situated in the same vertical plane, if a double convex non-achromatic lens of four inches diameter and fourteen inches focus be employed, perfect miniatures can be procured, *in the open air*, in a period varying with the character of the light, from 20 to 90 seconds. The dress also is admirably given, even if it should be black; the slight differences of illumination are sufficient to characterize it, as well as to show each button, button-hole, and every fold.

Partly owing to the intensity of such light, which cannot be endured without a distortion of the features, but chiefly owing to the circumstance that the rays descend at too great an angle, such pictures have the disadvantage of not exhibiting the eyes with distinctness, the shadow from the eyebrows and forehead encroaching on them.

To procure fine proofs, the best position is to have the line joining the head of the sitter and the camera so arranged as to make an angle with the incident rays of less than ten degrees, so that all the space beneath the eyebrows shall be illuminated, and a slight shadow cast from the nose. This involves obviously the use of reflecting mirrors to direct the ray. A single mirror would answer, and would economise time, but in practice it is often convenient to employ two; one placed, with a suitable mechanism, to direct the rays in vertical lines; and the second above it, to direct them in an invariable course towards the sitter.

On a bright day, and with a sensitive plate, portraits can be obtained in the course of five or seven minutes, in the diffused daylight. The advantages, however, which might be supposed to accrue from the features being more composed, and of a more natural aspect, are more than counterbalanced by the difficulty of retaining them so long in one constant mode of expression.

But in the reflected sunshine, the eye cannot support the effulgence of the rays. It is therefore absolutely necessary to pass them through some blue medium, which shall abstract from them their heat, and take away their offensive brilliancy. I have used for this purpose blue glass, and also ammoniaco-sulphate of copper, contained in a large trough of plate glass, the interstice being about an inch thick, and the fluid diluted to such a point, as to permit the eye to bear the light, and yet to intercept no more than was necessary. It is not requisite, when coloured glass is employed, to make use of a large surface; for if the camera operation be carried on until the proof *almost* solarizes, no traces can be seen in the portrait of its edges and boundaries; but if the process is stopped at an earlier interval, there will commonly be found a stain, corresponding to the figure of the glass.

The camera I have used, though much better ones might be constructed, has for its objective two double convex lenses, the united focus of which for parallel rays is only eight inches; they are four inches in diameter in the clear, and are mounted in a barrel, in front of which the aperture is narrowed down to 3½ inches, after the manner of Daguerre's.

The chair in which the sitter is placed, has a staff at its back, terminating in an iron ring, that supports the head, so arranged as to have motion in directions to suit any stature and any attitude. By simply resting the back or side of the head against this ring, it may be kept sufficiently still to allow the minutest marks on the face to be copied. The hands should never rest upon the chest, for the motion of respiration disturbs them so much, as to bring them out of a thick and clumsy appearance, destroying also the representation of the veins on the back, which, if they are held motionless, are copied with surprising beauty.

It has already been stated, that certain pictorial advantages attend an arrangement in which the light is thrown upon the face at a small angle. This also allows us to get rid entirely of the shadow from the back-ground, or to compose it more gracefully in the picture; for this, it is well that the chair should be brought forward from the back-ground, from three to six feet.

Those who undertake Daguerreotype portraitures, will of course arrange the back-grounds of their pictures according to their own tastes. When one that is quite uniform is desired, a blanket, or a cloth of a drab colour, properly suspended, will be found to answer very well. Attention must be paid to the tint,—white, reflecting too much light, would solarize upon the proof before the face had had time to come out, and owing to its reflecting *all* the different rays, a blur or irradiation would appear on all edges, due to chromatic aberration. It will be readily understood, that if it be desired to introduce a vase, an urn, or other ornament, it must not be arranged against the back-ground, but brought forward until it appears perfectly distinct on the obscured glass of the camera.

Different parts of the dress, for the same reason, require intervals, differing considerably, to be fairly copied; the white parts of a costume passing on to solarization before the yellow or black parts have made any decisive representation. We have therefore to make use of temporary expedients. A person dressed in a black coat, and open waistcoat of the same colour, must put on a temporary front of a drab or flesh colour, or by the time that his face and the fine shadows of his woollen clothing are evolved, his shirt will be solarized, and be blue, or even black, with a white halo around it. Where, however, the white parts of the dress do not expose much surface, or expose it obliquely, these precautions are not essential; the white shirt collar will scarcely solarize until the face is passing into the same condition.

Precautions of the same kind are necessary in ladies, dresses, which should not be selected of tints contrasting strongly.

It will now be readily understood, that the whole art of taking Daguerreotype miniatures, consists in directing an almost horizontal beam of light, through a blue coloured medium, upon the face of the sitter, who is retained in an unconstrained posture, by an appropriate but simple mechanism, at such a distance from the back-ground, or so arranged with respect to the camera, that his shadow shall not be copied as a part of his body; the aperture of the camera should be three and a half or four inches at least, indeed the larger the better, if the object be aplanatic.

If two mirrors be made use of, the time actually occupied by the camera operation varies from forty seconds to two minutes, according to the intensity of the light. If only one mirror is employed, the time is about one-fourth shorter. In the direct sunshine, and out in the open air, the time varies from under half a minute.

Looking-glasses, which are used to direct the solar rays, after a short time undergo a serious deterioration; the foil assuming a dull granular aspect, and losing its black brilliancy. Hence the time, in copying, becomes gradually prolonged.

The arrangement of the camera, above-indicated, gives reversed pictures, the right and left sides changing places. Mr. Woolcott, an ingenious mechanician of this city, has taken out a patent for the use of an elliptical mirror for portraiture; it is about seven inches in aperture, and allows him to work conveniently with plates two inches square. The concave mirror possesses this capital advantage over the convex lens, *that the proof is given in its right position, that is to say, not reversed;* but it has the serious inconveniences of limiting the size of the plate, and representing parts that are at all distant from the centre, in a very confused manner. With the lens, plates might be worked a foot square, or even larger.

Miniatures procured in the manner here laid down, are in most cases striking likenesses, though not in all. They give of course all the individual peculiarities, a mole, a freckle, a wart. Owing to the circumstance, that yellow and yellowish browns are long before they impress the substance of the Daguerreotype, persons whose faces are freckled all over give rise to the most ludicrous results, a white, mottled with just as many black dots as the sitter had yellow ones. The eye appears beautifully; the iris with sharpness, and the white dot of light upon it, with such strength and so much of reality and life, as to surprise those who have never before seen it. Many are persuaded, that the pencil of the painter has been secretly employed to give this finishing touch.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 153.]

Dec. 18, 1839. A PAPER was afterwards read, entitled, "Observations on the locality of the Hyracotherium," by William Richardson, Esq., F.G.S.

In 1829, when Mr. Richardson first examined the coast from Whitstable to Herne Bay, it presented an uniform, geological structure, composed of a capping of vegetable mould, under which was a stratum 3 or 4 feet thick of yellow brick earth, containing in the upper part rolled and angular flints, mammalian remains and fossils derived from secondary strata; and beneath, forming the mass of the cliff, was London clay of a dark brown colour, abounding in septaria, selenite, pyritous wood, teeth and vertebræ of fishes, Nautili with other characteristic marine testacea, Encrinital and Pentacrinital remains, and crustaceans.

The whole of the line of coast undergoes rapid degradation in consequence of the encroachment of the sea and land springs; and the changes thus annually produced,effect great alterations in the physical outline of the cliffs. The geological structure, however, presented by them in 1829 remained for the greater part the same in the autumn of 1839, except at the part called Studd Hill. At this point, the dark brown incoherent clay had been removed, and a deep blue, tenacious one exposed. A change had also taken place in the character of the fossils, the marine remains having gradually become less prominent and been replaced by others of a fluvio-marine character. In the autumn of last year, Mr. Richardson could not find a single marine shell, and only a few fragments of crinoidal stems. Terrestrial vegetables have, however, become so prodigiously abundant, that he has obtained at different times above 500 fossil cones, fruits, and other seed-vessels; and fragments of small trees converted into pyrites occur in so great quantities, that they have been removed by barge loads for œconomical purposes, and become a source of considerable profit to the neighbouring peasantry. These remains present no indications of having been transported from a distance. Neither land nor fresh-water shells have been observed.

From the abundance of vegetables, and the knowledge that Nature ever directs her means as well in number as in fitness to particular ends, Mr. Richardson inferred, that remains either of quadrupeds or birds would be found in Studd Hill; and though his search was long fruitless, it was at last rewarded by the discovery of the portion of the Hyracotherium described by Mr. Owen in the preceding memoir.

January 8, 1840. A paper was first read, on the carboniferous and transition rocks of Bohemia, by David T. Ansted, Esq., F.G.S.

After alluding to the difficulties which beset the researches of a geologist in a country so little frequented as that visited by himself, and noticing the granite and gneiss mountains which constitute the south-eastern and south-western boundaries of Bohemia, he proceeds to the main object of the memoir. The district described by Mr. Ansted is included within a triangle, having the country between Luditz and Pilsen for a base, and Prague for its apex; and its structure is explained by a series of sections from Luditz to Pilsen—Radnitz to Rakonitz—Zebrak to Ginetz—and Przilep to Karlstein, all of them being more or less in the dip of the strata. The formations composing the district, are granite, gneiss, graywacke, coal measures, trap rocks and accumulations of superficial debris. It is stated that a line drawn from Eger on the west to Prague on the east would completely separate the sedimentary deposits of a newer date than the carboniferous system from the coal measures and transition rocks; and that the latter occur only to the south of the line. Near Eger is a small local deposit of upper tertiary sandstone, mentioned by Mr. Ansted on account of its containing myriads of fossil infusoria cases.

Section 1. *Luditz to Pilsen.*—Luditz stands upon a range of round topped gneiss hills, but in a depression between two of them; and about 3 miles from the town, is a bed of thinly laminated micaceous sandstone, containing a few obscure vegetable markings, and believed by Mr. Ansted to be a recent deposit. Proceeding in the direction of Pilsen, the gneiss is succeeded by a hard cherty stone, considered by the author to belong to a rock which underlies the coal measures in other parts of the country, but to have been protruded at this point by igneous agency. The next hill is formed of trap, and beyond it, is a bed of similar cherty sandstone, covered up towards the S.E. by the red conglomerate on which Manotin is built. To the south of this town, slate rocks are finely developed for several miles, forming precipitous cliffs, with the strata dipping to the S.E. They are covered in part by gravel, and are succeeded by rotten shales, assigned by Mr. Ansted to the graywacke system. These shales are visible for only a short distance, being superficially replaced by a thick covering of gravel, which extends for ten miles. At the end of that distance, hills of sandstone commence, and contain near Pilsen workable seams of coal. The sandstone is coarsely grained and not very coherent; and the coal bands, which are accompanied by shales, are of variable thickness. The dip is very small, and to the S.E., but the stratification is totally unconformable with the graywacke. Pilsen is situated on a little stream, which unites close to the town with the Beraun; and the eastern limit of the sandstone seems to be a small tongue of coarse grit, which reaches the Beraun, and exposes a bed of coal on its western bank. At that point, however, the graywacke comes in, having been brought up by a mass of trap.

Section 2. *Radnitz to Rakonitz.*—The direction of this section is nearly S. and N., Radnitz being about 12 miles east of Pilsen, and Rakonitz 20 miles east of Luditz. Radnitz stands upon an incoherent coal measure sandstone; and two bands of coal are worked a little south of the town. Beyond the sandstone rises a hill of graywacke shale, protruded, Mr. Ansted believes, by the agency of a mass of trap visible a short way off. To the north of Radnitz is an abrupt hill of the shale, considered to have been also brought up by a fault; and on its northern face commences a broad valley formed of coal measures, and bounded at its further extremity by another hill of graywacke, likewise thrown up by a fault. Coal is worked on three sides of this hill. The graywacke continues thence for six or seven miles, when the coal sandstone again constitutes the surface for a short distance (2 miles), and, after another interval occupied by graywacke, reappears forming the country around Rakonitz.

Section 3. *Zebrak to Ginetz.*—This section refers to a more southerly part of the district, and traverses a portion of the coal measures situated south of that line of graywacke which extends from Pilsen to Prague, and separates, except at one point, the coal field connected with the two first sections, from the district about to be noticed. At Zebrak, the point just mentioned, the coal measures intersect the graywacke range, in consequence apparently of a fault; and the section commences at Zebrak in graywacke shale near the junction of the coal measures with the graywacke. These shales extend to Horzowitz, where they are overlaid unconformably by the coal sandstone, which constitutes the surface of the country for about two miles. At that point is a hill, on the summit of which occurs a cherty sandstone considered by Mr. Ansted to be the base of the coal measures and to have been forced up into its present position. The beds dip about 60° S.E., and rest apparently upon a very coarse, hard, red conglomerate, to which succeeds a vast development of shale, containing occasionally Trilobites. This division of the graywacke series, is at some distance, covered again by the conglomerate upon a change of dip, and then continues nearly three miles to Ginetz, with the strata moderately inclined to the N.E. At that town a band of limestone occurs reported to be rich in Trilobites.

Section 4. *Przilep to Karlstein.*—This section is parallel to the last, and crosses the line of country between Pilsen and Prague. Two or three tolerably thick beds of coal are worked near Przilep and supply Prague with fuel. Fossils also are not deficient. About 6 miles towards the north-west, other but inferior beds of coal are wrought; but towards the east, the coal thins out between lofty precipices of shale, which form a narrow gorge. Pursuing the line of section towards the south-east, the direction of the dip, and at no great distance from Przilep, the coal basin is shut in by the steep face of a hill. At this point, Mr. Ansted believes, that the lower beds of the coal measures are not only brought up, but are bent over the upper, because, though the dip of the strata is to the S.E. or in the direction of the section, yet, on the summit of the hill above mentioned, is exposed an excellent natural surface of chert; and in a quarry near the top the inclination of the beds is about 25° S.E. or in the regular dip of the coal measures; and in a narrow valley at the bottom of a somewhat rapid descent, the lowest division of the graywacke is exposed dipping S. or actually overlying the coal measures. This inversion of superposition, Mr. Ansted explains by assuming, that the granite comes near the surface, and that by its agency the graywacke has been thrown into a trough, and its lowest beds so brought up as to be made to rest against inverted beds of the coal measures. Proceeding in the line of section, the author found in graywacke shale, portions of a Trinucleus, ***Trilobites ornatus***. (Trans. Prague National Mus. Soc. 1833.*)

The graywacke shell extends with contorted strata to an anticlinal hill of limestone, beyond which occur broken and rotten shales, then limestone, next shales again, and lastly the picturesque limestone hill of Karlstein. Further south is a valley of graywacke bounded by an altered rock, which is succeeded by granite. The Karlstein limestone is stated to be identical with that at Ginetz, (see section 3) and the two other localities in the present line of section. It is of a pale blue colour, very hard, contains several species of Orthocera and Trilobites, and is of great œconomical importance. The recurrence of the same limestone at different points, Mr. Ansted explains, by supposing, that the granite in these cases, is also near the surface, and that a displacement of it bent the yielding shales, but snapt asunder a brittle band of limestone once continuous, the portions of which, not removed by subsequent operations, are exhibited at the points mentioned in the line of section; and that the consequences of these operations have been, a disturbance in the regular succession, and an exhibition of the beds in the following order: granite, altered rocks, newest graywacke with limestone, oldest graywacke, coal measures.

In conclusion, Mr. Ansted offers the following observations as the results of his examination of this portion of Bohemia. The graywacke series is imperfectly developed, presenting at only one spot a passage upwards into the carboniferous series, and no passage downwards into the graywacke, resting always unconformably upon it; the secondary rocks are also very imperfectly developed, the mountain limestone being absolutely wanting, and the only indications of beds newer than the coal measures being a red conglomerate, into which they pass upwards. The flora of the coal measures is however well known to be rich, and to have yielded near Radnitz the fossils described by Count Sternberg. A genus allied to Scorpio is also stated to have been found in them. The fossils of the graywacke are said to be not very numerous; but the Trinucleus appears to be abundant on the line of road between Prague and Pilsen; and in a gorge near Lodentz, about fourteen miles from Prague, is a quarry which yields shells and other organic remains; and on the opposite side of the road, near the same spot, similar fossils may be obtained. Trilobites occur at Ginetz, and Orthocera at Karlstein; and both these localities and the neighbourhood of Prague are mentioned as rich in organic remains. The ***Trinucleus Caractaci*** is stated to occur near Zebrak.†

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* Impressions of shells were also found by the author in a grayish sand rock, a little nearer Prague; and the Trinucleus is found at Zebrak and Praskoles, on the south side of the high road about 10 miles south of Beraun.

† See the fossils of Caradoc sandstone, Silur. Syst. pl. 23, f. 1.

A letter was afterwards read, addressed to Dr. Buckland, P.G.S. by the Rev. John Gunn, and dated Dec. 21st, 1839.

This letter was accompanied by three paramoudras from the chalk near Norwich; and they had been selected by Mr. Gunn on account of one of them presenting a tuberculated exterior, a character which he states a paramoudra commonly assumes when it is in contact with horizontal lines of nodular flints; and the other two had been chosen because Belemnites and shells are imbedded in their substance. The letter contains some observations on the irregularities in the surface of the Norfolk chalk, and on the pipes or sand galls by which it is penetrated. With reference to these tubular hollows, Mr. Gunn refers to Mr. Lyell's description of them, read at the meeting of the British Association at Birmingham, but he calls attention to their being constantly filled in the district examined by himself, with sand, gravel, or crag, to the total exclusion of all materials belonging to the strata between the chalk and the crag; and he therefore infers, that the sand galls were not eroded during the eocene period, but that during that long period the Norfolk chalk was denudated.

The letter was also accompanied by some specimens from the boulders contained in the diluvial (drift) strata of Norfolk and Suffolk. Mr. Gunn is of opinion that these masses of rock indicate what were the strata that formed the shore against which the (so-called) diluvial waves washed; and that the masses were borne out to sea in a similar manner to the portions of cliff now annually destroyed by the waves. If the bottom of the present sea were raised, he says it would present features analogous to those of the crag and diluvial formations of Norfolk and Suffolk.

XXXIV. *Intelligence and Miscellaneous Articles.*

PHÆNOMENA OF CALEFACTION.

M. BOUTIGNY has read a paper before the Academy of Sciences on Calefaction, by which term he designates the singular phænomenon presented by water when drops of it are thrown upon a very hot metallic surface.

It has generally been supposed that this effect is produced only at a very high temperature, but M. Boutigny finds that it occurs in a lead crucible, and consequently below 612° Fahrenheit.

M. Boutigny has observed also that æther gradually dropped into a platina crucible nearly red-hot, calefies as well as water, that is to say, the mass becomes round, without the occurrence of any appearance of ebullition, is afterwards rapidly agitated, and does not seem to wet the crucible. The quantity, however, goes on diminishing, but much less rapidly than if the vessel were cold. During this slow evaporation, a very irritating vapour arises, which does not at all resemble that of æther, but which in smell greatly resembles aldehyde, and of this the author supposes it to consist; the presence of air appears to be necessary to the production of this vapour. The commissioner to whom M. Boutigny's paper was consigned, made an interesting observation: having immersed a piece of litmus paper into the crucible to try whether the vapour was acid, he observed that the part immersed retained its colour, whilst that which was even with the orifice of the crucible became evidently red. The temperature was therefore higher in this place, and it is to be presumed that slow combustion took place analogous to that which occurs in the interesting experiments of Dobereiner.

Anhydrous sulphurous acid presented M. Boutigny with phænomena still more remarkable than æther; he found that when a little of this acid dropped upon a platina capsule heated almost to redness, the drops were strongly agitated, became round, immoveable and opalescent, and seemed even to crystallize. The small spheroid when placed on the hand produced a sensation of cold.

M. Boutigny was of opinion, that in this case the sulphurous acid suffered so great a diminution of temperature that it solidified. The commissioner rejects this explanation, and is satisfied with admitting, that the acid under these circumstances evaporating more slowly than in the open air, produces nevertheless, by this slow evaporation, so considerable a degree of cold as to congeal the moisture of the surrounding air, and to become hydrated. This explanation is apparently confirmed by the fact, that if the small solid globule be rapidly projected into a tube, and it be immediately corked, the globule disappears, but leaving in the place which it occupied a dew, that remains even when the tube is uncorked.

M. Boutigny is of opinion that the phænomena described may be connected with the explosions in steam-boilers, and he is still occupied with the subject, and has made a great number of experiments with different liquids, and particularly with alcohol of different degrees of strength, with æther, oil of turpentine and lemons, and with alkaline and acid solutions.—*Journal de Pharm.*, Mai, 1840.

HYDROMELLONIC ACID.

Discovered by L. Gmelin. Prepared by dissolving mellonuret of potassium in boiling water, and adding to the solution hydrochloric, sulphuric, or nitric acid. A dirty white gelatinous precipitate which dries to a yellow powder, the hydrated hydromellonic acid. It is slightly soluble in cold, more freely in hot water, has a slightly acid reaction, and is not decomposed by hydrochloric and nitric acids.

Formula $C_6N_4 + H$; eq. $= 94{\cdot}32$. *Turner's Elements of Chemistry*, p. 796.

CHROMIC ACID.

M. J. Fritzche prepares chromic acid by the careful addition of concentrated sulphuric acid to a hot concentrated solution of bichromate of potash; a crimson bulky precipitate is obtained, which is dried first by heat, and afterwards *in vacuo*. This is entirely chromic acid, which is to be washed to get rid of the mother water, and of the sulphuric acid which adheres to it. The author could not obtain the compound of sulphuric and chromic acids described by M. Gay-Lussac in the 16th volume of the *Ann. de Chim. et de Phys.*, and he is inclined to question its existence.—*L'Institut*, No. 341.

COMPOSITION OF CRYSTALLIZED PHOSPHORIC ACID.

In the opinion of Prof. Graham, phosphoric acid may combine with water in three different proportions, and form a phosphate, biphosphate, triphosphate of water. The existence of these compounds has hitherto been hypothetical, except the first, which appears to be vitrified phosphoric acid. The analysis of crystallized phosphoric acid has not yet been attempted, or at any rate not yet published. M. Peligot has attempted to supply this void in the history of phosphoric acid, by analysing crystals which slowly formed spontaneously in bottles filled with syrupy phosphoric acid. One of these bottles contained two very distinct crystalline layers. The upper crystals were transparent and hard; the lower ones were soft, and had the appearance of sugar of honey.

The crystals, separately collected, were dried *in vacuo*, on plates of absorbent porcelain; the quantity of water was determined by calcining them with oxide of lead. According to M. Peligot's analyses, the upper crystals contained from 27 to 28 per cent. of water, and the lower crystals 22 to 23 per cent.

The extreme avidity of these crystals for water, renders a precise analysis of them very difficult; if, as theoretically supposed by M. Peligot, hydrated phosphoric acid containing three equivalents of water ought to contain 27·4 per cent., and the bihydrate 20·1 per cent., it will appear probable that the crystals examined were in fact these two hydrates. Their properties also corroborated this opinion; for the most hydrated acid, when saturated with ammonia, precipitated silver of a yellow colour, while the second precipitated it white. —*Journal de Pharm.*, Juin, 1840.

DETECTION OF ALCOHOL IN ESSENTIAL OILS.

For the above purpose M. Borsarelli employs a small cylindrical tube closed at one end; this is two-thirds filled with the oil, and there are dropped into it small pieces of chloride of calcium, which are quite dry, and free from powder; the tube is then closed, and heated in a water-bath to 212° for four or five minutes, taking care to agitate it occasionally, and to allow it to cool slowly.

If the essential oil contains a notable proportion of alcohol, the chloride dissolves entirely, and forms a liquid stratum, which occupies the lower part of the tube, while the essential collects in the upper. If the oil contains only a very small portion of alcohol, the fragments of chloride of calcium effloresce, lose their form, and unite at the bottom of the tube into a white adhesive mass; when it is quite pure the pieces of chloride suffer no change, even in their form.

It is proper to observe, that in trying an essential oil it is right to employ but a very small portion at first, and to add successive portions gradually; otherwise, if the proportion of alcohol be very small, it may be absorbed by the chloride without sensibly altering it, and even without showing its presence. It is easy when the operation is over to determine the proportions of a mixture of alcohol and essential oil, by comparing its weight or volume with that of the pure oil which floats upon the alcoholic solution of the chloride.

The same process, the author states, may be employed for determining the quantity of alcohol which æther contains; but the tube should be longer, and not too perfectly closed.—*Journal de Pharm.*, Juin, 1840.

ŒCONOMICAL PREPARATION OF ACETONE.

M. Zeise of Copenhagen observes, that when a considerable quantity of acetone is required, the methods usually employed are too expensive, and he recommends the following process: mix thoroughly one part of well-powdered quicklime with two parts of powdered crystals of acetate of lead. Soon after the mixture is made, the lime usually begins to heat violently; but as no smell of acetone is perceptible, there is no sensible loss of it; it is better to put the mixture into the distillatory apparatus before this heating occurs, because afterwards the mass is so great that the introduction is more difficult. M. Zeise states that he has not found it advantageous to attempt getting rid of this circumstance by employing slacked lime; for in this case dried acetate must be used to prevent the product from containing too much water, and the drying of the acetate is more troublesome than powdering the lime.

The iron bottles in which mercury is imported, suit for this operation extremely well; four pounds of the acetate may be operated upon at once; the bottle is placed almost horizontally in the furnace, but so that the opening is rather raised; to this a slightly curved short tube is adapted, and luted with a mixture of clay, chalk and salt, and this enters into a glass tube sufficiently large, properly curved and surrounded with a tin-plate pipe, in which an ascending current of water is kept up; and lastly, to this a receiver surrounded with ice is attached. The heat is slowly raised, and it is only towards the end of the operation that it is increased to redness.

The product is a mixture of acetone, a small quantity of water, two oily substances, which are less volatile than acetone, one of which is probably the dumasine of Kane.

From this product pure acetone is obtained by dissolving chloride of calcium in it, distilling the solution in a water-bath, until at 212° nothing more passes over; the product is to be again treated with chloride of calcium, and three-fourths being distilled will be found to be pure acetone.

On adding water to the residue, the oily bodies are separated from the acetone, which dissolves in the water and is separated from it by chloride of calcium.

From about 8 pounds of crystallized acetate of lead, M. Zeise obtained by the process now described from 10 to 11½ ounces of perfectly pure acetone.—*Ann. de Chim. et de Phys.*, t. lxxii.

CARBURET OF PLATINA.

M. Zeise observes that the formation of a compound of carbon and platina has been hitherto ineffectually attempted; he has, however, succeeded in procuring by means of decomposing acechlor-platina by heat.

When acechlor-platina is heated in a retort, to which a receiver is adapted, with a bent tube, it begins to decompose and yield a little gas at about 419°; at 464° the disengagement of gas is plentiful, and a brown liquid distils. This continues with the gradual increase of the heat up to 527°, a little colourless liquid coming over from time to time; at 572° much gas and liquid were produced; when this heat ceased to produce any effect, the retort was heated to redness in the sand-bath; this occasioned a copious disengagement of gas; so that the quantity obtained at this period was greater than all previously procured, but the quantity of liquid was much smaller; when neither gas nor liquid was obtained, the operation was stopped, and the residue cooled out of contact with the air.

The distilled liquid contained so much hydrochloric acid that it smoked in the air, and when mixed with water it gave an oily liquid which floated. Its odour was resinous and æthereal: the volume of the liquid was considerably diminished by treatment with water.

The gas was a mixture of hydrochloric acid and an inflammable gas, which was probably protocarburet of hydrogen, and there were traces of carbonic acid. The residue was black, slightly coherent, and free from metallic particles. The slowness with which it burnt, indicated that it was a chemical compound of carbon and platina.

In the first experiment 100 parts of acechlor-platina yielded 60·0107 of carburet of platina, and in the second 60·708; the mean is 60·362.

On the supposition that it is a bicarburet of platina, 100 parts of acechlor-platina ought to have yielded 60·347 parts of carburet. Then as 100 parts acechlor-platina, and consequently 60·362 of carburet of platina, contain 53·692 parts of platina, we have for 100 parts of carburet of platina,

Carbon	11·041
Platina..............	88·959
	100

Calculation gives 11·029 carbon, and 88·971 platina; it retained scarcely a trace of chlorine. Carburet of platina may also be obtained by heating to redness a mixture of acechlor-platina and hydrate of lime.—*Ann. de Chim. et de Phys.*, t. lxxii.

SULPHATE OF CARBYLE.

M. E. Magnus states, that when anhydrous sulphuric acid is absorbed by absolute alcohol, there are formed, under the influence of particular circumstances, white silky crystals in the alcohol; they are a sulphate of carburetted hydrogen, to which M. Magnus gives the name of sulphate of carbyle, from *carbo* and *hydrogenium.* The method adopted was that of exposing the alcohol in a small tube to anhydrous sulphuric acid in a stopped receiver. Crystals are rarely formed immediately; the tube must be put into a second receiver containing sulphuric acid, and sometimes even into a third. For the sulphuric acid in the receiver also absorbs alcohol, which causes the cessation of the absorption of the acid by the alcohol which is contained in the tube. The formation goes on without the disengagement of sulphurous acid.

The crystals which form in the alcohol tube are, it is true, surrounded with fuming acid, but M. Magnus succeeded in isolating them. In pouring off the liquid acid which filled the interstices, they yield abundant vapours: they rapidly attract humidity from the air and deliquesce; to dry them they were placed upon a plate of baked clay, slightly heated in the air-pump over sulphuric acid until they yielded no more vapour.

When æther was employed instead of alcohol, no crystals were obtained; it is possible, however, that under some circumstances they might be formed; the experiment confirms the previous statement of M. Magnus, that when æther is absorbed by anhydrous sulphuric acid, heavy oil of wine is always produced, which is never the case with absolute alcohol. This may be regarded as one of the strongest objections against the opinion that alcohol is a hydrate of æther; for if this were the case, the sulphuric acid ought to remove the water, and then act upon it as if it were æther, which evidently does not occur.

When these crystals are cautiously heated, they fuse without decomposition, and on cooling become a crystalline mass. They combine with water and alcohol with the disengagement of heat, and by evaporation they do not separate from the solution. The aqueous solution, saturated with barytes, yields soluble salts with a little sulphate of barytes. The quantity of sulphuric acid obtained was variable, and appeared to be eliminated by the action of the water.

The soluble barytic salts are ethionate of barytes and a little isethionate, and it appears to be the conversion of ethionic into isethionic acid, which produces the sulphuric acid.

By analysis sulphate of carbyle appears to be composed of

Sulphuric acid......	84·930
Carbon	12·955
Hydrogen	2·115
	100

It is therefore a sulphate of carburetted hydrogen, or 1 equivalent of sulphuric acid 40+1 equivalent of carburetted hydrogen 7, similar to that formed by the action of anhydrous sulphuric acid on olefiant gas.—*Ann. de Chim. et de Phys.*, t. lxxii.

ON HELLENIN. BY M. C. GERHARDT.

This substance, obtained from *Inula hellenium*, is to be distinguished from *inulin* yielded by the same root. It is readily obtained by treating the fresh root of the elecampane with alcohol of sp. gr. 0·837; when the excess of alcohol is separated by distillation, the concentrated liquor becomes milky by cooling, and deposits abundance of crystals. They are purified by redissolving in alcohol and recrystallizing. When the root is distilled with water, downy flocculi, which are very white pure hellenin, are obtained in the receiver; but the quantity is so small that it is better to employ alcohol.

Hellenin crystallizes in four-sided prisms; they are perfectly white, their smell and taste is extremely weak, and they are lighter than water. They are insoluble in water, but very soluble in æther and alcohol, and these solutions are precipitated by water. It dissolves also in all proportions, in essential oils and in creasote. It may readily be pulverized when it is rendered impure by the resin, which always exists with it in the root. Its fusing point is about 161° Fahr.; it boils at 527° to 536°, and volatilizes before it boils, exhaling a very weak odour. At this temperature, however, it is more or less altered, so that the density of its vapour cannot be ascertained.

When hellenin is fused at a gentle heat, it recrystallizes in a mass on cooling; but if the heat be continued for some minutes, the mass on solidifying does not possess a crystalline texture, but resembles resin in appearance. The caustic alkalies do not decompose hellenin, even when heated, a property which it possesses in common with several substances, such as camphor, the oil of aniseed, mint, &c. On heating it in a solution of potash, it first fuses and eventually dissolves, and on the addition of hydrochloric acid the hellenin is precipitated without alteration. When heated with hydrate of potash, a great part of it is volatilized, whilst another portion is carbonized; on dissolving the mixture afterwards in water a slightly brown coloured liquid is obtained, which becomes slightly turbid on the addition of acids.

Acids act upon hellenin as they do upon the greater part of the essential oils; concentrated sulphuric acid dissolves it at common temperatures, forming a red coloured solution, without the evolution of any sulphurous acid, provided no heat be employed; nevertheless after a considerable time the mixture becomes black, as if acted upon by heat. The solution then contains a certain quantity of a peculiar acid, to which M. Gerhardt has given the name of sulpho-hellenic acid.

When hydrochloric acid gas is brought into contact with hellenin, it absorbs a large quantity of the gas, and a liquid of a violet colour is formed; when exposed to the air it exhales hydrochloric acid.

Nitric acid of middling strength dissolves hellenin without the evolution of any hyponitrous acid, and water precipitates it unaltered; when the mixture is heated the hellenin is converted into what M. Gerhardt calls *nitro-hellenin.*

Concentrated acetic acid dissolves hellenin; the solution is colourless, and deposits by evaporation unaltered crystals of this substance; water precipitates the solution.

Anhydrous phosphoric acid acts upon hellenin as it does upon camphor, converting it into a carburetted hydrogen, which M. Gerhardt has named *hellenene.*

Cold chlorine gas does not act upon hellenin, nor even when exposed to the direct rays of the sun; but when the mixture is heated, hydrochloric acid is disengaged and a resinous body is formed, in which a certain number of atoms of hydrogen are replaced by an equal number of atoms of chlorine; a drop of bromine produces with hellenin an effervescence of hydrobromic acid; the product is of a yellow red, dissolves in alcohol, and is precipitated from it by water. It is probably a compound analogous to that formed with chlorine, and the author terms it *hydrochlorate of chlorehellenin.*

Bichloride of tin and protochloride of antimony, the latter in a state of fusion, colour hellenin of a deep red colour, exactly like concentrated sulphuric acid.

When distilled with lime, hellenin yields a yellow inflammable liquid; it is neutral, does not mix with water, and in smell resembles acetone.

The analysis of hellenin performed by M. Gerhardt agrees nearly with that of M. Dumas, and he considers it as constituted of

30 atoms of carbon....	1146·6	77·92
20 atoms of hydrogen..	124·8	8·41
2 atoms of oxygen ..	200·0	13·67
	1471·4	100

In composition it more nearly approaches creasote than any other substance, which, according to Ettling, contains

Carbon..	77·42
Hydrogen	8·12
Oxygen........	14·46—100

Ann. de Chim. et de Phys., t. lxxii.

CHEMICAL AND CONTACT THEORIES OF VOLTAIC ELECTRICITY.

After giving an extract from Professor Faraday's Sixteenth Series of Researches, of which an abstract appeared in Lond. and Edinb. Phil. Mag., vol. xii. p. 122, M. de la Rive remarks, "The question of which Mr. Faraday treats in the memoir from which the foregoing extract is given, is the subject of a warm controversy at the present time, especially in Germany, where the partizans of the voltaic theory of contact are numerous. The authority of Mr. Faraday, and the clear and decisive manner in which he declares himself in favour of the chemical theory, are of great weight; the numerous investigations which this philosopher has made in electricity, and his great practical acquaintance with voltaic apparatus, give great value to his opinions on the origin of voltaic electricity.

"In beginning the memoir here alluded to, he has done me the honour to mention that I am one of those who have most strongly pleaded for fifteen years in favour of the chemical theory; he has repeated most of the experiments upon which I have rested my opinion, and he has found them correct. I hope soon to be able, in like manner, to take up the experiments lately made in Germany in favour of the theory of contact, in order to show that they are in no way contrary to the chemical theory, and to add to this theory some new direct proofs. At the meeting of the *Société de Phys. et d'Hist. Nat. de Genève*, on the 21st of May last, I read a memoir on this subject, which will appear in one of our earliest Numbers."—A. DE LA RIVE, *Bibliothèque Universelle*.

HYDROMELLONIC ACID AND METALLIC OXIDES.

The hydromellonic acid is decomposed by metallic oxides into a metallic mellonuret and water; it decomposes the carbonates both in the dry state and in solution, and the iodides and bromides on fusion. Its compounds with the alkaline metallic oxides and with the earths are insoluble in water.

Mellonuret of Potassium.—Prepared by fusing sulphocyanuret of potassium in a porcelain crucible at a red heat, and adding mellon as long as an evolution of sulphuret of carbon and sulphur is remarked. A brown opake glassy mass is obtained, which dissolved in boiling water yields, as the solution cools, hydrated crystals of mellonuret of potassium. It may also be formed by fusing 5 parts of chloride of antimony (butter of antimony) with 8 parts of sulphocyanuret of potassium, and removing by boiling water the soluble portions of the residue after the escape of the sulphur and the sulphuret of carbon. It is also formed as a secondary product in the process for the preparation of the sulphocyanuret of potassium; it is present in the solution in small, but in the residue in larger quantity, from which it may be removed by boiling water.

Prop.—Crystallizes from water in colourless fine needles, which unite into dense flakes; a concentrated solution congeals to a soft white mass, which is with difficulty dissolved by cold water; the crystals contain water of crystallization, which they lose at a high temperature; they then fuse without loss of weight to a clear yellow glass. The solution is tasteless, and precipitates all earthy and metallic salts.

By fusing sulphocyanuret of potassium with mellon, the sulphocyanogen is liberated, and is instantly decomposed by the high temperature into sulphuret of carbon, sulphur, and mellon. By fusing 1 eq. chloride of antimony with 4 eq. sulphocyanuret of potassium, there are formed 3 eq. chloride of potassium 3 KCl, 1 eq. sulphuret of antimony Sb_2S_3, 2 eq. sulphuret of carbon $2\,CS_2$, 1 eq. mellonuret of potassium KC_6N_4, and 1 eq. of free sulphur. By fusing ferrocyanuret of potassium with sulphur, the sulphocyanuret of potassium, and sulphocyanuret of iron, are formed; 4 eq. of the latter decompose into 4 eq. sulphuret of iron 4 Fe S, 2 eq. sulphuret of carbon $2\,CS_2$, and 1 eq. of mellon, which as soon as it is formed decomposes 1 eq. of sulphocyanuret of potassium into mellonuret of potassium and sulphocyanogen; the latter is further decomposed into sulphur, sulphuret of carbon, and mellon.

Formula $K + C_6N_4$; eq. = 132·47. *Turner's Elements of Chemistry*, pp. 797, 798.

CYANILIC ACID.

By a long-continued boiling of mellon in dilute nitric acid, a solution is effected with the evolution of gaseous products, and the liquid yields on evaporation colourless, transparent, octohedral crystals; by re-solution in hot water, hydrated cyanilic acid in soft tabular crystals of a mother-of-pearl lustre are obtained. This acid has the same composition as the crystalline cyanuric acid; contains, like the latter, 4 eq. water of crystallization, which it loses at 212°, when it becomes opake and falls to a white powder. By the destructive distillation it is converted into hydrated cyanic acid; by solution in sulphuric acid and caustic potassa into cyanuric acid. This acid has been but little examined. Its formation admits of explanation on the supposition that the elements of 1 eq. of mellon and 3 eq. of water give rise to 1 eq. cyanilic acid and 1 eq. ammonia, and the latter is in point of fact found combined with the nitric acid; according to this, its formation ought not to be dependent on the use of nitric acid alone.—*Ibid.* p. 798.

METEOROLOGICAL OBSERVATIONS FOR JULY, 1840.

Chiswick.—July 1. Overcast: boisterous. 2. Rain, with strong wind. 3. Cloudy and fine. 4. Very fine. 5. Cloudy: windy. 6, 7. Fine. 8. Fine: heavy rain. 9—12. Very fine. 13—17. Fine. 18. Overcast. 19. Cloudy: rain. 20. Heavy showers. 21. Very fine: rain. 22. Fine. 23. Cloudy. 24. Overcast and fine: rain. 25. Showery. 26. Cloudy: fine. 27. Fine. 28. Hazy. 29. Very fine. 30. Cloudy: rain. 31. Very fine.

Boston.—July 1, 2. Rain. 3. Stormy. 4. Fine: rain early A.M.: rain A.M. 5. Fine: rain A.M. 6. Cloudy: rain P.M. 7. Cloudy: rain early A.M.: rain P.M. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain P.M. 11—13. Cloudy: rain A.M. and P.M. 14, 15. Fine. 16. Rain: rain early A.M. 17. Fine. 18, 19. Cloudy: rain P.M. 20. Fine. 21. Fine: rain P.M. 22. Fine. 23, 24. Cloudy. 25. Rain: thunder and lightning with rain P.M. 26. Cloudy. 27. Fine. 28. Cloudy: rain A.M. 29. Fine. 30. Cloudy. 31. Fine.

Applegarth Manse, Dumfries-shire.—July 1. Heavy rain A.M.: cleared up P.M. 2. Drizzling all day. 3. Heavy rain all day. 4. Fair till 4 P.M. then wet. 5. Showery: fair evening. 6. Rainy. 7, 8. Showery: thunder. 9. Fair all day. 10. Showery. 11. Warm: a single shower: thunder. 12. Very wet. 13. Fine dry day. 14. Wet afternoon. 15. Very wet all day. 16, 17. Occasional showers. 18. Fair till afternoon, then wet. 19. Rain early A.M.: cleared up. 20. Fair all day. 21. Heavy showers all day: thunder. 22. Fair all day. 23. Fair till evening, then rain. 24. Showery all day. 25. Showery afternoon. 26—30. Fair all day. 31. The same: a few drops P.M.

Sun shone out 29 days. Rain fell 22 days. Thunder 3 days.

Wind north ½ day. North-north-east ½ day. East-north-east 1 day. East 1 day. South-east ½ day. South 4 days. South-west 8 days. West-south-west 3 days. West 7 days. North-west 2½ days. North-north-west 3 days.

Calm 11 days. Moderate 12 days. Brisk 4 days. Strong breeze 2 days. Boisterous 1 day. Variable 1 day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. July.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·922	29·985	29·809	29·30	29·40	29·60	62·0	68·2	52·4	68	58	56	61	50	s.	sw.	nw.	nbyw.	·133	·01	...	...	58
2.	29·774	29·739	29·544	29·23	29·54	29·31	61·2	66·6	59·6	66	55	54	59½	46½	se.	sw.	calm	sw.	...	·02	·30	...	58
3.	29·540	29·708	29·483	28·90	29·12	29·35	58·8	66·5	56·5	68	51	62	56	50½	w.	w.	w.	w.	·033	...	·16	...	58
4.	29·878	29·810	29·676	29·26	29·55	29·44	61·3	69·2	53·7	72	56	58	59½	47	w.	w.	w.	nw.	·080	...	·10	...	54
5.	29·732	29·710	29·681	29·13	29·34	29·45	63·3	68·6	58·7	69	50	63	60	49	w.	sw.	w.	w.	...	...	·02	...	56
☽ 6.	29·810	29·767	29·434	29·20	29·42	29·19	62·6	75·3	53·3	67	49	62	59	48½	s var.	sw.	w.	wsw.	·016	·14	·02	1·87	55
7.	29·634	29·718	29·536	29·00	29·33	29·48	59·7	71·7	50·6	71	51	53	61	48	w.	w.	w.	w.	·138	...	·83	...	55
8.	29·822	29·764	29·625	29·26	29·49	29·50	61·2	66·3	53·8	66	48	60	59	50	sw.	sw.	w.	w.	·025	·46	·06	...	55
9.	29·878	29·928	29·798	29·19	29·64	29·73	59·4	67·3	51·3	68	48	60·5	59	47	nw.	nw.	nw.	w.	·500	...	·05	...	53
10.	29·996	29·946	29·893	29·30	29·74	29·79	60·3	67·7	51·3	67	50	60	63	50	w.	nw.	w.	nnw.	...	...	...	...	53
11.	30·002	29·936	29·924	29·42	29·81	29·82	60·2	66·8	52·6	68	46	56	61½	49	nw.	nw.	w.	n.	...	...	·04	...	54
12.	30·012	29·949	29·905	29·43	29·84	29·85	58·3	69·2	52·2	65	42	55	57½	48	nw.	w.	calm	ene.	...	...	·41	..	53
13.	30·012	30·033	29·950	29·46	29·95	30·04	55·7	62·7	49·4	62	41	56	59½	43	nw.	ne.	n.	w.	...	...	·07	..	53
○ 14.	30·316	30·269	29·942	29·70	30·09	30·04	59·4	70·5	50·2	74	55	59	59½	40½	sw.	w.	calm	sw.	...	...	·05	1·80	50
15.	30·304	30·235	30·137	29·62	29·93	29·87	63·7	77·2	57·0	80	48	65·5	59	51½	s.	sw.	w.	s.	...	...	...	...	54
16.	30·152	30·101	29·919	29·50	29·83	29·71	66·5	74·3	56·0	79	46	58	61	51	s.	s.	calm	wsw.	...	...	·10	...	56
17.	29·998	29·909	29·889	29·36	29·61	29·52	62·7	74·6	52·8	75	56	62	58½	49½	sw var.	w.	w.	wsw.	...	...	·03	...	55
18.	29·778	29·735	29·717	29·18	29·56	29·45	63·7	69·7	58·0	70	56	64	61½	45	w.	w.	calm	s.	...	...	...	...	60
19.	29·700	29·662	29·470	29·05	29·34	29·30	63·7	68·6	58·7	69	49	61	63	53	s.	s.	sw.	s.	...	·06	·04	...	60
20.	29·566	29·511	29·441	28·94	29·24	29·30	63·3	70·0	53·2	69	51	63·5	63½	53	s var.	w.	w.	sw.	·091	·09	·10	...	58
21.	29·630	29·646	29·563	29·00	29·35	29·50	63·7	76·0	55·3	74	47	63·5	64	50½	w var.	sw.	w.	s.	·066	·54	...	...	59
☾ 22.	29·820	29·844	29·757	29·22	29·62	29·70	61·4	70·2	52·7	69	49	60	62	50½	w.	sw.	nw.	wbyn.	·305	·01	·04	1·41	56
23.	30·032	30·013	29·981	29·43	29·78	29·82	59·7	72·4	53·5	65	51	56	59½	50	w.	w.	w.	w.	...	...	...	...	52
24.	30·066	29·993	29·906	29·50	29·74	29·67	61·7	64·5	55·0	66	56	58	58	49	s.	sw.	w.	sw.	...	·02	...	...	54
25.	29·814	29·774	29·646	29·24	29·58	29·59	60·3	67·4	58·5	67	54	58	64	51	s.	sw.	sw.	sw.	·016	·22	·07	...	58
26.	29·694	29·739	29·627	29·16	29·70	29·78	58·8	70·2	56·3	66	54	61	66	53	se.	ne.	calm	e.	·291	·05	·24	...	56
27.	29·956	29·926	29·745	29·37	29·79	29·75	61·0	62·4	55·3	70	49	64	64	50½	nw.	sw.	calm	sw.	·105	...	...	...	58
● 28.	30·084	30·078	30·019	29·47	29·84	30·00	61·7	71·4	55·6	74	58	63	65	55	s.	w.	calm	w.	...	·01	...	...	57
29.	30·260	30·171	30·134	29·63	30·08	30·04	64·7	71·8	60·2	75	54	65	66	45½	nw.	nw.	calm	sw.	...	...	·03	...	57
30.	30·148	30·095	30·029	29·47	29·90	29·93	65·4	72·0	60·2	69	51	62·5	65	58½	ssw.	sw.	calm	sw.	...	·05	...	0·20	61
31.	34·210	30·159	30·126	29·59	30·01	30·06	62·3	73·2	58·7	70	45	60	61	49	nw.	nw.	nw.	w.	·016	...	...	...	60
Mean.	29·921	29·898	29·780	29·30	29·650	29·644	61·5	69·8	54·9	69·35	50·77	59·9	61·1	49·4					Sum. 1·815	1·68	2·76	5·28	Mean. 56·

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XXXV. *Comparative Measure of the Action of two Voltaic Pairs, the one Copper-zinc, the other Platina-zinc.* By M. JACOBI*.

I HAVE the honour to communicate to the Imperial Academy of Sciences, the result of my comparative experiments, concerning the force of two different voltaic pairs with partitions, the one copper-zinc, charged with sulphate of copper and sulphuric acid dilute with six parts by volume of water; the other platina-zinc, and charged, according to the recommendation of Mr. Grove, with concentrated nitric acid, and with the same diluted sulphuric acid. The first pair, copper-zinc, had 36 square inches of surface, the pair platina-zinc had only $2\frac{1}{2}$ square inches. To measure the force of the current, I employed M. Becquerel's electro-magnetic balance. This instrument is valuable for accurate measures, provided the helices be so disposed as to be able to fulfil the conditions of stable equilibrium, which necessarily ought to exist in a balance. This is attained by making the repulsion only act between the magnetic bars and helices. For this purpose, one of the helices ought to be fixed below, the other above the said bars. This last helix is traversed by the rod, by which the bar is suspended from the beam of the balance. It is still necessary that a correction be adapted to the currents measured by the balance. This correction, of which other synchronous investigations have shown the necessity, is in proportion to the square of the force of the current. For let k' be the actual current, k the current measured by the balance, we have the equation

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* Read before the Imperial Academy of Sciences of St. Petersburgh, on the 31st of January, 1840.

$$k = k' - y\,k'^2,$$

from which is deduced $k' = \frac{1}{2y}(1 - \sqrt{1 - 4ky})$. For my balance I have found, by a series of observations, $y = 0{\cdot}00004228$ (*Bulletin Sc. de l'Acad. Sc.* tom. v. p. 375.).

The following table contains the experiments made with the voltaic combinations in question. The first column contains the resistances, L, of the helices which serve as conjunctive wire, and which had been found by other experiments; the two other columns contain the forces of the effective currents, or of the currents measured in grammes, and corrected according to the formula above indicated.

L.	Force of the pair, Copper-zinc.	Force of the pair, Platina-zinc.
	gr.	gr.
23·1	0·380	0·395
135·3	0·097	0·135

Let A, A′ be the electro-motive forces, λ, λ′ the resistances of the pair itself, we shall have, according to Ohm's formula, the four following equations:

$$\frac{A}{\lambda + 23{\cdot}1} = 380 \qquad \frac{A'}{\lambda' + 23{\cdot}1} = 395$$

$$\frac{A}{\lambda + 135{\cdot}3} = 97 \quad . \quad \frac{A'}{\lambda' + 135{\cdot}3} = 135,$$

whence $A = 14610$, $\lambda = 15{\cdot}35$, $A' = 23000$, $\lambda' = 35$; or taking λ as the unit of surface, which is here 36 square inches, $\lambda' = \frac{35 \times 2{\cdot}5}{36} = 2{\cdot}4$. Let s be the total surface of a pile, z the number of pairs, C the force of the current, L any resistance, we have $C = \frac{z\,A\,s}{z^2\lambda + L\,s}$. From this equation we find, that the maximum of force is obtained, if the pile be arranged so that $\frac{z^2\lambda}{s} = L$, i. e. that the total resistance of the pile shall be equal to the resistance of the conductor, whatever its nature may be, this resistance being one which is interposed in the circuit, and not belonging to the pile. As for other arrangements than those which correspond to the maximum of effect, there is no constant relation between different voltaic combinations; we can only compare them, and judge of their relative preferableness by referring them to this maximum of action. We have, by eliminating z, the equations

$$C\,(max) = \frac{A\sqrt{s}}{2\sqrt{\lambda L}} = \frac{A'\sqrt{s'}}{2\sqrt{\lambda' L}},$$

whence we deduce, by substituting the numerical values above found for A, A′, λ, λ′ ... ... $s' = s\,.\,0{\cdot}06$
and with reference to the number of pairs $z' = z\,.\,0{\cdot}6$;
that is to say, *it requires only a pile of* 6 *square feet of platina to replace a pile of* 100 *square feet of copper*; or with reference to the number of pairs, 6 *pairs of platina, each of a square foot of surface, will produce the same effect as* 10 *pairs of copper, each of which presents a surface of* 10 *square feet.* This eminent superiority of platina, as in Mr. Grove's combination, is verified by many experiments on a large scale.

XXXVI. *On the Application of* Huyghens's *Principle in Physical Optics. By* R. POTTER, *Esq., B.A.*[*]

IN the present paper I propose to examine some of the consequences of the method at present followed in developing the results of the undulatory theory of light, which consists in considering elementary waves, having their origin in some previous positions of the main waves, as the cause of these latter in succeeding positions. In the *Mémoires de l'Acad.* for 1821 and 1822, Fresnel announces this method in the following terms:—

"Application du Principe d'Huygens aux Phénomènes de la Diffraction.

"Ce principe que me paraît une conséquence rigoureuse de l'hypothèse fondamentale, peut s'énoncer ainsi: *Les vibrations d'une onde lumineuse dans chacun de ses points peuvent être regardées comme la somme des mouvemens élémentaires qu'y enverraient au même instant, en agissant isolément, toutes les parties de cette onde considérée dans une quelconque de ses positions antérieures.*"

Again, in speaking of the analytical process, he says, "La recherche de la loi suivant laquelle leur intensité varierait autour de chaque centre d'ébranlement, présenterait sans doute de grandes difficultés; mais heureusement nous n'avons pas besoin de la connaître; car il est aisé de voir que les effets produits par ces rayons se détruisent presque complètement dès qu'ils s'inclinent sensiblement sur la normale, en sorte que ceux qui influent d'une manière appréciable sur la quantité de lumière que reçoit chaque point P peuvent être regardés comme d'égale intensité."

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[*] Communicated by the Author.

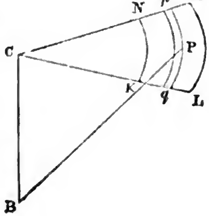
In the application of the above principle, we take the origins of the elementary waves on any proposed surface as that of a reflecting or refracting substance, or an aperture, without its being necessary that this surface should coincide with any one wave surface as it arrives.

There are only a few cases in which the integration for the whole vibration of a particle can be effected directly; in the following, however, the integration involves no difficulty, and they suffice for proving the discordance of the results of the principle with acknowledged facts. They show that the labour which has been expended in investigating more complicated cases, might with ordinary caution have been saved.

The integration is readily performed when a series of plane waves fall on a plane reflector, or an aperture parallel to their surfaces, the reflector or aperture being of one of the circular forms enumerated below, and the particle whose vibration is required, being in the normal to such surfaces through the centre of the circular arcs. These forms comprise a circular aperture of any radius, an annulus contained between two circles, a circular sector, and the quadrilateral figure bounded by two radii, and two circular arcs: this latter form approximates to a rectilinear parallelogram, when the angle between the bounding radii is small, and the radii of the circular arcs are large.

The integration for apertures or reflectors of the same forms, is readily performed also when light diverges from a luminous point in the normal line through the centre of the arcs; and the particle whose vibration is required is equally distant from the surface in the same line, and on the opposite or same side with the luminous point, according as an aperture or a reflector is considered.

Let us commence with a series of plane waves falling directly on a quadrilateral aperture K L M N, bounded by the two concentric circular arcs K N, L M, and the radii C M, C L containing the angle M C L $= \theta$.



Let B be the position of the particle whose vibration is required situated anywhere in the line B C which is perpendicular to the plane of the aperture, and let C B = h.

Let p P q be any element of the aperture, whose breadth $= \delta r$, and distance C p or C q from C = r, therefore its distance from B = $\sqrt{r^2 + h^2}$ and its area $= \theta r \delta r$.

Then, by the principle under discussion, we have the displacement of the particle at B caused by this element, proportional to

$$\frac{\text{area of element}}{\text{distance B P}} \sin \left\{ \frac{2\pi}{\lambda} (vt - \text{B P}) \right\}$$

$$= \frac{a \theta r \delta r}{\sqrt{r^2 + h^2}} \sin \left\{ \frac{2\pi}{\lambda} \left(vt - \sqrt{r^2 + h^2}\right) \right\},$$

where a is some number.

Integrating for the whole vibration we have

$$a\theta \int_r \frac{r}{\sqrt{r^2 - h^2}} \sin \left\{ \frac{2\pi}{\lambda} \left(vt - \sqrt{r^2 + h^2}\right) \right\}$$

$$= \frac{a\theta\lambda}{2\pi} \cos \left\{ \frac{2\pi}{\lambda} \left(rt - \sqrt{r^2 + h^2}\right) \right\} + \text{C between the limits } \left.\begin{matrix} r = r_1 \\ r = r_2 \end{matrix}\right\}$$

$$= \frac{a\theta\lambda}{\pi} \sin \left\{ \frac{\pi}{\lambda} \left(\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}\right) \right\}.$$

$$\sin \left\{ \frac{2\pi}{\lambda} \left(vt - \tfrac{1}{2}\sqrt{r_2^2 + h^2} - \tfrac{1}{2}\sqrt{r_1^2 + h^2}\right) \right\},$$

which gives the intensity of the light at B

$$= \frac{a^2 \theta^2 \lambda^2}{\pi^2} \sin^2 \left\{ \frac{\pi}{\lambda} \left(\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2}\right) \right\}.$$

The intensity becomes a maximum and $= \frac{a^2 \theta^2 \lambda^2}{\pi^2}$, or equal to $4\, a^2 \lambda^2$ when $\theta = 2\pi$.

If $$\sqrt{r_2^2 + h^2} - \sqrt{r_1^2 + h^2} = \frac{2n + 1}{2} \lambda,$$

where n is any integer. This equation may be satisfied by an indefinite number of values of r_1 and r_2.

When r_1 and r_2 are very great, and h small, we have

$$r_2 - r_1 + \frac{h^2}{2}\left(\frac{1}{r_2} - \frac{1}{r_1}\right) + \text{\&c.} = \frac{2n + 1}{2} \lambda$$

$$= r_2 - r_1 \text{ nearly.}$$

If now θ be not large and $r_2 - r_1$ be only a small odd number of multiples of $\frac{\lambda}{2}$, the aperture will be nearly a parallelogram, and the maximum intensity $= \frac{a^2 \theta^2 \lambda^2}{\pi^2}$. But if in the original expression we make $r_1 = 0$, the aperture becomes the whole sector, and the maximum intensity, by giving r_2 the proper value, is $\frac{a^2 \theta^2 \lambda^2}{\pi^2}$. This shows that at however great a distance from C such a quadrilateral aperture be situated, and however near to C the point B may be, the intensity ought to be the same as for the sector.

The result of the principle is therefore that light ought to bend into the shadows of bodies to an indefinite extent, as sound is known to pass through all apertures, and bend round all obstacles.

It proves that the result Mr. Airy (Tract, page 270.) has obtained by an approximate method is not to be depended upon, and that the objection to the undulatory theory which was believed to have been removed remains in full force.

If it be said that these expressions involving λ^2 as a multiplier, must represent light of very feeble intensity, and therefore insensible, or nearly so, we shall see that we have the same small quantity in the expression for a large circular aperture.

If we make $\theta = 2\pi$, r_2 very large, and $r_1 = 0$, we have a large circular aperture, and the intensity

$$= 4\, a^2 \lambda^2 \sin^2 \left\{ \frac{\pi}{\lambda} \left(\sqrt{r_2^2 + h^2} - h \right) \right\}$$

$$= 4\, a^2 \lambda^2 \text{ when } \sqrt{r_2^2 + h^2} - h = \frac{2n+1}{2} \lambda;$$

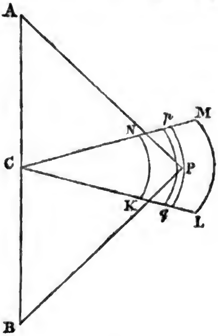
and however great or small h may be, compared with r_2, we see that there will be a succession of maxima and minima values for different values of h. This is at variance with the admitted properties of light, which it is allowed passes through large apertures without any change or diminution, or when diverging from a luminous point follows the law of the inverse square of the distance, except near the boundaries of the shadow.

If we compare the maximum intensity from an annulus, however narrow it may be, and however large the radii, with that from a large circular aperture, we see that *they are the same*; and the multiplier λ^2 would either show that only a very small quantity of light could pass directly through any large circular aperture; or otherwise, if it were maintained that the constant a may be very large; then our former conclusions will be free from any objection that they represent inappreciable quantities of light.

A fact stated by M. Fresnel, which I have confirmed by a severe experimental examination, bears upon the point, of the effect of the limits of apertures. His experiments for the diffraction by a single edge of an opake plate (see the before-mentioned memoir, page 429.) were, in fact, made with an aperture generally of a centimeter in breadth, whilst his luminous point and micrometer were in some measures distant about seven metres from each other. Notwithstanding the small breadth of the aperture compared with its distances from the luminous point and micrometer, yet we find him stating that the fringes formed by one edge were not affected by the other edge, and that his measures might be taken as if made with a single edge. The principle under discussion shows, that with the quadrilateral, sectorial or circular forms of apertures, the intensity should depend on the limits, however distant.

The same results arise when we take plane reflectors in place of apertures, only then the point B must be taken on the same side as that on which the waves are incident.

To discuss the second case, let A be the luminous origin in the line A C B perpendicular to the plane of the aperture, through the centre C. Let A C $=$ B C $= h$, and the other parts as in the former figure.



We have now for the displacement of the particle at B, due to the element p P q,

$$\frac{a\,\theta\, r\,\delta r}{\mathrm{A\,P}+\mathrm{P\,B}}\sin\left\{\frac{2\pi}{\lambda}\left(v\,t - (\mathrm{A\,P}+\mathrm{P\,B})\right)\right\}$$

and the whole displacement

$$=\frac{a\,\theta}{2}\int_r \frac{r}{\sqrt{r^2+h^2}}\sin\left\{\frac{2\,\pi}{\lambda}\left(v\,t-2\sqrt{r^2+h^2}\right)\right\}$$

$$=\frac{a\,\theta\,\lambda}{8\,\pi}\cos\left\{\frac{2\,\pi}{\lambda}\left(v\,t-2\sqrt{r^2+h^2}\right)\right\} + \text{C between the limits} \left.\begin{matrix} r=r_1 \\ r=r_2\end{matrix}\right\}$$

$$= \frac{a\,\theta\,\lambda}{4\,\pi} \sin\left\{\frac{2\,\pi}{\lambda}\sqrt{r_2^2+h^2} - (\sqrt{r_1^2+h^2})\right\}$$

$$\sin\left\{\frac{2\,\pi}{\lambda}(v\,t - \sqrt{r_2^2+h^2} - \sqrt{r_1^2+h^2})\right\}$$

and the intensity

$$= \frac{a^2\,\theta^2\,\lambda^2}{4^2\,\pi^2} \sin^2\left\{\frac{2\,\pi}{\lambda}(\sqrt{r_2^2+h^2} - \sqrt{r_1^2+h^2})\right\}.$$

As in the former case, when we take h small and r_1, r_2 very large, with θ small, so as to form a quadrilateral aperture approximating very nearly to a parallelogram, we arrive at the same conclusion, that light, according to the principle under discussion, ought to pass through apertures, however *obliquely* situated with respect to its direction, and diverge into the shadow to an indefinite extent; the maximum intensity at B, being the same as if the aperture were the whole sector, and this holding, however near A and B may be to C.

If we take $A = 2\,\pi$ and take r_2 very large, whilst r_1 is small, we have the case of the intensity in the centre of the shadow of a small circular disc, which it was found by approximate methods, ought to be the same as if the light passed uninterrupted; and M. Arago, having tried the experiment, found the result to accord. The complete investigation gives the intensity

$$= \frac{a^2\,\lambda^2}{2^2} \sin^2\left\{\frac{2\,\pi}{\lambda}\sqrt{(r_2^2+h^2 - \sqrt{r_1^2+h^2})}\right\},$$

which goes through a series of maxima and minima values for different values of h, when r_1 and r_2 are given; and *not* an uniform or slowly diminishing intensity along this line, as found by the approximate discussion.

There is another case which places the absurdity of the principle in a very striking point of view, which is the case of a large circular aperture, then $r_1 = 0$, and we have the intensity

$$= \frac{a^2\,\lambda^2}{2^2} \sin^2\left\{\frac{2\,\pi}{\lambda}(\sqrt{r_2^2+h^2} - h)\right\}:$$

the maximum intensity is here dependent on h for its position, but not for its magnitude; that is, the maximum intensity at B is $\frac{a^2\,\lambda^2}{2^2}$, however near A and B may be together, or however distant; contrary to the received and demonstrated principle, that the intensity of light diverging from a luminous origin varies as the inverse square of the distance.

Queen's College, Sept. 1840.

XXXVII. *On Sulphocyanogen.* *By Mr.* E. A. Parnell[*].

1. *Its Composition.*

WHILE engaged with an investigation of the action of alkalies on this substance (the results of which will be presently communicated), in which I was unable to account, in a satisfactory manner, for the production of a new acid, I was led to suspect the existence of hydrogen in sulphocyanogen; more especially as M. Liebig had obtained traces of water in its combustion by oxide of copper, which he then attributed to hygrometric moisture.

Before detailing my own results, I may state those which Liebig obtained with reference to this subject (*Ann. de Chim. et de Phys.*, tom. xli. p. 200.). Three-tenths of a gramme of sulphocyanogen, dried with great care *in vacuo*, afforded him in four combustions,

1. ·011 gramme of water.
2. ·017 — —
3. ·009 — —
4. ·016 — —

The mean of these gives 0·48 per cent. of hydrogen.

In repeating the analysis I have invariably obtained a much larger proportion of hydrogen. The sulphocyanogen examined was precipitated from the sulphocyanide of potassium by chlorine, and possessed all the characters of a pure substance: that used in the first and second analyses was kept on a sand-bath, at about 200°, for several hours, and afterwards for four hours in a water-bath at 212°.

1. 10·36 grains gave 0·85 grain of water equal to 0·91 per cent. of hydrogen.

2. 10·27 grains gave 7·49 carbonic acid, and 0·84 of water, or equal to 20·22 per cent. of carbon, and 0·90 of hydrogen.

To avoid all chance of error from the presence of hygrometric moisture, sulphocyanogen prepared by chlorine was dried in a Liebig's drying tube for some hours, by a nitre-bath at 242°. At this temperature a faint odour of cyanogen was perceived, but no other change.

11·02 of this gave 7·93 carbonic acid, and 0·96 of water, equal to 19·91 per cent. of carbon and ·96 of hydrogen.

To estimate the sulphur 10·11 grains were ignited with eight times as much nitrate and carbonate of potash; afterwards treated with nitric acid, diluted, filtered, and nitrate of barytes added. The sulphate of barytes amounted to 38·54 grains, which is equal to 52·59 per cent. of sulphur.

If the received equivalent of sulphocyanogen be doubled, and one equivalent of hydrogen added to it, making the formula $S_4\,C_4\,N_2\,H$, the hydrogen will amount to ·84 per cent. My estimations of the carbon and sulphur are then below the theoretical quantities. But before deciding on its constitution, an important question presents itself,—is the substance examined the radical of the sulphocyanides, or a product of the decomposition of that radical? To settle this point, the basic sulphocyanide of lead (the double sulphocyanide and oxide of lead), was first examined for hydrogen. It gave me 0·39 per cent., together with 4·20 per cent. of carbon. Now the hydrogen here is twice as much as it should be, supposing the salt to have the constitution $S_4\,C_4\,N_2\,H + 2\,Pb + 2\,Pb\,O$, or the usual formula doubled, and one atom of hydrogen added to the radical. So it might be a hydrate of the above containing one equivalent of water; in which case it should contain 74·32 per cent. of lead. If, on the other hand, it be $Pb\,Scy + Pb\,O\,.\,H\,O$, it should contain 73·26 per cent. The carbon and hydrogen agree for either view. Liebig obtained (see memoir above-quoted) 74·958 per cent. of lead; two analyses gave me the following results.

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[*] Communicated by the Author.

1. 53·67 grains treated with nitric and a little sulphuric acid, gave 53·97 grains of sulphate of lead, equal to 39·58 of lead, or 73·74 per cent.

2. 59·98 gave 64·91 of sulphate, equal to 44·32 of lead, or 73·85 per cent.

As from these analyses the question was still undecided, I took the sulphocyanide of silver, which gave 7·20 per cent. of carbon, and only ·05 of hydrogen, which is evidently due to hygrometric moisture; for if hydrogen existed in the radical, thus, $S_4\,C_4\,N_2\,H + 2\,Aq$, it should have contained ·30 per cent. Thus it appears that the radical of the sulphocyanides does not contain hydrogen, and consequently, what has been regarded as sulphocyanogen, is a product of the decomposition of that radical. I will presently consider how it is produced.

It also appears that the basic sulphocyanide of lead is a hydrate (forming one of those few substances which contain six elements); probably, according to the formula $Pb\,Scy + Pb\,O\,H\,O$, containing

	Calculated.	Found.
Sulphur.........	11·38	
Carbon	4·33	4·20
Nitrogen	5·01	
Hydrogen	·34	·39
Oxygen	5·66	
Lead...........	73·28	73·78
	100·00	

To return to the composition of "sulphocyanogen." It is greatly to be regretted that we possess no data to decide on the formula of this substance, but its composition per cent. as given by ultimate analysis, which is far less satisfactory in an uncrystallized substance, as in the present instance, than in a crystallized body: and I do not know of any circumstance to guide us in fixing its equivalent.

The constitution which first presents itself as most probable, is to double the old formula, and add one equivalent of hydrogen to it, which would give sulphur 54·48, carbon 20·70, nitrogen 23·97, hydrogen 0·84 = 100.

But this does not exactly accord with the results of analysis. Taking the mean, we shall have as its composition per cent.,

Sulphur…………	52·59
Carbon …………	20·06
Nitrogen (calc.)..	23·23
Hydrogen………	·92
	96·80

Here is then a deficiency of 3·2 per cent., which must be considered as oxygen. The formula with which this best agrees, and which I would adopt *provisionally*, is $S_{12}\ C_{12}\ N_6\ H_3\ O$, or

12	Sulphur ……	2414	53·27
12	Carbon……..	917·22	20·24
6	Nitrogen……	1062·24	23·45
3	Hydrogen …	37·43	·83
1	Oxygen ……	100·00	2·21
1	Equivalent…	4530·89	100·00

On this view of its composition, the reactions which occur in its production by chlorine may be explained in the following manner:

3 equiv. of water ………… = $H_3\ O_3$
6 — chlorine……… = Cl_6
6 — sulphocyanide of potassium } = $S_{12}\ C_{12}\ N_6\ K_6$
$H_3\ O_3\ S_{12}\ C_{12}\ N_6\ K_6\ Cl^6$.

Equal to
6 equivs. of chloride of potassium } = $K_6\ Cl_6$
1 — sulphocyanogen = $H_3\ O\ S_{12}\ C_{12}\ N_6$
2 — oxygen……… = O_2
$H_3\ O_3\ S_{12}\ C_{12}\ N_6 \quad K_6\ Cl_6$

Or six equivalents of sulphocyanide of potassium, six of chlorine, and three of water, become one of sulphocyanogen, six of chloride of potassium, and two of oxygen. But what becomes of this oxygen? I have observed, that however slowly the chlorine be passed through the solution of sulphocyanide of potassium, an oxidizing action on the sulphocyanogen already formed takes place, even at the commencement of the operation; sulphuric and cyanic acids being produced. It does not appear how this action can be satisfactorily explained on the old view (a simple removal of potassium by chlorine), for it involves the decomposition of water by chlorine in a strong solution of sulphocyanide of potassium, or in fact, that water is more readily decomposed by chlorine than sulphocyanide of potassium. But on the view given above, an oxidizing action on sulphocyanogen already formed is easily explained; it is even essential, since no evolution of oxygen gas is to be noticed.

The production of this substance by nitric acid can be explained in a similar manner. Six equivalents of water, sulphocyanide of potassium, and nitric acid, are equal to six of nitrate of potash and six of hydro-sulphocyanic acid, or $S_{12}\,C_{12}\,N_6\,H_6$, which with four equivalents of oxygen from the decomposition of another portion of nitric acid, become $S_{12}\,C_{12}\,N_6\,H_3\,O$, and $3\,H\,O$.

Four equivalents of oxygen of the atmosphere acting on six of hydro-sulphocyanic acid, produce the same effect.

With regard to the arrangement of the elements in this substance, it is obvious that many formulæ might with equal probability be selected. All that can be reasonably assumed on this subject is, that the carbon and nitrogen exist as cyanogen: it is also probable that the cyanogen and sulphur are more intimately connected with each other than either of them is with the oxygen or hydrogen; in which case this substance will be either a hydrate or an oxide of a hydruret of a sulphuret of cyanogen: but it appears probable that in all compounds which contain hydrogen united to a radical so as to form a hydruret, the hydrogen should be removed by chlorine, which in this instance is not the case. On another view the hydrogen (either wholly or in part) might be supposed to exist as an electro-positive or "zincous" element, but we do not find that it is displaced by metals, which certainly ought to be the case if such is the true constitution. But instead of adopting views of its constitution on insufficient data, it will be better in the present state of our knowledge of this substance to be satisfied with its empirical formula alone. The name of the substance will obviously require change, but I shall leave this to him to whom we are indebted for its discovery; and although I have been led to observe an error in the conclusion at which he had arrived, it must not be forgotten that it is by his refined and beautifully simple method of analysis that I have been able to attain my results.

For convenience, I shall continue in the remainder of this paper to speak of the substance derived from the sulphocyanides as sulphocyanogen.

2. *Action of Alkalies on Sulphocyanogen.*

We are indebted to Wœhler and Liebig for all that is as yet known respecting the action of alkalies on this substance; but this subject was not studied so completely by them as it appears to deserve, nor so satisfactorily as other points connected with the sulphocyanides in their remarkable researches.

It appears from their experiments, that when sulphocyanogen is digested in solution of potash, a small portion is dissolved, and the remainder becomes redder, partly soluble in water, and partly in alcohol. After alcohol and water had been successively applied to this altered sulphocyanogen until nothing more was dissolved, a clear yellow substance remained, which Liebig considered as a higher degree of sulphuration of cyanogen. But his analyses of this substance do not sufficiently accord with any theoretical numbers to decide its constitution.

My own experiments certainly confirm the above as far as they go, but it would appear that heat had not been applied to the mixture of sulphocyanogen and alkali, but that it had, on the contrary, been kept at common temperatures; for if sufficient potash had been present and a gentle heat applied, the whole of the sulphocyanogen would have been dissolved, and converted into other substances.

When three parts of sulphocyanogen are digested with about four parts of potash and twenty or twenty-five of water, a portion is at once dissolved, the remainder, on the application of a gentle heat, forming a reddish yellow transparent solution, from which acids throw down a yellow precipitate, which is a mixture of two substances, one of a light lemon colour, the other brown, or almost black. The appearance of the changes which are here undergone, varies considerably with the manner of performing the operation. If, for instance, the sulphocyanogen be in excess, it is either not entirely dissolved, or if it is, the precipitate produced by acids contains unaltered sulphocyanogen: on the contrary, if the alkali be in excess, the decomposition is more complete, the precipitate has a much lighter colour, and does not appear so dense as in the former case. The length of digestion also considerably affects the result; the longer it is continued (within certain limits) the more complete is the action: and lastly, the method of preparation of sulphocyanogen likewise influences it; while that prepared by chlorine becomes red at the commencement of the action, that prepared by nitric acid becomes yellow. The cause of this difference will be immediately explained.

Of the two substances precipitated by acids, the lemon-coloured one forms by far the largest portion; and not having obtained the other pure, I have as yet paid little attention to it, although it appears intimately connected with the decompositions which occur. The last is insoluble in water and alcohol, while the lemon-coloured substance is soluble in both these liquids, which can consequently be used to separate them.

I have found the following method of procedure to be the most convenient. Take three parts of sulphocyanogen (that prepared by nitric acid is preferable) and four of potash, or one of sulphocyanogen, and 27 or 28 parts of solution of caustic potash in common use (sp. gr. 1·06), keep this mixture at a gentle heat (120°) for about three hours, and then boil for half an hour. It is then entirely dissolved, but on cooling a small quantity of the black matter separates, which must be removed by filtration. To the filtered solution add hydrochloric or dilute sulphuric acid, which throws down the mixture in question, sometimes of a bright lemon-colour, but more frequently darker. It must be collected on a filter and washed with cold water until all the chloride of potassium or sulphate of potash is removed. Boiling alcohol must be used to purify it, as hot water dissolves too minute a portion to be conveniently employed for this purpose: the filtered alcoholic solution can be distilled nearly to dryness, which gives the substance perfectly pure in the form of a flocculent lemon-yellow crystalline powder.

Its taste is intensely bitter and acrid, but not immediately perceptible, on account of its slight solubility: it thickens the saliva, and a minute portion of its dust inhaled causes sneezing. One part requires rather more than one thousand of cold water to effect its solution. Boiling water dissolves 2·36 per cent. Cold alcohol takes up 4 per cent., boiling alcohol about 14 per cent. Wood spirit possesses about the same solvent power on it as alcohol.

When ignited in the air sulphur burns, and a brown substance remains, which is entirely dissipated by a strong red heat: heated in a tube, sulphur, bisulphuret of carbon and sulphuretted hydrogen are given off, leaving the same brown matter. It is soluble in concentrated sulphuric acid without change, and is again precipitated on the addition of water. Nitric acid completely decomposes it, giving rise to sulphuric, carbonic and nitrous acid. Hydrochloric acid dissolves a little without change.

When its alcoholic solution is evaporated it appears to give crystals, but this is not the case. It is owing to a bright pellicle which had been formed on the surface of the alcohol contracting, and presenting this appearance. The strong alcoholic and wood-spirit solutions are precipitated by water. All its solutions are yellow; they redden litmus paper slightly, but some time is required to effect this; it would at first be said to be neutral. From its behaviour with metallic bases and the mode of its formation, it is manifestly entitled to be classed among acids; in short, it appears from my experiments, that as obtained by the above process it is a hydrate of an hydracid which is quadribasic, losing four atoms of hydrogen and acquiring four of a metal. All its salts that I have examined are coloured, being either yellow, brown, or black. They are uncrystallizable, and those which are soluble are partially decomposed by evaporation: for this reason, I have been unable to obtain any definite soluble salt in a state fit for analysis, and the insoluble salts that I have examined contain a large excess of acid. According to my experiments, its empirical formula will be $S_{12}\,C_{10}\,N_5\,H_6\,O_2$; its rational formula $S_{12}\,Cy_5\,,\,H_4 + 2\,aq.$

The following are the results of my analyses.

11·01 grains of the pure substance (prepared by the above process), dried at 212°, gave 7·00 grains of carbonic acid and 1·78 grains of water, or 17·58 of carbon and 1·79 of hydrogen per cent. Other analyses have given a mean of 17·60 of carbon and 1·74 of hydrogen. The mean of three estimations of the sulphur, (by heating with nitric acid which readily decomposes it, and precipitation of the sulphuric acid by nitrate of barytes) is 55·16 per cent. The nitrogen was estimated in the usual manner, by observing the relation between it, and the carbonic acid as produced by combustion by oxide of copper; but the product was collected in one receiver instead of several small tubes. The results are as follows:

Barometer 30·2 inches.

Mixture in receiver, 85 measures over ·6 inch of mercury, equal to 83·3 measures, common pressure.

After absorption of carbonic acid, there remained 32 measures, over 3·4 inches of mercury, equal to 28·3 measures, common pressure.

83·3—28·3 = 55·0; therefore 83·3 of mixture contain 28·3 of nitrogen, and 55·0 of carbonic acid: and as 28·3 : 55·0 : : 1 : 1·94; or as 1 of nitrogen to 2 of carbon nearly.

We have then for the per-centage,

Sulphur	55·16
Carbon	17·59
Nitrogen	20·37
Hydrogen	1·76
Oxygen (loss)	5·12
	100·00

which numbers closely correspond to

			Calculated.
12	Sulphur	2414·	55·64
10	Carbon	764·3	17·61
5	Nitrogen	885·2	20·42
6	Hydrogen	74·8	1·72
2	Oxygen	200·0	4·61
1	equivalent	4338·3	100·00

Since it appears to be a hydrated hydracid of a sulphuret of cyanogen, the name hydrothiocyanic acid will perhaps not be inapplicable. Its salts will then be thiocyanides, and its symbol may be Thcy H_4.

Thiocyanides.—The alkaline reaction of potash, soda, ammonia, and barytes cannot be completely destroyed by any excess of this acid, although the pure salts are neutral. The alkaline carbonates are not decomposed by it at common temperatures, but if boiled, carbonic acid is evolved, and a salt of the alkali formed. Its solutions in potash and soda give yellow uncrystallized residues on evaporation. The acid was digested some time with ammonia, filtered and evaporated over sulphuric acid *in vacuo*: this gave a yellow uncrystallized salt, soluble in water, giving a solution neutral to test paper, and intensely bitter; but free acid had been deposited during evaporation, notwithstanding it was at first highly alkaline.

Barytes.—The acid was digested with barytes, water, and carbonic acid gas passed through the solution to separate excess of barytes: a yellow solution remained, which on evaporation gave a yellow salt, but mixed with crystals of hydrate of barytes.

A solution of the acid produces no precipitate in solutions of salts of magnesia, both oxides of iron, manganese, zinc, or nickel.

Copper.—A solution of the acid produces an ochre-brown precipitate in solution of sulphate of copper. The precipitate is decomposed by the hydrochloric, nitric and concentrated sulphuric acids; also by sulphuretted hydrogen, the hydrothiocyanic acid being liberated. It is blackened by alkalies, an alkaline thiocyanide being produced: the black or brown substance remaining appears to be a subsalt. When the thiocyanide of copper is ignited in a tube, it gives off sulphur, hydrated cyanic acid, and bisulphuret of carbon, leaving a residue of sulphuret of copper.

Lead.—Solutions of hydrothiocyanic acid produce a yellow precipitate in acetate and subacetate of lead, which is decomposed by the stronger acids, and by sulphuretted hydrogen, the acid being reproduced unaltered in the last case. Nitric acid instantly produces sulphate of lead. On being heated in a tube, it gave rise to similar products as the copper salt; hydrated cyanic acid, sulphur, bisulphuret of carbon, and sulphuret of lead. Like most insoluble salts of slightly soluble acids (when prepared from solutions of the acid), this is contaminated with a large excess of acid. Two combustions by oxide of copper gave

1.	8·62 per cent. of carbon	·49 of hydrogen.
2.	8·72 ———	·50 ———

For the lead: 1. 12·3 grains treated with nitric acid gave 9·36 grains of sulphate of lead, equal to 51·96 per cent. of lead.

2. 10·77 treated in the same manner, gave 8·19 of sulphate, or 51·93 per cent.

Calculated according to the formula Thcy 4 Pb + 4 aq, it should contain 7·90 of carbon, ·51 of hydrogen, and 53·40 of lead per cent.; notwithstanding this difference, no other formula could have been selected, in accordance with the ultimate composition obtained for the acid.

Like the copper salt, thiocyanide of lead is blackened by alkalies, a subsalt being produced.

Silver.—The behaviour of the solution of hydrothiocyanic acid with nitrate of silver is very peculiar and characteristic, forming a test sufficiently delicate to detect one part of the acid in 10·000 of water. On mixing the solutions a yellow flocculent precipitate is formed, which on standing a short time, or immediately on heating, aggregates, changing to a black colour, without any evolution of gas or odour of cyanogen. The change is not hastened by solar light. When the black substance is treated with sulphuretted hydrogen, the acid is reproduced, and sulphuret of silver formed. Concentrated sulphuric acid has no effect on it; if diluted, sulphate of silver is formed, and the acid is liberated. It is soluble with decomposition in nitric acid, and by boiling in hydrochloric acid, sulphuretted hydrogen being evolved in the latter case. The affinity of silver for the radical of this acid appears stronger than for chlorine; for if this acid and hydrochloric acid be present in the same solution, and nitrate of silver added, the thiocyanide of silver is first precipitated. It is insoluble in ammonia.

19·60 grains were treated with hot nitric acid, which dissolved it; the silver was precipitated by hydrochloric acid, and nitrate of barytes added to the filtered solution to obtain the sulphuric acid.

The chloride of silver amounted to 18·25 grains, or 13·747 of silver, equal to 70·14 per cent. The sulphate of baryta amounted to 21·24 grains, equal to 2·94 of sulphur: sulphur not converted into sulphuric acid, separated on a weighed filter, amounted to ·20 grain, = 3·14 or 16·01 per cent. of sulphur.

The proportion of silver here is just twice as great as the proportion of lead in the salt of that metal. In fact, it appears to be a double thiocyanide and oxide of silver, or a subsalt: thus Thcy 4 Aq + 4 Aq O, which by theory should contain

		Found.
Sulphur	15·80	16·01
Silver	70·78	70·14

As the neutral thiocyanides are yellow and the subsalts black, so we have reason to believe that the yellow silver compound is the neutral thiocyanide; but this cannot be decided by analysis, on account of the rapid change which it undergoes.

Mercury.—Nitrate of protoxide and chloride of mercury are precipitated by the aqueous solution of this acid. The precipitate is at first white, but by heating it becomes yellow. In its properties it resembles the copper and lead salts; like them it is converted into a subsalt by alkalies, and it gives similar products on heating. Nitric acid instantly acts on it; a white compound is produced which undergoes no further change by nitric acid alone, but on the addition of hydrochloric acid it is immediately dissolved.

Nitrate of suboxide of mercury gives the black subthiocyanide. Chloride of platinum and bichloride of tin are precipitated yellow by solutions of the acid, but I have not examined these precipitates.

Such is the incomplete investigation I have had it in my power to make of these compounds. As the sulphocyanogen derived from the sulphocyanides does not appear to be what it was considered, but a highly complicated substance, the interest with which the products of its decomposition by alkalies will be viewed, is materially lessened. For this reason I have not pursued this subject so completely as I intended, nor, perhaps, as it deserves. Although I have been led by a point in this investigation, which I could not comprehend, to the discovery that sulphocyanogen, if not exactly so composed as I have represented it, is certainly not the radical of the sulphocyanides, but a product of its decomposition; yet the point in question has not been ascertained, namely, the changes that sulphocyanogen undergoes by alkalies.

At first I imagined the black substance which separated from the solution of sulphocyanogen in alkali, on cooling, and the brown or black substance precipitated by acids, *insoluble in water and alcohol,* as accidental or merely secondary products: I now look on these differently. When the last brown substance is digested again in an alkali, more hydrothiocyanic acid is separated; the remainder is quite black, and is evidently identical with the first black substance. It appears more to resemble paracyanogen than anything else with which I am acquainted; but not having obtained it pure, and but in small quantity, I have not been able to decide this point. Sulphocyanide of potassium, and sulphite of potash, are likewise formed when sulphocyanogen is digested in potash, but the sulphite is in very small quantity, and I believe accidental.

Hydrothiocyanic acid is not produced by potash and soda only; barytes, ammonia, and even the alkaline carbonates give birth to it. Its production by ammonia is remarkable. The sulphocyanogen becomes light yellow, but is not dissolved; sulphocyanide of ammonium, with only a trace of thiocyanide, is formed. The yellow substance into which the sulphocyanogen is converted is, however, hydrothiocyanic acid.

This proves that whatever may be the action of the alkali, it is not the affinity of its radical for the radical of the acid (or as it may be called, thiocyanogen) which is the leading cause; for if so, the whole of the acid should have existed as thiocyanide of ammonium.

It is also probable that the action is an oxidizing one, for I find that the acid is produced by the action of chlorine and nitric acid on sulphocyanogen. Indeed, it is difficult to prepare sulphocyanogen by nitric acid without its production, and hence I have recommended that prepared by this means as preferable to that by chlorine for the preparation of this acid *. It has been observed, that when chlorine is passed into a dilute solution of sulphocyanide of potassium, the sulphocyanogen is precipitated of a light yellow colour, and does not subside readily. I have ascertained, by experiment, that this light yellow substance is not sulphocyanogen, but hydrothiocyanic acid. It is, however, more difficult to convert sulphocyanogen into this acid by chlorine, than by nitric acid; chlorine consequently gives the purer substance.

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* If a mixture of sulphocyanogen and hydrothiocyanic acid be digested

In conclusion, I would state, that these experiments have been performed in Prof. Graham's laboratory, to whom and to his late assistant Mr. Fownes, I am happy in acknowledging myself indebted for their suggestions during this research.

University College, June 25, 1840.

XXXVIII. *On the Use of Hydriodic Salts as Photographic Agents. By Mr.* Robert Hunt.

[Continued from p. 211, and concluded.]

38. *On the darkening of the Photograph.*

MR. TALBOT first directed attention, at the last meeting of the British Association, to a peculiarity possessed by some of these kinds of photographs, namely, that they were neither fixed nor otherwise; but that on exposure to sunshine they changed in their dark parts from a red to a black, the lights of the picture being unaffected by the light.

39. This singular effect I have proved to be entirely dependent on the influence exerted by the less refrangible rays of the solar spectrum in exalting the oxidation of the silver; but a brief statement of some effects produced by the dissevered rays, will place the matter in a much clearer light.

40. By allowing a very intense prismatic spectrum, formed by a flint-glass prism, to fall upon any of these photographs which blacken by white light, it will be found that the darkening process commences in the red ray, at which point it goes on with the greatest intensity, and is gradually shaded off to the lowest edge of the extreme red; the shading is also continued through the orange and yellow rays being sharply cut off at that line of the spectrum where the pure green is visible.

41. As it was not possible to pursue my inquiry on the effects of the spectrum with any degree of satisfaction without a heliostat, an instrument I have not the means of procuring,

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in alkali, the sulphocyanogen is first dissolved, leaving the acid. This explains why sulphocyanogen prepared by nitric acid becomes yellow when treated with alkali, which is not the case with that prepared by chlorine.

I turned my attention to the effects produced by the light which had permeated coloured media, the absorptive powers of which were carefully analysed.

The media I was induced to adopt transmitted rays in the following order.

BLUE. *Ammonia-Sulphate of Copper.*—The whole of the most refrangible rays, from the edge of the green to the extremity of the violet.

GREEN. *Nitro-muriate of Copper.*—Those rays which have place between the extreme upper edge of the blue, and a line which would accurately divide the pure yellow.

YELLOW. *Bi-chromate of Potassa.*—That portion of the spectrum which would lie between a line drawn below the orange, rather within the red ray, and through the lower edge of the pure green ray.

RED. *A strong Solution of Carmine in Ammonia.*—A portion of the orange and all the rays below it.

42. The most remarkable effects were produced upon the papers *a*, *b*, *c*, *d*, and *n* (13). They have been subjected to similar influences, prepared with all the hydriodates I have mentioned (20—27); but I do not feel myself warranted in occupying your pages with any statement of the results on any, but those prepared with the pure hydriodate of iron and the hydriodate of baryta. These drawings were all well washed with hot water, and when quite dry, arranged under the different fluids, and exposed in a window which faces the south. I will name the papers from the salt used, and the colour shall indicate the rays.

43. *Hydriodate of Iron, Muriate of Ammonia.*—BLUE. The picture nearly destroyed by the browning of the yellow lights, at the same time as the darker parts have much faded.

GREEN. The dark parts nearly all faded out; the few remaining spots much reddened, but no change in the yellow of the light parts.

YELLOW. Looking through the paper, the lights appear darkened by a blueish-green tinge; the dark parts, originally a red brown, are changed to a blue-black.

RED. The lights yellower than before; the darks a deep black.

44. *Chloride of Sodium.*—BLUE. The lights darkened, and the dark parts faded and reddened. GREEN. Picture entirely obliterated; the yellow unchanged. YELLOW. The lights tinged a decided blue; shadows darkened.

RED. The lights of a green tinge; but I consider this to arise from the deepening of the yellow hue; the dark parts blackened.

45. *Muriate of Strontia*.—Blue. These are more permanent than any other variety of the hydriodic photographs. Under this influence the shadows are browner than before; the lights scarcely changed. Green. The yellow much increased in depth; the dark parts faded slightly and become very red. Yellow. The lights very little tinged with blue; the darks without any apparent change. Red. The lights deepened; the shadows a fine black.

46. *Muriate of Baryta*.—Blue. The yellow parts are become brown; the dark portions faded and reddened. Green. Lights unchanged; the dark parts very red. Yellow. Lights unchanged; shadows tinged green, over a very decided blackening which has taken place. Red. The yellow much heightened; the dark parts much tinged with green.

47. *Hydrochloric Acid*.—Blue. Faded out: lights darkened. Green. Faded out; yellow much increased. Yellow. The lights rendered very yellow; darks unchanged. Red. Yellow become very strong; the shadows are very much blackened.

48. *Hydriodate of Baryta*.—Under this head it will only be necessary to name the effects on three kinds of photographs, the others being very similar in all their changes to those just mentioned.

49. *Muriate of Ammonia*.—In my paper on the influence of coloured media, vol. xvi. p. 270 of your Journal, I have already mentioned the singular change which ensues upon exposing this kind of drawing to light under media such as we are now considering. To that paper I refer you.

50. *Muriate of Baryta*.—Blue. Faded in the dark parts, which are become a brick red; the yellowness of the lights increased. Green. The lights unchanged; the shadows suffused with a pink hue. Yellow. Lights unchanged; shadows much darkened and strongly tinged with a light blue. Red. Lights unchanged; dark parts a deep blue. These singular effects, which, although they are traceable on nearly all those photographs which blacken by after exposure to sunshine, are much more decided when the salts of baryta in one or other of the processes have been used. I communicated these facts with others to Sir John Herschel, who has paid me a very high compliment by inserting my communication in his valuable memoir "On the Chemical Action of the Solar Spectrum." I the less regret my inability to pursue my observations on the effects of the pure prismatic rays on the hydriodic preparations, finding that the subject is one which, among others equally curious and important, is engaging the attention of this eminent philosopher.

51. *Muriate of Strontia*.—Blue. Lights but very little

changed; darks faded and reddened. GREEN. Lights unchanged; shadows less faded, not so red. YELLOW. Lights unchanged; dark parts a blue-black. RED. Lights unchanged; dark parts become very black.

52. From a careful perusal of these results it will appear that this curious darkening of the finished picture is most evident under the influence of red light, but that this property extends up to the green rays, beyond which a different power is exercised; the *deoxidizing* influence appearing to be greatest in the blue rays, whilst the yellow iodide of silver suffers decomposition in the most refrangible rays.

53. *The fading of Hydriodic Photographs.*—I have before noticed (30.) the want of absolute permanence in these pictures. The study of the *modus operandi* of solar light in its action on them opens some very remarkable facts in relation to the iodide of silver, which when first observed, led me to believe the existence of two distinct salts, whereas I now entertain a different opinion. The drawing fades first in the dark parts, and as they are perceived to lose their definedness, the lights are seen to darken, until at last the contrast between light and shadow is very weak.

54. If a dark paper is washed with an hydriodate and exposed to sunshine, it is first bleached, becoming yellow; then the light again darkens it; if, when quite dry, it is put away in the dark, it will be found in a few days to be again restored to its original yellow, which may be again darkened, but not so easily as at first, and the yellow colour is again restored in the dark. The sensitiveness to the influence of light diminishes after each exposure, but I have not been enabled to arrive at the point at which this entirely ceases.

55. If a dark paper, bleached by an hydriodate and light, be again darkened, and then placed in a bottle of water, the yellow is much more quickly restored, and bubbles of gas will escape freely, which examination will show to be oxygen.

56. By inclosing pieces of hydriodated paper in a tube to darken, we discover, as might have been expected, some hydrogen is given off. If the paper is then well dried and carefully shut up in a warm dry tube, it remains dark; moisten the tube or the paper, and the yellowness is speedily restored.

57. Take a photograph thus formed and place it in a vessel of water, in a *few days* it will fade out, and bubbles of oxygen will accumulate around the side. If the water is examined, there will be found no trace of either silver or iodine; thus it is evident the action has been confined to the paper.

58. We see that the iodide of silver has the power of sepa-

rating hydrogen from its combinations. I cannot regard this singular salt of silver as a definite compound: it appears to me to combine with iodine in uncertain proportions. In the process of darkening the liberation of hydrogen is certain; but I have not in any one instance been enabled to detect free iodine; of course it must exist either in the darkened surface, or in combination with the unaffected under layer; possibly this may be the iodide of silver, with iodine in simple mixture, which, when light acts no longer on the preparation, is liberated, combines with the hydrogen of that portion of moisture which the hygrometric nature of paper is sure to furnish, and as an hydriodate again attacks the darkened surface, restoring thus the iodide of silver. This is strikingly illustrative of the fading of the photograph. The picture is light iodide of silver and dark oxide of silver; as the yellow salt darkens under the influence of light it parts with its iodine, which immediately attacks the dark oxide, which is gradually converted into an iodide, oxygen, as I have shown, being liberated. The following experiments go not only to prove this position, but also serve to illustrate in some measure the action of light on this compound.

59. *Iodide of Silver.*—Precipitate with any hydriodate, silver from its nitrate in solution, and expose the vessel containing it, liquid and all, to sunshine, the exposed surfaces of the iodide will blacken; remove the vessel into the dark, and after a few hours all the blackness will disappear: we may thus continually restore and remove the blackness at pleasure.

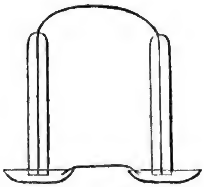
60. If we well wash and then dry the precipitate it blackens with difficulty, and if kept perfectly dry it continues dark; but moisten it and the yellow is restored after a little time.

61. In a watch-glass, or any capsule, place a little solution of silver; in another, some solution of any hydriodic salt; connect the two with a filament of cotton, and make up an electric circuit with a piece of platina wire, expose this little arrangement to the light, and in a very short time it will be seen that iodine is liberated in one vessel, and the yellow iodide of silver formed in the other, which blackens as quickly as it is formed.

62. Place a similar arrangement to the above (61.) in the dark, iodine is slowly liberated. *No iodide of silver formed*, but around the wire a beautiful crystallization of metallic silver.

63. A piece of platina wire was sealed into two glass tubes; these when filled, the one with hydriodate of potassa in solution, and the other with a solution of the nitrate of silver, were reversed into two watch-glasses containing the

same solutions, the glasses being connected with a piece of cotton, as in the adjoining figure.



A few hours of daylight occasioned the hydriodic solution in the tube to become quite brown with liberated iodine; a small portion of the iodide of silver was formed along the cotton, and at the end dipping in the salt of silver. (The glasses were kept purposely wide apart to prevent a quick formation.) *During the night the hydriodic liquid became again colourless and transparent, and the dark salt along the cotton was as yellow as at first.*

64. A curious illustration of the action I have endeavoured to elucidate, may be had by operating with chlorine. Its first action on one of these hydriodic photographs is the separation of iodine, which, when exposed to the light, is seen to act on the edges first, and gradually over the whole extent of the darkened portions. This curious action may be repeated until all the iodine is removed from the paper.

I think it will now be evident, that before we can expect to have quite permanent and well-finished hydriodic photographs, we must have at command the means of removing all the iodide of silver without injuring the dark oxide.

65. *On the Action of the dissevered Rays of the Solar Spectrum on dark photographic Papers washed with an hydriodate Liquid.*—Sir John Herschel, in his valuable memoir before-mentioned, has clearly shown, "that the total effect of a ray of white light on iodic preparations, is in fact the difference of two opposing actions, either of which may be exalted or enfeebled at pleasure by circumstances under our command, but difficult to reproduce exactly at our pleasure. When these opposing actions," I still quote Sir John's words, "exactly neutralize each other, the paper is insensible. When either preponderate, it is positive or negative in its character, according to that of the preponderant action; nay, it may at one and the same moment be positive to light incident under certain circumstances, insensible under others, and negative under a yet different illumination."

66. These singular facts were noticed by me in a very early stage of my inquiries; and you may perhaps remember my forwarding to you, with some specimens illustrative of my paper on "The Chemical Action of the Solar Spectrum," an hydriodated photograph, which exhibited the effects of coloured media in determining the action*.

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* See Sir John Herschel's memoir "On the Chemical Action of the Rays of the Solar Spectrum," Phil. Trans. 1840, Part i. page 43.

67. It is essential to a right understanding of the following results, that the absorptive power of the media I used in my experiments be set forth. Into a frame were fixed four coloured glasses.

A PURPLE GLASS, cutting off all the rays below the green, a portion of the green being also absorbed.

A GREEN GLASS, admitting the permeation of those rays only which lie between the least refracted extremity of the blue and the extreme orange. A portion of the yellow rays are absorbed.

A PALE AMBER GLASS, shortening the spectrum by the violet and indigo rays only.

A RED GLASS, absorbing all the most refrangible rays, permitting those only which lie below the blue to permeate.

68. If any of the sensitive darkened photographic papers (*a*, *b*, *c*, *d*, *o*,), washed with a good hydriodic solution, be placed in close contact, face to face, with an engraving which has been rendered transparent by being well soaked in water, and exposed to sunshine with the above frame superposed, we produce (to use Sir J. Herschel's nomenclature) a positive and a negative photograph on the same sheet. Beneath the blue glass the picture is copied as perfectly, but not quite so quickly, as under a colourless glass, the lights of the engraving being correctly copied on the photograph.

Beneath the green glass the lights and shadows of the photograph are completely reversed. In all the parts which correspond with the lights of the engraving the oxidation is much exalted, the paper having assumed a defined blackness. The darker parts of the engraving are copied in lights not simply formed by the contrast of the original brown of the paper with the induced blackness, but by a positive brightening of the parts.

Beneath the yellow glass the results are singularly uncertain. Often on the same sheet, with the same hydriodic solution, two experiments will give totally different results. I send you two specimens in proof of this, prepared in every way alike, and both executed within the same half-hour.

Under the red glass a reversed picture is formed, *not by the darkening of the oxide*, which retains its original colour, but by the eating out of strong lights under the dark parts of the engraving.

69. From these results it is evident that the blackening action on the wet hydriodidated paper is dependent on a different class of rays from those which blacken the finished drawings: in the wet process I ever find the maximum of darkness beneath the green glass, or rather within the limits of the green and yellow rays, little or no darkening effect being evident in the red rays; whereas on the dry picture

the maximum of effect is in the red rays (42—50.). I am inclined, however, to believe, that examination of these phænomena by a fixed spectrum will prove a shifting of the actions according to the kinds of paper used; but I am satisfied the entire action will be confined in the one instance to the green and yellow rays, and in the other to the orange and red rays.

70. The very curious brightening of those parts of the photograph corresponding with the darks of the engraving, attracted my attention powerfully. My first impression was, that the carbonaceous matter of the ink used in printing exerted a kind of catalytic action in directing the formation of an iodide of silver. I think I have proof of this to some extent.

71. Most printed pages, unless they are very old, when placed in juxtaposition *in the dark* with an hydriodidated photographic paper, leave an impression after some hours, and I have succeeded in partially copying some engravings thus. However, the result is uncertain; the copy, at all times faint, is often very imperfect, being sometimes bleached in circles, of which a letter or two form the centres; at other times the letters are copied, but all of them shaded from an extension of the bleaching action.

72. From effects I noticed from the accidental contact of some carbonate of iron, I was sanguine of being enabled to copy a written page. In this I was disappointed. I have not, with any of the numerous kinds of writing ink which I have tried, succeeded in obtaining the slightest trace of a letter on the photographic paper.

73. From the rapidity with which this effect is produced when the photograph and engraving are exposed to light, it is evident some other cause than the one I have just considered was in active operation. A careful examination of the photographs formed under the before-mentioned glasses, particularly under the red glass, convinced me that the quickening agent was to be sought for in the calorific rays, which are absorbed and retained with greater force by the dark parts of the engraving.

74. To put this notion to the test, I placed a printed page in contact with a paper wetted with an iodidated solution, over which I placed a glass, and then a plate of copper, which I made hot by rubbing it with a heated iron. The passage of the heat through the glass was sufficient to effect as fair a copy as is produced under the red glass by the influence of light.

75. These researches, which were pursued with a view

alone to an explanation of the peculiar mode of operation exhibited by the hydriodic salts on different preparations of silver under the influence of light, have thus opened a new and unexpected field of interesting inquiry, which may possibly end in the establishment of the new art of THERMOGRAPHY.

I shall pursue this subject with the same interest by which I have been led forward in my inquiries on Photography since the publication of Mr. Talbot's processes and those of Daguerre. For a few interesting applications and curious discoveries which I have made I merit not, nor do I seek praise. To every inquirer there is a mine of discovery, of which the few specimens I have gathered on the surface will, I trust, show the richness of the yet buried treasure.

I remain, Gentlemen, yours, &c.

Devonport, July 4, 1840. ROBERT HUNT.

XXXIX. *On the Form of Rutile.* *By* W. H. MILLER, *Esq., Professor of Mineralogy in the University of Cambridge.*

THE following values of the angles between normals to the faces of rutile were obtained from two extremely perfect crystals, for which I am indebted to Mr. Brooke. The instrument used as a goniometer was a twelve-inch theodolite. The coincidences of the signals were observed with a telescope having a power of about twelve. Each result is the mean of two observations made with the signals interchanged, in order to eliminate the error arising from the imperfect centring of the crystal. In one the faces p'', p''' (fig. 1.) gave double images, those in p''' being close and ill-defined. The observed values of $p'\,p'''$ were 65° 33′ 0″—30″—8″; those of $p\,p''$ 65° 32′ 22″, or 65° 34′ 26″, according as one or the other of the images was made to coincide with the signal seen by direct vision. In the second and more perfect

Fig. 1.

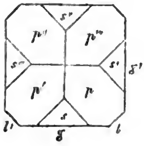
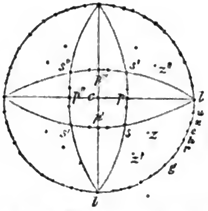


Fig. 2.



of the two crystals, the observed values of $p' p'''$ were 65° 34′ 24″—36″—51″—27″; those of $p p''$ were 65° 34′ 27″—53″—25″—35″. The values of $p' p'''$, $p p''$ given by the second crystal, agree very well with each other, and with the larger of the two values of $p p''$ given by the first. The mean of these three, 65° 34′ 32″, will probably be very near the truth.

The sphere of projection (fig. 2.) exhibits the poles of all the faces observed by Mohs and Levy, as well as by myself. The symbols of the simple forms are

g	{110},	h	{210},	l	{100},
r	{320},	x	{410},	c	{001},
p	{101},	s	{111},	e	{310},
z	{321},	t	{313},	u	{710}.

The calculated angles between normals to the faces are

$l l'$	90°	0′	$l r$	33°	41′	$p p'$	45°	2′
$l g$	45	0	$c l$	90	0	$s s'$	56	52
$l u$	8	7·8	$c p$	32	47·3	$p t$	10	14
$l x$	14	2	$c s$	42	20	$z z'$	20	46
$l e$	18	26	$c t$	34	10·6	$z z''$	61	14·8
$l h$	26	33·9	$c z$	66	42·3	$p z$	41	43·3

z is the intersection of the zone circles $p p'''$, $s p''$, $c r$; t is the intersection of the zone circles $p l'$, $c e$.

Of the crystals above-mentioned, one is a combination of the simple forms having the faces l, g, h, e, p, s; the other of those having the faces g, h, e, u, p, s; t was observed by Levy (*Déscription d'une Collection de Mineraux*).

In the collection of minerals presented to the University by Professor Whewell, a crystal occurs which is a combination of the forms having the faces h, c, r, p, z, and also others which are combinations of the forms having the faces x, p, and occasionally c, l, g, h, e. Among the latter are several twins (fig. 3.), in which the faces $c\, p$, $c_{,}\, p_{,}$ are all in one zone, and the angle between normals to $c\, c_{,}$ rather more than 55°. On account of the unevenness of the faces, this angle could not be accurately measured; if the twin plane were parallel to a plane v, the pole of which is the intersection of the zone circles $z z''$, $c l$, the symbol of v would be (301), $c v = 62° 38'{\cdot}4$, $c c_{,} = 54° 43'{\cdot}2$. Hence probably the twin plane is parallel to a face of the form {301}. The twins of most usual occurrence are those described by Haidinger (Edinburgh Journal of Science, vol. iii. p. 62.). Mr. Brooke measured the angle between the faces $l\, l_{,}$ (fig. 4.) of one of these twins in his collection, and found that it agreed perfectly with the supposition that the twin plane was parallel to one of the faces p.

The values of *c p*, *c s*, deduced from the observed values of *p l*, *s g*, given by W. Phillips in his Mineralogy (third edition), are 32° 45′ and 42° 20′. My determination of these angles differs only about two minutes from the former, and agrees exactly with the latter. Unfortunately the figures which were intended to express the angles $p p''$, $s s''$ ("*a* on *a* over summit = 90°, *c* on *c* over summit = 109° 47′") are very erroneous, probably through some mistake in transcribing them, for the errors (which remain uncorrected in the fourth edition) are much too large to have been errors of observation.

Fig. 3.

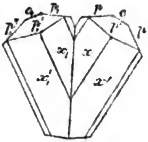
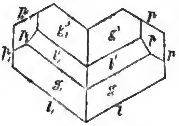


Fig. 4.



St. John's College, Aug. 31, 1840. W. H. Miller.

XL. *Note upon* Mr. Griffith's *Paper "upon the true Order of Succession of the Older Stratified Rocks near Killarney and Dublin." By* Charles William Hamilton, *F.G.S. &c.*[*]

THERE are a few points in this paper to which I think it necessary to reply.

I brought forward the result of my own observations with diffidence, because I felt that they had been few and imperfect; but I think they are not useless, and that I have, as I then hoped, "pointed out an interesting field of inquiry, and helped to show that a large portion of Ireland is still a subject for Geological debate;" and besides, I am still convinced that my observations, as far as they went, were not inaccurate, as Mr. Griffith asserts, although his subsequent observations may have shown that some of my conclusions were so.

Mr. Griffith has represented the fault which is observable at Brickeen Island (L. & E. Phil. Mag. vol. xvi. Pl. 2. fig. 2.) as involving an upcast of about *three thousand feet*, and the beds of chloritic slate, quartz-rock, and old red sandstone as cropping out at a low angle on the south-eastern slopes of Glena mountain, at the base of which the fault is to be seen.

Now in this case I am thoroughly convinced that my representation is correct and Mr. Griffith's incorrect; and as it is a point easily accessible to any geologist who may visit Killarney, I do hope that some one will take the trouble of comparing the two observations. I believe that the succession of the strata is in exactly the reversed order, that there is an anticlinal on the summit of Glena mountain, that the strata on the south-eastern slopes dip to the S.E. at an angle approaching very nearly to the perpendicular, and that consequently the slates at the back of Lady Kenmare's cottage are in the *highest* and not the *lowest* part of the series there developed.

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[*] Communicated by the Author.

Mr. Griffith attaches much importance to the difference of strike upon the opposite sides of the fault. I have made a great many careful observations, and although until we get a correct map it would be impossible to lay these down so as to draw any correct conclusions from them, I may observe that the observations taken at the same side of the fault vary quite as much among themselves as they do with those on the other side.

In p. 173 Mr. Griffith says, "it appears to me that Mr. Hamilton is mistaken in separating the old red sandstone from the Devonian system;" but the very quotation he makes explains clearly that I made no such separation, but referred the compact arenaceous rocks overlying the coarse red conglomerate to the *upper part* of the Devonian system.

I shall not occupy your pages with other points in which I am still hardy enough to rely upon my own observations in opposition to so high an authority as my friend Mr. Griffith; but I could not leave these unnoticed, because I wish to press upon the attention of geologists the fact that the difficulties of this district have not yet been cleared away, and that Mr. Griffith is premature in referring to the Silurian epoch, that vast depth of sandstone and conglomerate which occurs between the bays of Kenmare and Castlemaine, and the whole section between Foillatarriv and Brandon; I have before expressed my opinion (Journal of the Geological Society of Dublin, vol. i. p. 282.) that this latter section bears the strictest analogy to that between the Bangor quarries and Llyn Schal, and I see no reason for retracting that opinion.

To Mr. Weaver's remarks and insinuations of "an unrestrained indulgence of fancy," &c., I shall not reply; his objections to the possibility of the old red sandstone dipping in one place to the south, and another to the north, seem hardly to require an answer; and the correctness of his observations has been already subjected to a sufficiently rigorous inquiry by Mr. Griffith.

XLI. *On the Heat of Vapours and on Astronomical Refractions.* *By* John William Lubbock, *Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from vol. xvi. p. 569.]

On the Vapours of Æther, Alcohol, Petroleum, and Oil of Turpentine.

THE following Table is extracted from a valuable paper by Dr. Ure in the Phil. Trans. for 1818.*

Table of the pressure of the vapours of æther, alcohol, petroleum or naphtha, and oil of turpentine.

Æther.		Alcohol sp. gr. 0·813.		Alcohol sp. gr. 0·813.		Petroleum.	
Temp. τ	Pressure.	Temp. τ	Pressure.	Temp. τ	Pressure.	Temp. τ	Pressure.
Fahr.	Inch.	Fahr.	Inch.	Fahr.	Inch.	Fahr.	Inch.
34°	6·20	32°	0·40	193·3°	46·60	316°	30·00
44	8·10	40	0·56	196·3	50·10	320	31·70
54	10·30	45	0·70	200	53·00	325	34·00
64	13·00	50	0·86	206	60·10	330	36·40
74	16·10	55	1·00	210	65·00	335	38·90
84	20·00	60	1·23	214	69·30	340	41·60
94	24·70	65	1·49	216	72·20	345	44·10
104	30·00	70	1·76	220	78·50	350	46·86
		75	2·10	225	87·50	355	50·20
2nd.	Æther.	80	2·45	230	94·10	360	53·30
		85	2·93	232	97·10	365	56·90
105°	30·00	90	3·40	236	103·60	370	60·70
110	32·54	95	3·90	238	106·90	372	61·90
115	35·90	100	4·50	240	111·24	375	64·00
120	39·47	105	5·20	244	118·20		
125	43·24	110	6·00	247	122·10	Oil of Turpentine.	
130	47·14	115	7·10	248	126·10		
135	51·90	120	8·10	249·7	131·40	Temp.	Pressure.
140	56·90	125	9·25	250	132·30		
145	62·10	130	10·60	252	138·60		
150	67·60	135	12·15	254·3	143·70	304°	30·00
155	73·60	140	13·90	258·6	151·60	307·6	32·60
160	80·30	145	15·95	260	155·20	310	33·50
165	86·40	150	18·00	262	161·40	315	35·20
170	92·80	155	20·30	264	166·10	320	37·06
175	99·10	160	22·60			322	37·80
180	108·30	165	25·40			326	40·20
185	116·10	170	28·30			330	42·10
190	124·80	173	30·00			336	45·00
195	133·70	178·3	33·50			340	47·30
200	142·80	180	34·73			343	49·40
205	151·30	182·3	36·40			347	51·70
210	166·00	185·3	39·90			350	53·80
		190	43·20			354	56·60
						357	58·70
						360	60·80
						362	62·40

[* Or Phil. Mag., First Series, vol. liii. p. 95.]

From these observations I take the following data for æther :—

$$p = 1 \qquad \theta = 73^\circ$$

$$p' = \frac{99{\cdot}10}{30} \qquad \theta' = 143^\circ$$

$$p'' = \frac{166{\cdot}00}{30} \qquad \theta'' = 178^\circ$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0{\cdot}1523534].$$

Hence for æther

$$\beta = -{\cdot}03153 \qquad \gamma = 1{\cdot}0325 \qquad E = {\cdot}67086.$$

For alcohol

$$p = 1 \qquad \theta = 141{\cdot}0^\circ$$

$$p' = \frac{97{\cdot}10}{30} \qquad \theta' = 200{\cdot}0^\circ$$

$$p'' = \frac{166{\cdot}10}{30} \qquad \theta'' = 232{\cdot}0^\circ$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0{\cdot}1830354].$$

$$\beta = {\cdot}04025 \qquad \gamma = {\cdot}96131 \qquad E = 1{\cdot}55796.$$

For petroleum

$$p = 1 \qquad \theta = 284^\circ$$

$$p' = \frac{46{\cdot}86}{30} \qquad \theta' = 318^\circ$$

$$p'' = \frac{64}{30} \qquad \theta'' = 343^\circ$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0{\cdot}2259762].$$

$$\beta = -0{\cdot}6268 \qquad \gamma = 1{\cdot}0668 \qquad E = {\cdot}35294.$$

For oil of turpentine

$$p = 1 \qquad \theta = 272^\circ$$

$$p' = \frac{45}{30} \qquad \theta' = 304^\circ$$

$$p'' = \frac{62 \cdot 40}{30} \qquad \theta'' = 330^\circ$$

$$\frac{p''^{\beta} - 1}{p'^{\beta} - 1} = [0 \cdot 2441091].$$

$$\beta = -\cdot 1816 \qquad \gamma = 1 \cdot 2219 \qquad E = -\cdot 73937.$$

And hence for the vapour of æther

$$\tau = \frac{[2 \cdot 2601058]}{p^{-\cdot 03153} - \cdot 67086} - 448^\circ.$$

For the vapour of alcohol

$$\tau = \frac{[2 \cdot 5396942]}{p^{\cdot 04025} - 1 \cdot 57796} - 448^\circ.$$

For the vapour of petroleum

$$\tau = \frac{[2 \cdot 6940380]}{p^{-\cdot 06268} - \cdot 35294} - 448^\circ.$$

For the vapour of oil of turpentine

$$\tau = \frac{[3 \cdot 1166099]}{p^{-\cdot 1816} + \cdot 73937} - 448^\circ.$$

The temperature being reckoned in Fahrenheit's scale and the pressure in atmospheres.

Mr. E. Russell has calculated for me the following table, showing how far the above formulæ represent the observations of Dr. Ure. The results are exhibited in the plate annexed, and it will be seen that the discrepancies between the theory here suggested and the results of observations are chiefly owing to the irregularities of the latter, which arise doubtless from the great difficulties incidental to such experiments. When the pressures are small, the variation of temperature becomes great for a small variation of pressure, so that the agreement of theory with observation may be considered as complete, even if the absolute amount of the error of the calculated temperature is then more considerable.

Æther.			Alcohol.			Sp. gr. 0·813.			Petroleum.		
Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.	Pressure.	Temp. calc. τ	Error.
Inch.			Inch.			Inch.			Inch.		
6·20	30°·8	−3°·2	0·40	33°·6	+1°·6	46·60	193°·6	+0°·3	30·00*	316°·0	0°·0
8·10	42·2	−1·8	0·56	42·8	+2·8	50·10	197·1	+0·8	31·70	320·1	+0·1
10·30	52·9	−1·1	0·70	48·1	+3·1	53·00	199·9	−0·1	34·00	325·4	+0·4
13·00	63·5	−0·5	0·86	53·3	+3·3	60·10	206·3	+0·3	36·40	330·7	+0·7
16·10	73·6	−0·4	1·00	57·2	+2·2	65·00	210·3	+0·3	38·90	335·5	+0·5
20·00	84·2	+0·2	1·23	62·6	+2·6	69·30	213·7	−0·3	41·60	340·7	+0·7
24·70	94·9	+0·9	1·49	67·8	+2·8	72·20	215·8	−0·2	44·10	345·3	+0·3
30·00*	105·0	0·0	1·76	72·4	+2·4	78·50	220·3	+0·3	46·86*	350·0	0·0
32·54	109·3	−0·7	2·10	77·4	+2·4	87·50	226·2	+1·2	50·20	355·4	+0·4
35·90	114·7	−0·3	2·45	81·9	+1·9	94·10	230·2	+0·2	53·30	360·2	+0·2
39·47	119·9	−0·1	2·93	87·3	+2·3	97·10*	232·0	0·0	56·90	365·4	+0·4
43·24	125·0	0·0	3·40	91·8	+1·8	103·60	235·6	−0·4	60·70	370·7	+0·7
47·14	129·8	−0·2	3·90	96·1	+1·1	106·90	237·4	−0·6	61·90	372·3	+0·3
51·90	135·4	+0·4	4·50	100·7	+0·7	111·24	239·8	−0·2	64·00*	375·0	0·0
56·90	140·8	+0·8	5·20	105·4	+0·4	118·20	243·3	−0·7			
62·10	145·9	+0·9	6·00	110·2	+0·2	122·10	245·2	−1·8			
67·60	151·0	+1·0	7·10	116·0	+1·0	126·10	247·1	−0·9			
73·60	156·2	+1·2	8·10	120·7	+0·7	131·40	249·6	−0·1			
80·30	161·6	+1·6	9·25	125·5	+0·5	132·30	250·0	0·0			
86·40	166·2	+1·2	10·60	130·5	+0·5	138·60	252·8	+0·8			
92·80*	170·8	+0·8	12·15	135·6	+0·6	143·70	255·9	+1·6			
99·10	175·0	0·0	13·90	140·8	+0·8	151·60	258·3	−0·3			
108·30	180·8	+0·8	15·95	146·3	+1·3	155·20	259·8	−0·2			
116·10	185·4	+0·4	18·00	151·1	+1·1	161·40	262·2	+0·2			
124·80	190·2	+0·2	20·30	156·1	+1·1	166·10*	264·0	0·0			
133·70	194·9	−0·1	22·60	160·7	+0·7						
142·80	199·5	−0·5	25·40	165·7	+0·7						
151·30	203·5	−1·5	28·30	170·4	+0·4						
166·00*	210·0	0·0	30·00*	173·0	0·0						
			33·50	178·0	−0·3						
			34·73	179·6	−0·4						
			36·40	181·8	−0·5						
			39·90	186·1	+0·8						
			43·20	189·9	−0·1						

Oil of Turpentine.		
Pressure.	Temp. calc. τ	Error.
Inch.		
30·00*	304°·0	0°·0
32·60	310·5	+2·9
33·50	312·7	+2·7
35·20	316·6	+1·6
37·06	320·6	+0·6
37·80	322·2	+0·2
40·20	327·1	+1·1
42·10	330·8	+0·8
45·00*	336·0	0·0
47·30	340·0	0·0
49·40	343·4	+0·4
51·70	347·0	0·0
53·80	350·2	+0·2
56·60	354·3	+0·3
58·70	357·1	+0·1
60·80	359·9	−0·1
62·40*	362·0	0·0

The observations marked with an asterisk are those which were employed in procuring the constants β, E.

The æther which was observed by Dr. Ure below the pressure of 30 inch. appears to have been slightly different in quality from the other; in estimating the comparison this circumstance should be borne in mind.

ON THE CONDITIONS OF THE ATMOSPHERE, AND ON THE CALCULATION OF HEIGHTS BY THE BAROMETER.

The same principles are applicable to the constitution of the atmosphere; but we are far from possessing such extensive and satisfactory data for testing the accuracy of the formulæ. The best observations for this purpose are those of M. Gay Lussac, recorded by M. Biot in the *Connaissance des Temps* for 1841, in the following table.

Table des observations par ordre de hauteurs barométriques.

Numéro des observations par ordre de pression.	Températures en degrés du thermomètre centésimal.	Moyenne des indications des deux hygromètres.	Hauteur moyenne du baromètre dans l'atmosphère, ramenée à celle d'un baromètre à niveau constant.	Hauteurs correspondantes au-dessus de l'Observatoire de Paris, calculées par la formule barométrique de M. Laplace.
			m	m
1	+30°·75	57°·5	0·76568	0·00
2	12·50	62·0	0·5381	3032·01
3	11·00	50·0	0·5143	3412·11
4	8·50	37·3	0·4968	3691·45
5	10·50	33·0	0·4905	3816·79
6	12·00	30·9	0·4666	4264·65
7	11·00	29·9	0·4626	4327·86
8			0·4528	4511·61
9	8·75	29·4	0·4528	4511·61
10	8·25	27·6	0·4404	4725·90
11	6·50	27·5	0·4353	4808·74
12	5·25	30·1	0·4229	5001·85
13	1·00	33·0	0·4141	5175·06
14	4·25	27·5	0·4114	5267·73
15	2·50	32·7	0·3985	5519·16
16	0·00	35·1	0·3918	5631·65
17	+ 0·50	30·2	0·3901	5674·85
18	– 3·00	32·4	0·3717	6040·70
19	– 1·50	32·1	0·3696	6107·19
20	– 3·25	33·9	0·3670	6143·79
21	– 7·00	34·5	0·3339	6884·14
22	– 9·50		0·3288	6977·47

I shall employ the 1st, 5th, and 21st observations for the determination of the constants, and I propose then to calculate with these constants the temperatures corresponding to the intermediate observations. As the pressures are proportional to the heights of the barometer, if the variation of gravity be neglected we may take the heights of the barometer to represent them, and we have

$$\begin{array}{lll} p = \cdot 76568 & \theta = 30^{\circ}\cdot 75 & \dfrac{1}{\alpha} = 266\cdot 67 \\ p' = \cdot 4905 & \theta' = 10^{\circ}\cdot 50 & \dfrac{1}{\alpha} + \theta' = 277\cdot 17 \\ p'' = \cdot 3339 & \theta'' = -\ 7^{\circ}\cdot 00 & \dfrac{1}{\alpha} + \theta'' = 259\cdot 67 \end{array}$$

I find

$$\frac{\left(\frac{p''}{p}\right)^{\beta} - 1}{\left(\frac{p'}{p}\right)^{\beta} - 1} = \frac{(\theta'' - \theta)\left(\frac{1}{\alpha} + \theta'\right)}{(\theta' - \theta)\left(\frac{1}{\alpha} + \theta''\right)}$$

$$= [0\cdot 2988164].$$

The quantity between brackets being the logarithm of the corresponding number

$$\beta = -\ \cdot 32931 \qquad \gamma = 1\cdot 4910$$

$$E = \frac{\left(\frac{p''}{p}\right)^{\beta}\left(\frac{1}{\alpha} + \theta''\right) - \left(\frac{1}{\alpha} + \theta\right)}{\theta'' - \theta}$$

$$= -\ 1\cdot 1618 \qquad H = \cdot 53772$$

$$\tau = \frac{[2\cdot 8081857]}{p^{-\cdot 32931} + 1\cdot 1618} - 266^{\circ}\cdot 67$$

in the centigrade scale, the pressure corresponding to $\cdot 76568^{m}$ of mercury in the barometer being unity. In Fahrenheit's scale,

$$\tau = \frac{[3\cdot 0634582]}{p^{-\cdot 32931} + 1\cdot 1618} - 448^{\circ},$$

the pressure corresponding to 30·14 inches of mercury being unity.

If we take $\gamma = 1\cdot 5$, assuming the 21st observation of M. Gay Lussac, $E = -\ 1\cdot 1920$.

The difference in the results obtained with these constants from those obtained with the other system of constants $\gamma = 1\cdot 4910$ and $E = -\ 1\cdot 1618$, is quite insignificant, only changing the density slightly in the fifth place of decimals. By taking $\gamma = 1\cdot 5$

$$\varrho' = \varrho\,(1 - q)^{2}\,(1 - H\,q),$$

so that the expression for the density becomes more simple, consisting of only three terms, c^{-3u}, c^{-2u}, c^{-u}, (as will be seen hereafter), which is advantageous in the theory of astronomical refractions.

Mr. Russell has calculated for me the following table, by means of my expressions, and with the constants

$$\gamma = 1{\cdot}5, \quad E = -1{\cdot}192:$$

No.	Observed pressure p.	Calculated height in miles.	Temperature τ.		Density ρ.
			Calculated.	Observed.	Calculated.
1	0·76568	0·000	+30°·75	+30°·75*	·99999
2	·5381	1·895	14·74	12·50	·74276
3	·5143	2·131	12·68	11·00	·71514
4	·4968	2·310	11·10	8·50	·69473
5	·4905	2·376	10·52	10·50*	·68736
6	·4666	2·632	8·24	12·00	·65928
7	·4626	2·676	7·85	11·00	·65457
8					
9	·4528	2·785	6·87	8·75	·64299
10	·4404	2·926	5·61	8·25	·62828
11	·4353	2·985	4·99	6·50	·62222
12	·4229	3·130	3·67	5·25	·60744
13	·4141	3·236	2·80	1·00	·59692
14	·4114	3·269	2·61	4·25	·59346
15	·3985	3·428	1·05	2·50	·57818
16	·3918	3·512	0·28	0·00	·57010
17	·3901	3·533	+ 0·08	+ 0·50	·56805
18	·3717	3·772	− 2·12	− 3·00	·54576
19	·3696	3·800	− 2·37	− 1·50	·54321
20	·3670	3·835	− 2·70	− 3·25	·54004
21	·3339	4·295	− 7·00	− 7·00*	·49947
22	·3288	4·369	− 7·70	− 9·50	·49317

The observations marked with an asterisk are those which were employed in deducing the constants.

The temperatures calculated by Mr. Russell from my formula may be considered as identical with the "température régularisée par la continuité", given by M. Biot in the *Conn. des Temps*, 1841, p. 13. The observations* of M. Gay Lussac of temperature are represented in the following diagram:

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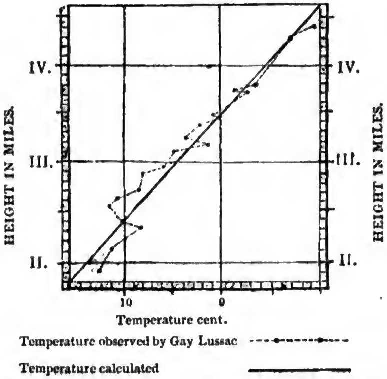
* The irregularities of the observations of temperature in any future ascent might perhaps be diminished if the ballast were suffered to escape gradually in a continued stream.



The abscissa represents the temperature in degrees of the centigrade thermometer, and the ordinate the height in miles.

At first the decrements of temperature are nearly equal for equal increments of altitude. These observations by no means furnish so good a criterion of the accuracy of my formula as the observations which have been made of the temperature of steam and other vapours. The determination of the constants γ and E for the atmosphere must be repeated at some future time; for it is obvious that no great reliance can be placed upon the extreme precision of the values now obtained* until other ascents have been made, and many similar observations have been compared together. We may then hope to obtain constants accurately appertaining to a mean state of the atmosphere, and the variations which take place in their values corresponding to fluctuations of the temperature; the pressure and the humidity of the atmosphere at the earth's surface may then be investigated. M. Biot has suggested that balloons furnished with self-registering instruments should be moored over each of the principal observatories of Europe. This plan appears to me subject to great difficulties. The weight of the line attached will diminish the buoyancy, so that I apprehend it will be found impossible to send up a balloon so fastened to any considerable altitude. The escape of gas will, I imagine, render it very difficult to maintain the balloon at the same height for any length of time. The height of the balloon will also be subject to great variations from the tension of the line changing with the force and direction of the wind. I am disposed to attach much greater value to well-regulated ascents, in which every effort should be made to reach the highest possible altitude, and of course simultaneous observations should be made at the earth's surface at such short intervals of time that every observation of the aëronaut may be comparable with a similar observation at the surface of the earth. As, however, the density and temperature of the atmosphere above the height of five miles from the earth's surface can never be the subject of direct experiment and observation, the observations which can be made upon the conditions of steam and other vapours will always maintain an indirect importance from the light which they throw upon the conditions of the atmosphere. I do not think that an examination of observations made in aërostatic ascents will ever furnish a sure guide to the relation sought between the temperature and the pressure, although if such a relation is furnished by theory and corroborated by observations of other vapours, (which can be carried through a greater extent of the thermometric scale, and, above all, through the low pressures where the variations of temperature become more rapid,) the obervations of aëronauts may serve to determine with sufficient accuracy the constants involved in the formula for atmospheric air.

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* Dulong found, for atmospheric air, perfectly dry, $\gamma = 1{\cdot}421$. See Poisson, *Méc.*, tom. ii. p. 646.

The following table*, calculated by Mr. Russell, shows the density and temperature of the air at different altitudes, calculated by means of my expressions and with the constants

$$\gamma = 1{\cdot}5, \quad E = -\,1{\cdot}192:$$

Height in miles.	Pressure p.		Temperature τ.		Density ϱ.
	Metres.	Inches.	Cent.	Fahr.	
0	0·76568	30·145	+ 30°·75	87°·35	1·00000
1	·63718	25·087	22·43	72·37	·85614
2	·52741	20·764	13·83	56·89	·73038
3	·43399	17·086	+ 4·94	40·89	·62066
4	·35480	13·968	− 4·23	24·39	·52515
5	·28799	11·336	13·71	+ 7·32	·44223
6	·23189	9·130	23·50	− 10·30	·37042
7	·18506	7·286	33·60	28·48	·30844
8	·14621	5·756	44·05	47·29	·25512
9	·11420	4·496	54·82	66·68	·20942
10	·08806	3·467	65·96	86·73	·17042
15	·01806	0·711	127·32	197·18	·05034
20	·00125	0·049	199·38	326·88	·00720
24	·00000	0·000	−265·95	−446·71	·00000

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* In calculating this table, the law of Marriotte and Gay Lussac, expressed by the equation

$$p = k\,\varrho\,(1 + \alpha\,\theta),$$

has been implicitly supposed to hold good throughout: this of course is only conjectural, and it is not intended to attach precision to the temperatures assigned to the great altitudes.

[To be continued.]

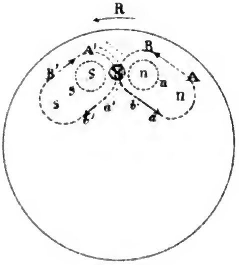


Fig 2.

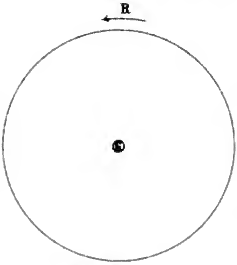


Fig 4.

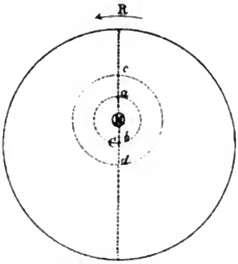


Fig. 5.

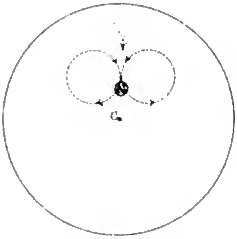


Fig. 6.

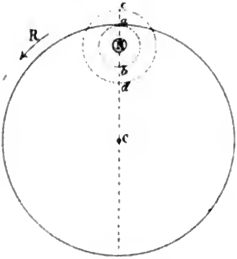


Fig. 9.

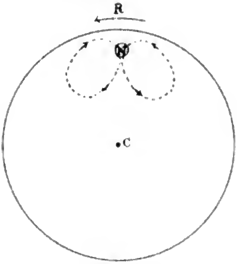


Fig. 10.

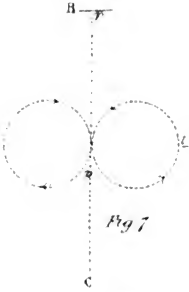


Fig 7.

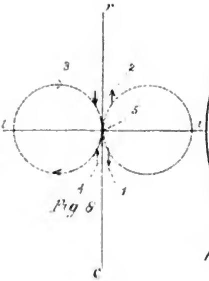


Fig 8.

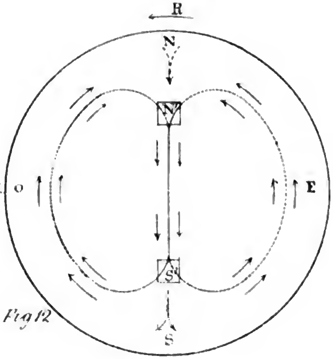


Fig 12.

Fig 3.

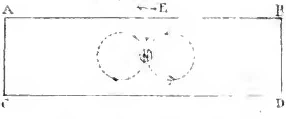


Fig 1.

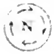
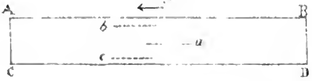


Fig. 11.



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Mr. Faraday on Magneto-electric Induction.

XLII. *On Magneto-electric Induction; in a Letter to M.* Gay-Lussac. *By* MICHAEL FARADAY, *D.C.L., F.R.S.*[*]

MY DEAR SIR,

I BEG to address to you the following pages upon the subject of electro-magnetism, and request the favour of their insertion in the *Annales de Chimie et de Physique.* They may, I fear, provoke a controversy that I would willingly avoid; but under the existing circumstances I feel compelled to adopt the present course of proceeding, for silence, should I maintain it, would be regarded as an admission of error, not only in a philosophical, but also in a moral point of view, from which I believe myself wholly exempt.

You will undoubtedly understand that I allude to the Memoir by Messrs. Nobili and Antinori. I address myself to you, because your judgement was sufficiently favourable to my former memoir for it to obtain a place in your excellent and truly philosophical Journal; and because Messrs. Nobili and Antinori's memoir being also inserted, the *Annales* contain all that has been written upon the subject. I therefore venture to hope that you will not refuse to admit the present article.

On the 24th of November, 1831, my first memoir, which you did me the honour to insert in the *Annales* for the month of May, 1832 (p. 5—69.), was read before the Royal Society; and it was the first announcement that I made of my researches in electricity.

On the 18th of December, 1831, I addressed a letter to my friend M. Hachette, which he was pleased to communicate to the Academy of Sciences on the 26th of the same month[†].

[* Translated from the *Annales de Chimie et de Physique*, vol. li. p. 404. Mr. Faraday, in the preface to his collected "Experimental Researches in Electricity," published last year, and reviewed in L. and E. Phil. Mag., vol. xiv. p. 468, refers to several papers of his own, long since published, in the following terms. "Before concluding these lines I would beg leave to make "a reference or two; first, to my own papers on Electro-magnetic Rota- "tions in the Quarterly Journal of Science, 1822, xii. 74. 186. 283. 416., "and also to my Letter on Magneto-electric Induction in the *Annales de* "*Chimie*, li. p. 404. These might, as to the matter, very properly have "appeared in this volume, but they would have interfered with it as a "simple reprint of the 'Experimental Researches' of the Philosophical "Transactions." As the papers here alluded to are now scarce, and as one of them has appeared in the French language only, we propose to transfer them in succession to our pages, in order that the purchasers of Mr. Faraday's volume may be enabled to possess, in a small compass, the entire series of his researches in electricity and magnetism. We begin with the letter on Magneto-electric induction, addressed to M. Gay-Lussac.—EDIT.]

[†] Vide the *Lycée*, No. 35.

It was also inserted in the *Annales* for December, 1831 (p. 402.). My second series of Researches, bearing date of 21st of December, 1831, was read before the Royal Society on the 12th of January, 1832, and may be found in the *Annales* for June, 1832 (p. 113—162.). These are my only publications upon this subject, up to the present time, excepting a few notes appended to memoirs by other authors; and the whole were written, and publicly read, anterior to any publication on the same subject, by any individual whatever.

During this time, my letter to M. Hachette, which was inserted in the *Annales*, attracted the attention of Messrs. Nobili and Antinori, and those laborious philosophers published a memoir, of the date of 31st of January, 1832, and which was thus posterior to all my publications. This memoir is contained in the *Annales* for December, 1831 (p. 412—430.). A second memoir, entitled "*New Electro-magnetic Experiments*," by those gentlemen, dated 24th of March, 1832, appeared in the *Annales* for July (p. 280—304.).

My letter to M. Hachette, which in his kindness to me he read before the Academy of Sciences, has, I fear, become a source of error and misunderstanding, and has been productive of injury, rather than benefit, to the cause of philosophic truth. At the same time I know not how to explain my meaning, and place the facts in their proper light, without having the air of complaining in a manner of Messrs. Nobili and Antinori, which to me is particularly disagreeable. I respect those gentlemen, on account of all they have done, not in relation to electricity alone, but for the cause of science in general; and were it not that the contents of their memoirs oblige me to speak, and leave me only the alternative of admitting or denying the exactitude of their assertions, I should have passed unnoticed the scientific errors discoverable in them, leaving to others the task of animadversion. These gentlemen had, unfortunately, no further knowledge of my researches than they gathered from my short letter to M. Hachette, and without taking the trouble to refer to my memoirs, which in these circumstances I cannot but think they ought to have done, they at once misinterpret the sense of an expression relating to M. Arago's beautiful observations, assume that I had not previously done that for which they take credit to themselves; and finally, they advance what to me appear to be fallacious ideas upon the magneto-electric currents, and present these ideas as *corrections* of mine, though with mine they were as yet unacquainted.

First, allow me to rectify what I regard as the most important mistake of all, the false interpretation given of my words, for the correction of the errors committed in the experiments might have been left to time.

Messrs. Nobili and Antinori write (*Annales*, vol. xlviii. p. 428.), "Mr. Faraday considers M. Arago's magnetism of rotation to be entirely connected with the phænomenon which he discovered ten years ago. *He ascertained* THEN, *according to the statement in the notice, that by the rotation of a metallic disc under the influence of a magnet, electric currents may be produced in the direction of the radii of the disc in sufficiently considerable quantities to render the disc a new electrical machine.* We are entirely ignorant how Mr. Faraday discovered this fact, and we know not how a result of this nature *could remain* SO LONG *generally unknown, and, so to speak,* FORGOTTEN in the hands of the discoverer," &c.

Now *I never said* what is here imputed to me. In my letter to M. Hachette, quoted at the head of the notice, I gave a short account of what I had recently discovered, and read before the Royal Society on the 24th of the preceding month. This notice may be found at page 402 of the same number of the *Annales*, and is as follows: "The fourth part of the memoir treats of M. Arago's equally curious and extraordinary experiment, which, as is known, consists in making a metallic disc revolve under the influence of a magnet. Mr. Faraday considers the phænomenon which is manifested in this experiment as *intimately connected* with that of magnetic rotation which he was so fortunate as to observe ten years ago. *He has ascertained that by the rotation* of the metallic disc under the influence of a magnet, electric currents *may be* formed in the direction of the radii of the disc, in sufficient number to render the disc a new electrical machine."

I never either said, or intended to say, that I had obtained these electric currents by the rotation of a metallic disc, at an epoch previous to the date of the memoir that I was then engaged in writing; but I said that the extraordinary effect discovered by M. Arago was connected in its nature with the electro-magnetic rotation I had discovered several years before, both being due to a tangential action; and that by the rotation of a disc near a magnet I could (at the time I was writing) cause currents of electricity to escape, or have a tendency to escape in the direction of the radii, thus rendering the disc a new electrical machine; and this I think is fully proved in the part of my memoir of which I gave a sketch: it may be seen in the *Annales*, vol. l. p. 65—118.

I am extremely desirous of explaining this error, because I have always admired M. Arago's prudence and philosophic reserve, in resisting the temptation to give a theory of the effect which he had discovered until he could offer one perfectly applicable, and in refusing his assent to the imperfect theories of others. Admiring him I adopted his reserve in this respect, and for that reason, perhaps, had my eyes open to recognise the truth as soon as it was presented.

We now arrive at that part of the subject which relates to the philosophy of my memoirs. The fourth part of my memoir of the 24th of November, 1831, contains my opinion on the cause of the phænomenon discovered by M. Arago, an opinion that even now I see no reason to alter. Messrs. Nobili and Antinori, in their papers of the 31st of January and 24th of March, 1832, animadvert upon certain errors which they attribute to me, and enter upon extended developments of magneto-electric phænomena. I cannot, however, discern that they have added a single fact to those contained in my memoirs, unless it be the obtaining of a spark with a common magnet, a result that I had myself previously obtained, but only with the electro-magnet. On the other hand, these gentlemen's memoirs appear to me to contain erroneous ideas upon the nature of magneto-electric currents; they exhibit also mistaken views as to the action and direction of those currents in Arago's revolving disc. They say, "*We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher*," &c. (*Annales*, vol. l. p. 281.) And again at page 298, commenting on what they *suppose* to be my ideas (for though my papers had been read, and were published, they had not thought proper to consult them), they say, "We have already given our opinion upon this idea, but if at the commencement of our researches it were difficult to reconcile it with the nature of the currents discovered by M. Faraday, what can we say after all the new observations that we have made during the progress of our investigations? We say that we have a competent judge in the galvanometer, and that by its means the question must be decided."

With the most sincere desire to be set right when I am in error, I yet find it impossible to discover any corrections in the memoirs of these gentlemen by which I can profit; but I fully admit the competency of the galvanometer, and shall proceed as briefly as possible to submit our different ideas to its decision, in all that relates to Arago's phænomenon: and I am at the present time so satisfied with the facts and results stated in my published memoirs (though were I to rewrite them I should make alterations in some parts), that it will not be necessary to refer to any experiments that are not therein contained.

It is not my intention further to animadvert upon Messrs. Nobili and Antinori's first memoir. An English translation of it appeared in the Philosophical Magazine*, to which I added some corrections in the form of notes, copies of which I had the honour of sending to you, and to the authors. My present object is to compare the second part of their publications with the fourth part of my first memoir, and with parts of the other memoirs, as a means of throwing light upon the general principles. The intention of the two articles is to explain Arago's phænomenon, and as fortunately they are both contained in the fiftieth volume of the *Annales*, they may be referred to with facility. The reference to my own papers will be thus, (F. 114.), and to those of Messrs. Nobili and Antinori by a simple indication of the page of the *Annales*.

At page 281, after a few general remarks, we read, "We have recently verified, extended, and perhaps corrected in some particulars the results of the English philosopher; *we then said* that magnetism of rotation found a real support in the new facts developed by Mr. Faraday, and that, consequently, the theory of such magnetism then *appeared* to be so far advanced as fully to merit an effort to develope the physical principles upon which it depends. *It is to this object that the present article is devoted*," &c. Upon this extract I shall only remark, that exactly four months previously I had said the same thing, in the memoir that I read before the Royal Society, and had given, what I hope will prove, a true and exact explanation of the philosophy of the effect under consideration (F. 4—80.).

At page 282 we read, "We have already noticed these currents in our first researches, that is, in the first paper inserted in the Number for December" (p. 412.). But I had "already noticed these currents" four months earlier (F. 90.).

At page 283 are described "galvanometrical explorers or probes," which are nothing more than what I had previously described under the name of *collectors* or *conductors* (F. 86, &c.).

At the commencement of the investigation of the state of Arago's revolving disc adjacent to a magnet, two relative positions of the plate and the magnet are chosen; one called (p. 284.) the "*central arrangement*," in which the magnetic pole is placed vertically to the centre of the disc; and the other (p. 285.) the "*excentric arrangement*," in which the magnet acts beyond that point.

[* Phil. Mag. and Annals, N.S. vol. xi. p. 401.]

With regard to the *central arrangement*, we read (p. 284.), "In this case the magnet acting upon the centre of the disc, the probes do not transmit any indication of a current to the galvanometer, *let them be placed where they may*; and if by chance small deviations should be remarked, they arise from imperfect centralization, so that if this *defect be corrected* all indications, &c. of an equivocal source immediately disappear. Indeed what is the result if we employ an electro-dynamic spiral which turns quite round on its own centre, always opposite to the same magnetic pole? *Absolutely nothing.* Its revolution is an *unimportant circumstance*; for the formation of the currents is *wholly due to another condition, they being manifested* only at the moment when the spirals are *brought near* to the magnets, or *removed* from them. So long as the spirals *are present*, whether they *move or not*, *there is no current*: so also *there is none* in the case of central rotation in which the points of the disc remain constantly at the same distance from the magnetic pole, by renewing thus the combination of continued presence, to which Mr. Faraday's *new laws* in relation to currents DO NOT ASSIGN ANY EFFECT."

This assertion is so erroneous in every part, that I have been obliged to quote the passage at full length. In the first place, *there is a tendency* to the formation of currents of electricity in the revolving disc, in the case of "central arrangement," as well as in every other case (F. 149—156.); but their direction is from the centre to the circumference, or *vice versâ*, and it is to these parts that the collectors should be applied. It is precisely this which renders the revolving disc a new electrical machine (F. 154.), and it is upon this point that Messrs Nobili and Antinori are so entirely mistaken in their two memoirs. This error is repeated throughout the whole of the memoir that I am now comparing with my first paper, which, if I mistake not, contains the theory of Arago's phænomenon in all its parts.

At page 284 we find, that when a helix turns upon its axis concentrically with a magnetic pole, the result is *absolutely nothing*, and that the condition of rotation is unimportant. Now, though I have not made any experiments on the subject, I venture to assert that there will be an effort in the electric current to pass in a transverse direction to the helix, and that the circumstance of its rotation, instead of being unimportant, is in these cases the only condition essentially requisite for the production of currents. The helix, in fact, may be considered as analogous to a cylinder which might occupy its place, but to which it is very inferior, as it consists of a long coil of wire. It may also be regarded as a simple wire placed in any situation occupied by a cylinder, and I have shown that they produce currents in their state of rotation, if their opposite extremities are connected with the galvanometer.

It is said at page 284, that the formation of currents is "*wholly due to another condition*, they being manifested only at the moment when the spirals are brought near to the magnets, or removed from them. So long as the spirals are present, whether they move or not, *there is no current*. So also there is none in the case of *central rotation*," &c. Now in my first paper I showed that the essential condition was not the approximation or removal of the metal in movement, but simply that it should intersect the magnetic curves (F. 101. 116. 118. &c.); and that consequently, *cæteris paribus*, the movement without change of distance is the most effective and powerful means of obtaining the current, instead of being the condition in which the result is absolutely nothing. In my second paper I *proved* that a movement through the magnetic curves was the only condition necessary (F. 217.); and that so far from the approximation or removal of the metal being necessary, currents may be produced in the magnet itself, merely by moving it in the proper direction (F. 220.).

Lastly, when treating of this "central arrangement," and the supposed absence of effect when "the points of the disc remain constantly at the same distance from the magnetic pole," Messrs. Nobili and Antinori say (p. 285.), "by thus renewing the combination of continued presence to which Mr. Faraday's *new laws* in relation to currents *do not assign any effect*;" and in a note we read, "These laws are reduced to three," which are specified, at first fully, and then in a more condensed form, as follows: "FIRST LAW. During gradual approximation: the current produced contrary to the current producing; repulsion between the two systems. SECOND LAW. The distance unvarying. No effect. THIRD LAW. During recession. The current produced in the same direction as the current producing. Attraction between the two systems." I have never myself given these as the simple laws which govern the production of the currents that I was so fortunate as to discover; nor do I understand how Messrs. Nobili and Antinori can say that they are *my* laws, though at page 282 one of them is so called. But I described these three cases together in my first memoir (F. 26. 39. 53.), as well as in the notice, that is, in my letter to M. Hachette, as effects that I had observed. It has been established, by what I have already said, that they are not the laws of the action of magnetic electricity, for the simple fact that currents of electricity may be obtained by means of the revolution of a cylinder (F. 219.), or of a disc in connection with a magnet (F. 218.), or of the magnet itself (F. 220.), disproves each of them. ONE LAW, which includes all the effects, is given in my memoir (F. 114. 116. &c.), and it simply expresses the direction in which the moving conductor intersects the magnetic curves. This law of direction being given, I endeavoured to recapitulate the whole in the terms that I shall here repeat (F. 118.).

"All these results show that the power of inducing electric currents is circumferentially excited by a *magnetic resultant* or *axis of power*, just as circumferential magnetism is dependent upon and is exhibited by an electric current."

I have quoted this passage of the Italian physicists at full length, because it contains nearly all our points of difference, both as to fact and opinion, concerning this part of the subject. Having thus shown all the errors included in it, I shall endeavour to be more concise, while I exhibit, *with the assistance of the galvanometer*, such others, derived from them, as are dispersed over the remainder of the memoir. It is indeed curious to remark, how, with galvanometrical indications generally correct, these gentlemen have suffered themselves to be led astray under the influence of preconceived opinions. For example, at page 287—288, and in fig. 2. plate iii. is shown the result of an examination by the galvanometer of the currents in a revolving disc. These currents are indicated nearly correctly by means of arrows; but the *two consequences* deduced from them agree with the theory enunciated, and are diametrically opposed to the facts.

"The immediate inspection of the arrows which mark the currents in the two regions of the disc (fig. 2.) leads to one of these consequences (p. 287.), and it is *that a system of currents is developed upon the parts that enter contrary to those produced on the other side*. The other consequence arises from comparing the currents produced upon the disc with the currents of the producing cause, and it is *that the direction of the currents upon the parts that enter is contrary to that of the producing currents, while on the other side the direction in the two systems is identical*."

But I showed in my first memoir (F. 119.), that "when a piece of metal is passed either before a single pole, or between the two opposite poles of a magnet, or near electromagnetic poles, whether ferruginous or not, electric currents are produced across the metal transverse to the direction of motion. This fact is proved by means of wires (F. 109.), plates (F. 101.) and discs (F. 92. &c.); and in all these cases the electric current was in the *same direction*, whether the metal were brought near to, or caused to recede from the magnet, provided that the direction of its movements were unaltered. In Arago's revolving disc the electricity that I was able to obtain from one of these parts in a multitude of experiments always agreed with these results (F. 92. 95. 96.), and consequently (F. 119. &c.) I recapitulated them in a short description, as presented in Arago's disc, establishing more particularly (F. 123.), that the currents produced near or under the poles are discharged or return into the parts of the metal situated on each side of and more distant from the place of the pole, where the magnetic induction is necessarily "weaker."

[To be continued.]

XLIII. *On the Detection and Estimation of Colophony (common Rosin) when dissolved in the Fixed Oils.* *By* J. Denham Smith, *Esq.*

To Richard Phillips, Esq., F.R.S., L. & E., &c.

My dear Sir,

SOME samples of linseed oil were sent to me for analysis in July last, the bulks of which had been exported, but were found on their arrival to be unsaleable and perfectly useless, for when mixed with white-lead in the usual manner for making paint, the mixture became quite hard at the expiration of a few hours, it having set as plaster of Paris does when moistened with water.

At first sight it was obvious that all these oils had been considerably adulterated; for not only was their colour much deeper than that of ordinary linseed oil, but they were all extremely viscid, resembling castor oil, in this respect, much more than the comparatively thin and fluid commercial linseed oil. Suspecting both from smell and taste, particularly the latter, that the adulterant was common colophony (black rosin), I endeavoured to ascertain whether my conjecture was well-founded, and if so, to determine the proportion of colophony contained in the several samples; especially as the adulterated oils were likely to become the subject of legal proceedings.

I am not aware that there is any mode on record for separating, or even detecting common rosin when dissolved in the fixed oils, so that I was obliged to make numerous experiments, before a method was discovered which appeared to offer satisfactory results. It would be useless to describe these unsuccessful trials; I therefore at once proceed to explain the mode I ultimately adopted for detecting and estimating the proportion of colophony contained in the various specimens of oil.

I found that when colophony was dissolved in pyroxylic spirit, the solution gave a bulky white precipitate, on the addition of acetate of lead also dissolved in pyroxylic spirit; but that when unadulterated commercial linseed oil was digested with this solvent, the clear solution when cold was merely rendered turbid on the addition of the spirituous solution of acetate of lead. Discovering that pyroxylic spirit exerted a very marked solvent action on the precipitate above-mentioned, and that the supernatant, or filtered solution, gradually deposited more of this precipitate, after standing for some time or when gently evaporated, I had recourse to the common rectified spirit of wine, about sp. gr. ·832, which although it is capable of dissolving this compound of resin and oxide of lead, yet does so in almost an inappreciable quantity, especially if the spirit be cold.

Having dissolved 30 grs. of common rosin in a small portion of linseed oil, by the assistance of heat, about 3 fluid ounces of rectified spirit of wine were poured upon the oil, and thoroughly mixed with it by agitation; this was boiled for two or three minutes, and then allowed to cool and the oil to subside; the next day the clear spirituous solution was poured off, about the same quantity of fresh spirit added to the residual oil, and the mixture agitated and boiled as before; when bright this solution was decanted and mixed with the former, the remaining oil again treated with about an ounce of spirit, and this solution added to the others; a fourth time spirit was added, but this solution gave no precipitate with acetate of lead; this agent merely rendering it turbid, as in the case of pure linseed oil when dissolved in spirit; from this I concluded that the whole of the resin, which the rectified spirit was capable of separating, was extracted from the oil. The three solutions when mixed were of a light yellow colour, and perfectly clear, when on the addition of a freshly prepared solution of acetate of lead in rectified spirit the characteristic bulky white precipitate fell. At the expiration of four-and-twenty hours this was collected on a filter weighing 11·7 grs., and washed with cold rectified spirit until the washing left the merest perceptible stain when evaporated in a porcelain dish; I then dried the precipitate on folds of bibulous paper, and finally with a gentle heat, until it suffered no decrease of weight, when the weight of the precipitate and filter was found to be 29·4 grs. —11·7 grs., weight of filter, = 17·7 grs. of the compound of resin and oxide of lead obtained from 30 grs. of common colophony, which would indicate that 59 grs. of this compound are equal to 100 grs. of rosin.

This experiment was repeated with 40 grs. of rosin dissolved in a little oil, and this mixture treated in the manner above described. In this instance 26·7 grs. of the precipitate were obtained, which is equal to 66·7 per cent. of the resin employed. It appears, from these experiments, that although this method is incapable of affording strictly accurate results, I presume from the slight solvent action of the spirit upon the precipitate, yet that an approximation to the quantity of colophony contained in oils adulterated with this substance, may be obtained, which perhaps by some subsequent modifications of and precautions in the process, may lead to a mode capable of yielding not merely a tolerable approximation, but rigidly accurate results.

When unadulterated commercial linseed oil is treated with rectified spirit in the manner described, the alcoholic solution affords no precipitate on the addition of a spirituous solution of acetate of lead, but it merely becomes turbid, exhibiting appearances very different from the bulky precipitate which is produced if colophony be present. The samples of adulterated oil when submitted to the process I have described, respectively afforded 27·7, 21 and 26·3 per cent. of this compound of resin and oxide of lead, which would indicate, according to the average of the two experiments with resin and oil already mentioned, 44·1, 33·4 and 41·7 per cent. of common rosin respectively contained in these specimens of oil.

When this white precipitate is suspended in rectified spirit, and a current of hydrosulphuric acid gas passed through it, decomposition takes place, sulphuret of lead is formed, and a light yellow-coloured solution, which reddens litmus, is obtained; this, on evaporation, leaves a brown and brittle residuum exactly resembling common rosin. For this mode of separating the acid resin from the oxide of lead, I am indebted to my friend Dr. Brett. From the circumstance of this alcoholic solution affording no precipitate with nitrate of silver until the addition of a little ammonia, I conclude the resin acid which is combined with the oxide of lead in the precipitate obtained in the foregoing experiments, to be the sylvic acid. When ignited in a covered crucible, so as to avoid the access of atmospheric air, and consequent oxidation of the lead, I find that the precipitate, whether obtained from the two first experiments, or whether reduced from the precipitates afforded by the adulterated oils submitted to examination, produces precisely the same quantity of metallic lead, viz. 27 per cent. Both these precipitates also, when exposed to a moderate heat, fuse, affording a brown transparent substance very much resembling common colophony, but which seems to be harder and more brittle than rosin is. Pure linseed oil, when mixed with the same quantity of rosin as analysis indicated in one of the samples of adulterated oil marked "Raw oil," that containing 44·1 per cent., was exactly of the same density as the sample in question, both being ·982, whilst pure linseed oil was considerably lighter, its density being ·9518; thus corroborating not merely the fact of adulteration, but also the close approximation to correct results afforded by the mode of analysis adopted.

I remain, my dear Sir, yours very truly,

J. DENHAM SMITH.

Duke-Street, Liverpool,
Sept. 3, 1840.

XLIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 149.]

April 30, 1840.— THE following communications were read:—

1. A Letter from Sir John Barrow, Bart., V.P., addressed to the President, accompanying a series of Magnetic Observations made on shore, and on board Her Majesty's ships 'Erebus' and 'Terror,' under the direction of Captain James Clark Ross, R.N., together with a Series of Observations made on the temperature and specific gravity of the ocean at various depths, and at the surface, namely,

"Observations of the magnetic intensity on shore, and on board H.M.S. Erebus, with needle F. 1.

"Magnetic dip observations on shore, and on board H.M.S. Erebus, with needle F. 1.

"Observations for the magnetic dip on shore, and on board H.M.S. Terror.

"Observations of the magnetic dip by needle F. C. 5. on shore, and on board H.M.S. Terror.

"Observations in magnetic intensity by needle F. C. 5. on shore, and on board H.M.S. Terror."

The whole of these observations are up to the 31st December, 1839. They are transmitted to the Royal Society from the Lords Commissioners of the Admiralty.

2. Postscript to Major Sabine's paper, entitled "Contributions to Terrestrial Magnetism," which was read at the meeting of March 19th; (see p. 144), containing an extract from a letter from Capt. James Clark Ross, commanding the Antarctic expedition, dated from St. Helena, February 9th, 1840; noticing the success which had

attended the employment of Mr. Fox's instrument, in observations of the magnetic dip and intensity on shipboard.

3. "A few remarks on a Rain Table and Map," drawn up by Joseph Atkinson, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The table and map which accompany this paper exhibit the average annual depth of rain falling in different places in Great Britain.

4. "Extracts from a Meteorological Journal kept at Allenheads, in the county of Northumberland," by the Rev. W. Walton, F.R.S.

The general result of these observations, which were recorded twice each day, namely, at 9 A.M., and at 3 P.M., during the whole of the year 1839, is, that the mean temperature taken at those times was 44° 8′; the mean height of the barometer, corrected and reduced to the temperature of 32°, was 28·401 inches, and the quantity of rain in the year was 55·71 inches. The author subjoins several remarks on the conclusions deducible from an examination of the tables.

5. "Description of an Astronomical Clock invented by the late Captain Henry Kater, F.R.S.," drawn up from his own memorandums by his son Edward Kater, Esq. Communicated by Sir John F. W. Herschel, Bart., V.P.R.S.

The great object aimed at by Captain Kater in the construction of the escapement of a chronometer, is to communicate equal impulses to the pendulum through some principle perfect in itself, and not dependent for its success on superior execution. In the escapement invented by him, the pendulum merely raises a weight, and is impelled by that weight through an increased space in its descent. It neither unlocks a detent, nor has anything to do with the train; and as the weight raised, and the spaces described, are constant quantities, this escapement is, in the strict meaning of the term, one of equal impulse.

May 7.—A paper was read, entitled "Researches in Embryology, Third Series: a Contribution to the Physiology of Cells." By Martin Barry, M.D., F.R.S., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Of this paper an abstract was given in our number for June, vol. xvi., p. 526.

A paper was also read, entitled "On the Odour accompanying Electricity, and on the probability of its dependence on the presence of a new substance;" by C. F. Schœnbein, Professor of Chemistry, Bâle, communicated in a letter to Michael Faraday, Esq., D.C.L., F.R.S., &c.

The author's attention having been long directed to the remarkable fact, that odour, resembling that of phosphorus, is given off during the escape of positive electricity from the point of a conductor into air; and is likewise perceptible when lightning has struck any object, and also when water is electrolyzed, he has investigated the circumstances attending these phenomena; and the results he has obtained will, he expects, afford a clue to the discovery of their cause.

The odour which accompanies the electrolyzation of water, he observes, is only disengaged at the positive electrode. He also finds that the odoriferous principle can be preserved in well-closed glass bottles for any length of time. The only metals which yield this odour are gold and platina; but dilute sulphuric, phosphoric, and nitric acids, and from aqueous solutions of several of the salts, also disengage it. Raising the temperature of the fluid to the boiling point prevents the odour from arising; and the addition of comparatively small quantities of powdered charcoal, iron, zinc, tin, lead, antimony, bismuth or arsenic, or of a few drops of mercury, to the odorous principle contained in a bottle, immediately destroys the smell; and the same happens when platina or gold, heated red-hot, is introduced into the vessel containing that volatile substance.

May 14.—A paper was read, entitled, "Tables of the Variation, through a cycle of nine years, of the mean height of the Barometer, mean Temperature, and depth of Rain, as connected with the prevailing Winds, influenced in their direction by the occurrence of the Lunar Apsides, with some concluding observations on the result." By Luke Howard, Esq., F.R.S., &c.

From the Tables here given, the author draws the following conclusions:—

1. The barometer is higher under the lunar apogee, than under the perigee; the mean height in the former case being 29·84517, and in the latter, 29·75542.

2. The mean temperature is lower under the apogee than under the perigee; that of the former being 48°·7126, and of the latter, 49°·0356. The mean of the whole year was 48°·7126.

3. The rain of the weeks following the apsis exceeds that under the perigee; but with two striking exceptions in the annual result of nine years, the one in the wettest, and the other in the driest year of the cycle.

With regard to the winds, the author remarks that those from the north, north-east, and east, prevailed under the apogee on 38 days, under the perigee on 21 days; and those from the south, south-west, and west, prevailed under the apogee on 20 days, under the perigee on 38 days.

It appears, therefore, that in the climate of London, the moon in her perigee brings over us the southern atmosphere, which tends to lower the density and raise the temperature of the air, occasioning also a larger precipitation of rain. In the apogee, on the contrary, there is a freer influx of air from the northward, a higher barometer, a lower temperature, and less rain; subject, however, to a large addition of rain under this apsis twice in a cycle of nine years, at the times when also the extremes of wet and dry take place on the whole amount of the year.

A paper was also read entitled, "Experimental Researches into the strength of Pillars of Cast Iron, and other materials." By Eaton Hodgkinson, Esq. Communicated by Peter Barlow, Esq., F.R.S., &c.

The author finds that in all long pillars of the same dimensions, the resistance to crushing by flexure is about three times greater when the ends of the pillars are flat, than when they are rounded. A long uniform cast-iron pillar, with its ends firmly fixed, whether by means of disks or otherwise, has the same power to resist breaking as a pillar of the same diameter, and half the length, with the ends rounded, or turned so that the force would pass through the axis. The strength of a pillar with one end round and the other flat, is the arithmetical mean between that of a pillar of the same dimensions with both ends round, and one with both ends flat. Some additional strength is given to a pillar by enlarging its diameter in the middle part.

The author next investigated the strength of long cast-iron pillars with relation to their diameter and length. He concludes that the index of the power of the diameter, to which the strength is proportional, is 3·736. He then proceeds to determine, by a comparison of experimental results, the inverse power of the length to which the strength of the pillar is proportional. The highest value of this power is 1·914, the lowest, 1·537, the mean of all the comparisons, 1·7117. He thus deduces, first, approximate empirical formulæ for the breaking weight of solid pillars, and then proceeds to deduce more correct methods of determining their strength.

Experiments on hollow pillars of cast iron are then described, and formulæ representing the strength of such pillars are deduced from these experiments.

After giving some results of experiments still in progress for determining the power of cast-iron pillars to resist long-continued pressure, the author proceeds to determine from his experiments the strength of pillars of wrought iron and timber, as dependent on their dimensions. The conclusion for wrought iron is, that the strength varies inversely as the square of the pillar's length, and directly as the power 3·75 of its diameter, the latter being nearly identical with the result obtained for cast iron; for timber, the strength varies nearly as the 4th power of the side of the square forming the section of the pillar. Experiments for determining the relation of the strength to the length in pillars of timber, were not instituted, as, from the great flexure of the material, it was considered that no very satisfactory conclusions on this point could be derived experimentally.

In conclusion, the author gives the relative strengths of long pillars of cast iron, wrought iron, steel, and timber.

May 21.—The following papers were read, viz.:

1. " Remarks on the Meteorological Observations made at Alten, Finmarken, by Mr. S. H. Thomas in the years 1837, 1838, and 1839." By Major Sabine, R.A., V.P.R.S., and Lieut. Col. Sykes, F.R.S.; being a Report from the Committee of Physics, including Meteorology, to the Council, and communicated by the Council to the Royal Society.

These observations, made at Alten in lat. 69° 58′ 3″ N., and 23° 43′ 10″ east of Paris, would seem to have a claim to the attention of the Royal Society, as they offer the *experimentum crucis* of

Professor Forbes's empirical formula respecting the gradual diminution of the daily oscillations of the barometer, within certain limit hours, from the equator to the poles. Professor Forbes has laid down an assumed curve, in which the diurnal oscillation amounts to ·1190 at the equator and 0 in lat. 64° 8′ N., and *beyond that latitude* the tide should occur *with a contrary sign*, plus becoming minus. Now Alten being nearly in lat. 70°, if Professor Forbes's law hold good, the maxima of the diurnal oscillations should occur at the hour for the minima at the equator, and a similar inversion should take place with respect to the minima. Mr. Thomas has himself however modified the value his observations would otherwise have had, by adopting 2 P.M., instead of 3 P.M., for the hour of his observations for the fall; and he has adapted his barometrical observations to a mean temperature of 50° Fahr., instead of 32°. The first year's observations commence on the 1st October, 1837, and terminate on the 30th September, 1838. The barometer stood 66 feet 5 inches above low-water mark, and the thermometer hung at 6 feet above the ground; but care was not always taken to prevent the sun shining on it. The mean height of the barometer for the year was 29°·771, and the mean of the thermometer almost coincident with the freezing point, viz., 32°·017. The maximum height of the barometer was 30°·89 in January, and the minimum 28°·71 in October. The mean of the barometer at 9 A.M. was 29°·764, therm. 33°·455; at 2 P.M. 29°·765, therm. 33°·327; and at 9 P.M. 29°·784, therm. 29°·270. The diurnal observations would seem to support Professor Forbes's theory; but the 9 P.M. observations are entirely opposed to it, as they appear with the same maximum sign as at the equator, whereas the sign ought to have been the reverse; indeed, with respect to the diurnal observations, the mean of five months of the year at 9 A.M. gives a plus sign, although the mean of the year at 2 P.M. only gives the trifling quantity of ·001 plus. There is one remarkable feature in these observations that cannot fail to strike the meteorologist. M. Arago, from nine years' observations at Paris, reduced to the level of the sea, makes the annual mean height 29°·9546; twenty-one years' observations at Madras make it 29°·958; and three years' observations at Calcutta, by Mr. James Prinsep, make it 29°·764; and Mr. Thomas brings out 29°·771. That there should be this coincidence between the observations at Calcutta and Alten is curious. Neither Mr. Thomas nor Mr. Prinsep state whether or not their means are reduced to the level of the sea. It is to be suspected they are not.

For the next year, that is to say, from Oct. 1838 to Sept. 1839, both inclusive, Mr. Thomas uses a French barometer and French measurements, with centigrade thermometer attached to the barometer, and Fahrenheit's for the detached thermometer. He changes his time of observation from 9 A.M. to 8 A.M., 2 P.M., and 8 P.M., and he reduces his barometrical observations to 0 centigrade. The results of the year are as follow:—mean annual pressure 29°·627 English; thermometer Fahr. 33°·36; greatest pressure in April, least in January!! The mean of 8 A.M. is 29°·620; therm. 33°·75. The mean of 2 P.M. is 29°·631; therm. 34°·73; and at 8 P.M. 29°·631; therm. 30°·57. The diurnal observations assist to support Professor Forbes's theory; but as in the preceding year, the P.M. observation is at fault; and if the hour had been 9 o'clock instead of 8 o'clock, it would probably have been more so than it appears. The low annual mean state of the barometer for the year 1837–38 is even increased in the last year's observations; and as fresh instruments* appear to have been used, there is ground to believe that the fact is associated with the locality, and it may be desirable not only to record in the Proceedings of the Royal Society the data already supplied, but to recommend to Mr. Thomas more particular inquiry on the subject.

The phænomena of the Aurora Borealis appear to have been observed by Mr. Thomas with great assiduity, and recorded with great care. On examining the register, with reference to M. Erman's important remark, that "in Siberia two kinds of aurora are distinguished, one having its centre in the west, and the other in the east, the latter being the more brilliant," it is found that twenty-two nights occur in the course of the two winters in which the formation of arches of the aurora is noticed and their direction recorded; of these, *ten* are to the *west*, having their centres rather to the southward of west, the arches extending from N.W. to S.S.E. and S.E.; *seven* are to the *east*, or more precisely to the southward of east, the arches extending from N.E. to S.E. and S.W. Of the five others, *four* are said to be from east to west across the zenith, and cannot therefore be classed with either of the preceding, and *one* is noticed generally as being to the north. The facts here recorded appear to afford an evidence of the same nature as those mentioned by M. Erman, as far as regards there being two centres of the phænomena. In respect to the relative brilliancy of the eastern and western aurora, nothing very decided can be inferred from the register. If, as M. Erman supposes, they may be referred respectively to "les deux foyers magnétiques de l'hemisphère boréal," it is proper to notice that the position of Alten is nearly midway between those localities.

There can be no doubt that the frequent appearance of the aurora, and the peculiarities of the phænomena observed there, render it a most desirable quarter for a magnetical and meteorological observatory.

EDWARD SABINE.
W. H. SYKES.

2. "Second Letter on the Electrolysis of Secondary Compounds, addressed to Michael Faraday, Esq., D.C.L., F.R.S., &c." By J. Frederic Daniell, Esq., For. Sec. R.S., Professor of Chemistry in King's College, London.

The author, in this letter, prosecutes the inquiry he had commenced in the former one, [of which an abstract appeared in the

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* It appears that the barometer was compared before leaving France, and subsequently to its being taken back to that country.

L. & E. Phil. Mag., vol. xv., p. 317.—Edit.] into the mode in which the chemical elements group themselves together to constitute *radicles*, or proximate principles. He considers his experiments as establishing the principle that, considered as electrolytes, the inorganic oxy-acid salts must be regarded as compounds of metals, or of that extraordinary compound of nitrogen and four equivalents of hydrogen to which Berzelius has given the name of *ammonium*, and compound anions, chlorine, iodine, &c., of the Haloide salts; and as showing that this evidence goes far to establish experimentally the hypothesis originally brought forward by Davy, of the general analogy in the constitution of all salts, whether derived from oxy-acids or hydro-acids. Some remarks are made on the subject of nomenclature, and the rest of the paper is occupied with the details of the experiments, all bearing on the important subject which he has undertaken to investigate.

May 28.—The following papers were read, viz.:

1. "Meteorological Register kept at Port Arthur, Van Diemen's Land, during the year 1838, and Register of Tides at Port Arthur, from August 1838 to July 1839, both inclusive." By Deputy-Assistant-Commissary-General Lempriere. Communicated by Sir John Franklin, R.N., F.R.S., &c.

2. "Notice relative to the form of the Blood-particles of the *Ornithorhynchus hystrix*." By John Davy, M.D., F.R.S.

A portion of the blood of the *Ornithorhynchus hystrix*, mixed when fresh with a strong solution of common salt, being examined by the author, exhibited a few globules of irregular shape. Another portion, preserved in syrup, contained numerous globules, most of which had an irregular form, but many were circular; none, however, were elliptical, like those of birds. Hence the author concludes, that in form they accord more with those of Mammalia.

3. "Researches on Electro-chemical equivalents, and on a supposed discrepancy between some of them and the atomic weight of the same bodies, as deduced from the theory of isomorphism." By Lieut.-Colonel P. Yorke. Communicated by Michael Faraday, Esq., D.C.L., F.R.S., &c.

The author describes various experiments made with a view to determine the electro-chemical equivalents of sodium and potassium. Three experiments gave, respectively, 22·3, 22·9, and 25, as the equivalent of the former; and two other experiments gave, respectively, 45 and 41·7, as the equivalent of the latter of these substances. He then inquires what would be the result of the electrolyzation of the aqueous solutions of soda and potash, on the hypothesis of these bodies being composed of two equivalents, or atoms, of metal, and one of oxygen. To determine this question he employs a solution of dichloride of copper in muriatic acid, as being a substance composed of two atoms of metal and one of an electronegative element. Its electrolysis gave as the equivalent of copper, 52·8, 59·4, and 61·6, numbers approximating closely to 63·2, or double the atomic weight of copper. After a long train of investigation, he concludes that there is no reason deducible from the theory of isomorphism for doubting the correctness of the received atomic weights of silver, sodium, &c., but that the difficulty, or anomaly, if it may be so called, should be considered as attaching itself to the di-compounds of copper; and that Faraday's propositions on this subject remain unimpeached.

4. "Second series of Approximate Deductions made from about 50,000 observations taken during the years 1836, 1837, and 1838, at the Port Louis Observatory, Mauritius, four times each day; namely, at 8 A.M., at noon, and at 4 and 8 P.M." By J. A. Lloyd, Esq., F.R.S.

5. "On the Solubility of Silica by Steam; with an account of an experiment on the subject, conducted in the East Indies by Julius Jeffreys, late of the Hon. East India Company's Medical Establishment."

The inner surfaces of a flue built of siliceous bricks appeared to be deeply eroded by the passage over it of steam at a very high temperature, and fragments of siliceous materials laid in the course of the current were partially consumed. A siliceous crust was deposited on several vessels of stone ware, coated with a micaceous glaze, placed in the upper part of the furnace, and this crust was re-dissolved when the vessels were removed to a hotter situation in the same furnace. The author notices the experiments of Dr. Turner* and others, which failed in showing the solubility of silica by steam, in consequence, as he conceives, of the heat having not been sufficiently great to effect the solution.

June 4.—A paper was read, entitled, "Contributions to the Chemical History of Archil and of Litmus." By Robert Kane, M.D., M.R.I.A. Communicated by Francis Baily, Esq., V.P.R.S.

After a preliminary sketch of the labours of Heeren and of Robiquet in investigating the origin of the beautiful colouring materials termed *Archil* and *Litmus*, obtained from different kinds of colourless lichens, and their detection of the two proximate principles termed *erythrine* and *orceine*, the author states the object of the inquiries detailed in the present paper to be threefold; viz. first, to ascertain the primitive form of the colour-making substance in a given species of lichen, and trace the stages through which it passes before the coloured substance is developed; secondly, to determine the nature of the various colouring substances which exist in the archil of commerce; and thirdly, to examine the colouring materials of ordinary litmus. He finds in the lichen *Roccella tinctoria* the following bodies, either pre-existing in the plant, or formed during the processes employed for its analysis; 1. Erythryline; 2. Erythrine (the Pseudo-erythrine of Heeren); 3. Erythrine bitter; 4. Telerythrine; and 5. Roccelline (the Roccellic acid of Heeren). The properties and constitution of these substances are then described, and the chemical formulæ given, which are deducible from their respective analyses. The author finds the archil of commerce to consist essentially of three ingredients, namely, orceine, erythroleic acid, and

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[* See L. & E. Phil. Mag. vol. v. p. 297.—EDIT.]

azoerythrine; of each of the two former there exist two modifications, and there is, in addition, a yellow matter. After comparing his results with those obtained by Heeren, by an examination of the products evolved by his erythrine in contact with air and with ammonia, and stating reasons for some changes in nomenclature, the author gives the chemical formulæ resulting from his own analysis of these different substances.

His inquiries into the constitution of ordinary litmus, which form the last division of his subject, lead him to the conclusion that that substance contains the principles designated by him as Erythrolein, Erythrolitmine, Azolitmine, and Spaniolitmine; and that the colouring constituents of litmus are, in their natural condition, red; the blue substances being produced by combination with a base, which bases in that of commerce are lime, potass, and ammonia; and there is mixed up in the mass a considerable quantity of chalk and sand. The details of the analyses of these several substances, and the resulting chemical formulæ representing their constitution, are then given.

The concluding section of the paper is occupied by an inquiry into the decoloration of the bodies which exist in archil and in litmus. The latter of these, the author concludes, is reddened by acids, in consequence of their removing the loosely combined ammonia by which the blue colour is produced; and the so-called hydrogen acids liberate the colouring matter by their combining with the alkali to form bodies (either chlorides or iodides), with which the colouring matter has no tendency to unite. Hence it appears that the reddening of litmus is no proof that chloride of hydrogen is an acid, and that the double decomposition which occurs is the same in principle, whether hydrogen or a fixed metal come into play. After detailing the blanching effects of other deoxydizing agents on the colouring matter of litmus, and the action of chlorine on orceine and azolitmine, the author remarks, that in these actions chlorine is subjected to conditions different from those which determine the nature of the results with the generality of organic bodies, and that the displacement of hydrogen, so marked in other cases, does not exist in the class of substances under consideration; but that, in reality, the products of the bleaching energy of chlorine resemble in constitution the compounds of chlorine which possess bleaching powers.

A paper was also read, entitled, "On the Corpuscles of the Blood." By Martin Barry, M.D., F.R.S.

The author in the course of his researches in Embryology, detailed in his "third series," observed that some of the corpuscles of the blood undergo progressive alterations in their structure. The corpuscles so altered he believes to be of the same kind as those described by Professor Owen; and having found that the alterations in question terminate in a separation of the corpuscles into globules, he thinks this fact confirms the idea of Professor Owen—that the blood-disc undergoes spontaneous subdivision. The author farther observed, that the corpuscles of the blood, in certain altered states, undergo rapid and incessant changes of form, which cannot be traced to the action of neighbouring cilia. A corpuscle will sometimes assume the figure of an hour-glass, as if it were preparing to divide itself into two parts, but it instantaneously either regains its previous form, or assumes a new one. These motions are incessant, and so rapid, that it is not easy to catch and delineate any of the resulting forms; they are compared to the writhings of an animal in pain. The author has seen them in a rabbit, as late as two hours and a half after death, and thinks it probable that they may continue for a longer time, although, when under the microscope, they gradually and in a short time cease; the rapid changes of form, which are at first apparent, passing into gentle undulations, and being succeeded by an alternation of rest and motion*.

Should these facts be thought to confirm the opinion of John Hunter, that the blood "has life within itself," or "acquires it in the act of forming organic bodies," because its corpuscles in certain states exhibit "vital actions," still his assertion that "the red globules" are the least important part of the blood, will appear to have no just foundation.

The author finds that the phænomena attending what is called "vital turgescence" of the blood-vessels, depend not merely on an accumulation and stagnation of blood, but on changes in the condition of its corpuscles, which assume a more or less globular, or elliptical appearance resembling cells. Their interior is dark, from a great increase of red colouring matter which accumulates around a pellucid and colourless point, corresponding in situation to that of the central part of nuclei in other cases; and so completely do the corpuscles fill their vessels, that the fluid portion of the blood is excluded, and the corpuscles are compressed into polyhedral forms. This condition of the blood-corpuscles during vital turgescence of the vessels, the author thinks deserving of consideration, in connexion with many of the phænomena attending local accumulations of blood, both in health and in disease; and more especially with reference to increased pulsation, the exudation of colourless fluid, and the heat and redness of inflamed parts.

According to the views of the author, the formation and nourishment of organs is not effected merely by the fluid portion of the blood, for he has discovered that the cells which he showed in his "Third Series of Researches in Embryology†" form the chorion, are altered blood-corpuscles; and he has farther found that muscular fibre (that is, the future muscle-cylinder, not the fibril) is formed by the coalescence of cells, which also are derived from corpuscles of the blood. He has seen and figured every stage of transition, from the unaltered blood-corpuscle to the branched cells forming the chorion, on the one hand, and to the elliptical or oblong muscle-cells, on the other. The colour is not changed, except that the blood-corpuscles, when passing into cells for the formation of muscle, become of a much deeper red. There seems to occur in these an increase of red colouring matter.

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* [See a note on this subject by Dr. Barry, p. 157 of the present volume.—Edit.]

† [See Lond. and Ed. Phil. Mag. vol. xvi. p. 526.—Edit.]

Valentin, in describing the mode of the formation of muscle, had stated that globules approach one another and coalesce to form threads, which in many places have the appearance of a necklace, but subsequently lose the traces of division, and become cylinders. Schwann had conjectured that the globules just referred to—as having been observed by Valentin—are cells, and that these cells coalesce to form a secondary cell, that is, the muscle-cylinder. The author confirms the observations of Valentin and the conjectures of Schwann, with the addition, that the globules coalescing to form the muscle-cylinder are blood-corpuscles which have become cells. The fibrils appear to be subsequently formed within the cylinder, which thus becomes the muscular fasciculus. The medullary portion of the cylinder appears to be composed of the pellucid objects, one of which is contained within each altered blood-corpuscle. Some of these pellucid objects, however, continue to occupy a peripheral situation.

The author thinks it is not probable that muscular fibre and the chorion are the only tissues formed by the corpuscles of the blood; he is disposed rather to inquire, how many are the tissues which they do not form? Nerves, for instance, are known to arise very much in the same manner as muscle-cylinders; and epithelium-cells sometimes present appearances which have almost suggested to the author the idea that they were altered corpuscles of the blood.

Schwann had previously shown that "for all the elementary parts of organisms there is a common principle of development,"—the elementary parts of tissues having a like origin in cells, however different the functions of those tissues. The facts made known in the present memoir not only afford evidence of the justness of the views of Schwann, but they farther show that objects, such as the corpuscles of the blood, having all the same appearance, enter immediately into the formation of tissues which physiologically are extremely different. Some of these corpuscles arrange themselves into muscle, and others become metamorphosed into constituent parts of the chorion. But the author thinks it is not more difficult to conceive corpuscles having the same colour, form, and general appearance, undergoing transformations for very different purposes, than to admit the fact made known by two of his preceding memoirs,*—namely, that the nucleus of a cell, having a central situation in the group which constitutes the germ, is developed into the whole embryo, while the nuclei of cells occupying less central situations in the group, form no more than a minute portion of the amnion. It is known that in the bee-hive a grub is taken—for a special purpose—from among those born as workers, which it perfectly resembles until nourished with peculiar food, when its development takes a different course from that of every other individual in the hive.

The Society then adjourned over the Whitsun Recess, to meet again on the 18th of June.

GEOLOGICAL SOCIETY.

Feb. 1, 1840.—*Annual General Meeting.*

The President announced that the Wollaston Medal had been awarded to Prof. Dumont, of Liége, for his Memoir, Map, and Sections on the Geological Constitution of the Province of Liége, published in 1832; and one year's interest of the Wollaston Fund to Mr. James De Carle Sowerby, in order to facilitate the continuation of his researches in Mineral Conchology; Dr. Buckland, on presenting the Medal to Dr. Fitton, who had been requested by M. Dumont to receive it on his behalf, said:—

Dr. Fitton,

I am highly gratified that it has become my duty on the present occasion, to commit to your care as the Representative of our common friend, Professor Dumont, the Wollaston Gold Medal, which has been awarded to him by the Council of this Society for his Memoir on the Geological Constitution of the province of Liége published at Brussels in 1832.

The grounds of our tardy recognition in 1840, of the merits of a work published so long as eight years ago, are the same, that in 1830, prompted the Judges appointed by the Academy of Brussels, to select this Memoir as most worthy of the Prize then proposed by that Academy, for the best Geological description of the province which has formed the subject of M. Dumont's successful labours.

In the work thus doubly crowned, the Author has described the mineralogical and zoological characters of the rocks which occupy this district, and determined in minute detail, the relative places in order of succession, and the superficial extent of each subordinate division of the several formations. He has also illustrated the same by an accurately coloured Geological Map, and by coloured Sections, showing the general disposal of the strata in their original order of deposition, and the extraordinary derangements and disturbances that have subsequently thrown them into a state of almost inextricable confusion. In the execution of this work, M. Dumont has evidenced unusual powers of discriminating and accurate observation, combined with a high capacity of reducing the minutiæ of local details under the dominion of enlarged and masterly theoretical generalizations. Advancing at the early age of twenty one, to a task of gigantic labour, in a region where the unexampled disturbances, and almost incredible complexity of its component strata had baffled the sagacity of the most experienced geologists, this extraordinary youth at once withdraws the veil of confusion which had hitherto disguised the stratigraphical arrangements of his native province, and as it were, by an intuitive touch, reduces to order the entangled and almost incredible phænomena of dislocation, contortion, and inversion which had perplexed his predecessors in the same field of observation.

In addition to the scientific value of M. Dumont's exact and laborious researches, in illustrating a high and difficult problem in positive geology, his work assumes a place of great statistical and commercial importance, as describing the structure and contents of a rich and productive carboniferous district containing eighty-three beds of valuable coal; and its practical utility has been fully shown, by the fact of a second edition having been required to supply the demands of the landed proprietors, and persons practically interested in the operations and products of the coal mines.

The geological tribunal of Brussels, including the highly distinguished geologist Omalius d'Halloy, at once appreciated duly, and rewarded as they deserved, these brilliant discoveries; but the phænomena represented on M. Dumont's map and sections were so unusually complex and improbable, that the geologists of England could not but forbear to admit their reality, until it was fully confirmed by our personal examination, with the aid of that new light which M. Dumont's discoveries had thrown upon them. The result of such inquiry has been a full corroboration of M. Dumont's representations, and at this late hour we at length come forward with the homage of our tardy but sincere acknowledgements; a duty too long delayed, from the exercise of precaution in its administration, but for this very reason now become more urgent, when the grounds for conscientiously discharging it have passed the ordeal of severe and critical investigation. It is for this great work then on the geological constitution of the Province of Liége, such as in 1832 it issued from the hands of a young, and then unknown individual, and apart from any more recent attempts to identify the Belgian formations with those of England, that our Society has awarded to M. André Hubert Dumont their Gold Wollaston Medal for the present year; in testimony of their admiration of the almost precocious talents then displayed by him, and of their sense of his worthiness to fill the distinguished scientific position to which he is now advanced, as Professor of Mineralogy and Geology in the College of Liége*.

Dr. Fitton, on receiving the Medal from the hands of the President, said, that he had been requested by M. Dumont to express his great regret that unavoidable duties prevented his appearing in person on this occasion. M. Dumont's letter states with deep feeling his sense of the honour which the Geological Society of London has thus conferred upon him, and his hope that he may soon be enabled to come into England, for the purpose of extending his personal acquaintance with the members of this Society, and of being enabled, with the aid of their knowledge, to perfect the comparison of the ancient strata of Belgium with those of this country. The Society could not but anticipate great advantage to Geology from the application of M. Dumont's talents to the comparative inquiries to which his letter alludes.

On presenting the prize awarded to Mr. James De Carle Sowerby, Dr. Buckland said:—

It is with no small pleasure that I rise to perform the duty of placing into your hands the award that has been made to you by the Council of the Geological Society, of one year's interest of the Wollaston Fund, in order to facilitate the continuation of your researches in Mineral Conchology.—The services are great which have been rendered to Geology by the extremely useful and well-timed work on fossil shells, which was many years ago begun by your excellent father, and continued by him to the end of his life, and has been since conducted by yourself; and the association of his name with that of Dr. Wollaston, recalls to my mind, as it must to the minds of most of my hearers, pleasing and grateful recollections of the benefits which during their lives they both conferred on this Society, and which their works will have extended to all our contemporaries and successors in this department of scientific inquiry. It was your father's peculiar merit to be one of those accurate and enthusiastic observers of nature, who have in modern times contributed so much to remove from science the rugged and austere aspect under which it used to be presented; and who by facilitating to every one the means of advancing pleasantly in its pursuit, have, in an essential manner, promoted, and given popularity to the study of Botany and Conchology.

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* [A paper by M. Dumont "On the Equivalents of the Cambrian and Silurian Systems in Belgium," will be found in Lond. and Edinb. Phil. Mag., vol. xv. p. 146.—Edit.]

It is to Mineral Conchology, which he so especially promoted, that we who are occupied with the investigation of the structure of the earth, have in modern times been mainly indebted for evidences which have led to the establishment of many of the most important stratigraphical distributions, that have been founded on the successive changes in animated nature which are made known to us by the study of fossil shells. It was on this foundation that Cuvier and Brongniart established their important divisions of the marine and freshwater strata of the Tertiary formations, which have since been more minutely distributed by Mr. Lyell into the eocene, pliocene, and miocene series, according to their relative numbers of extinct and recent species of fossil shells. It was on a similar foundation that Mr. William Smith rested his identification of the Secondary strata of England. It is on the same basis of conchological evidence that Mr. Murchison has founded his fourfold subdivisions of the Silurian portion of the Transition rocks; and it is chiefly to the illumination which this branch of Palæontology has shed upon the changes that took place on the surface of the earth, whilst its strata were in process of formation, that we owe the rapid advances in geological knowledge which have been made since the commencement of the present century. To this rapid progress, arising from the introduction of the evidences of mineral Conchology, your own publications and those of your family have largely contributed; you have further co-operated materially in advancing our inquiries by your personal assistance, at all times cheerfully and liberally rendered, to all your fellow labourers in the same fields of scientific research, who stood in need of your aid, for the elucidation of minute distinctions in the characters of fossil organic remains, which have at this time become so important an element in geology. The volumes of the Transactions of this Society, and other publications by many of its Members, including myself, bear further testimony to the importance of your labours, in illustrating our works with drawings and engravings of fossil shells and plants, expressing their characters with a degree of accuracy and truth, which no pencil or burine but those of a scientific artist could possibly accomplish; and I am sure I give utterance to the feelings of all our fellows now around me, when I thus publicly acknowledge the services you have rendered both to ourselves, and to the science we cultivate; and express the satisfaction with which we thus publicly recognise the value of your exertions.

Mr. Sowerby then expressed himself in the following terms:—

SIR,

I hardly know what to say, so deeply do I feel the unexpected and kind award bestowed upon me by this Society, but I must assure you, that I am extremely grateful for the honour done me. When, Sir, you spoke of my father, you excited feelings most dear to me, and I have long felt that I have experienced more consideration than I have deserved, in consequence of the esteem that has ever been attached to his memory. But I must have been a most ungrateful son had I not, after his persevering and kind instructions, done something for the advancement of Natural History. What little I have performed, especially for Members of this Society, has been for the love of Science; and I feel far more than amply rewarded by the honourable present I have just received at your hands. You have stated, Sir, that you take a pleasure in associating the name of Wollaston with that of Sowerby; I shall never forget the kindness and patience with which Dr. Wollaston communicated information. When the reflective goniometer was first completed by him, he spent several hours one morning with me in his study measuring the cleavages of various minerals related to hornblende and augite which I took to him for his opinion; and at another time he indulged me with an equally long lesson on the chemical examination of minute portions of minerals. Little did I think at that time that I should ever share encouragement continued by his bounty, after his departure from this world; but I have lived to feel that his benevolence lives beyond the grave.

Sir, I receive this award as a trust reposed in me, and hope that I shall not be found wanting in carrying out the object the Council has in view.

I beg sincerely to thank the Society for the confidence placed in me.

The following Fellows were declared to have been elected the Officers and Council for the ensuing year.

President.—Rev. W. Buckland, D.D. Professor of Geology and Mineralogy in the University of Oxford.

Vice-Presidents.—G. B. Greenough, Esq. F.R.S. & L.S.; Leonard Horner, Esq. F.R.S. L. & E.; Sir Woodbine Parish, K.C.H. F.R.S.; Rev. William Whewell, B.D. F.R.S. Professor of Casuistry in the University of Cambridge.

Secretaries.—Charles Darwin, Esq. F.R.S.; William John Hamilton, Esq.

Foreign Secretary.—H. T. De la Beche, Esq. F.R.S. & L.S.

Treasurer.—John Taylor, Esq. F.R.S. & L.S.

Council.—Arthur Aikin, Esq. F.L.S.; Francis Baily, Esq. F.R.S. L.S.; Viscount Cole, M.P. F.R.S.; W. H. Fitton, M.D. F.R.S. L.S.; W. Hopkins, Esq. M.A. F.R.S.; R. Hutton, Esq. M.P. M.R.I.A.; Charles Lyell, Esq. F.R.S. L.S.; William H. Miller, Esq. M.A. Professor of Mineralogy in the University of Cambridge; R. I. Murchison, Esq. F.R.S. L.S.; E. W. W. Pendarves, Esq. M.P. F.R.S.; Philip Pusey, Esq. M.P. F.R.S.; George Rennie, Esq. F.R.S.; Daniel Sharpe, Esq. F.L.S.; Rev. Adam Sedgwick, F.R.S. L.S. Woodwardian Professor in the University of Cambridge.

Address to the Geological Society, delivered at the Anniversary, on the 21st of February, 1840, *by the* REV. PROFESSOR BUCKLAND, D.D., F.R.S., *Corresponding Member of the Institute of France, President of the Society.*

GENTLEMEN,

BY the Report just read, you have seen that the state of our Society is one of steady and salutary progression; forty-three new Members have been added to the List of our Fellows, from which seventeen have been removed by death or resignation, leaving our actual number 768, with an increase of twenty-six during the last year. The vacancies that have occurred upon our foreign list have been supplied by three highly distinguished cultivators of science on the Continent, each pre-eminent for his successful labours in high departments of our subject, namely:

Major Puillon de Boblaye, in Positive Geology,
Professor Adolphe Brongniart, in Vegetable Palæontology,
Professor Gustave Rose, in Crystallography and Mineral Analysis.

We are rich in property, though our funds are, at this moment, low, but they will speedily be repaired by the sale of two large and costly parts which have been added to our Transactions.

The Reports of the Library and Collections in our Museum are satisfactory. The chief additions to the former consist of presents from Authors and Members of the Society. Our principal benefactor has been Mr. Greenough, who has given us a Collection of the older Authors,—supplying many of our deficiencies in the Literature of Geology and Mineralogy. Considerable progress has been made in the arrangement of the Cabinets by our Sub-Curator, Mr. Woodward, under the superintendence and directions of Mr. Lonsdale; one hundred and sixty drawers of rock specimens and fossil remains having been labelled, and in part catalogued, since the meeting of last year. It is satisfactory to find that the number of persons who come to study our Collections has been much increased.

Our entire establishment continues to receive the inestimable advantages it has long enjoyed, from the zealous superintendence, and scientific acquirements of our Curator, Mr. Lonsdale.

Our Wollaston Medal has been awarded to Professor Dumont, for his Map, Sections and Memoir on the Geological Constitution of the Province of Liége, published in 1832; and one year's interest of the Wollaston Fund has been presented to Mr. James De Carle Sowerby, to facilitate the continuation of his researches in Mineral Conchology.

More than a quarter of a century has now elapsed since I became a Member of this Society; and fifteen years have passed since I was first placed, by your kindness, in the honourable position of filling this Chair, at that important period of our history when we received the national recognition of a Royal Charter. I shall never cease to consider it one of the brightest rewards of my labours in geology, that my name is enrolled in that charter, as the first President of the Society in its corporate capacity.

Since that important epoch, our chartered body has received from the Government of the country the valuable sanction and advantage of an establishment in the very convenient apartments of Somerset House, which we now occupy. The number and character of the scientific labourers who have joined our ranks, and the volumes added to our Transactions, since these events, show that such encouragements have not been conferred on a society disposed to slumber under the sunshine of prosperity; but that, aided by these advantages, we have endeavoured to maintain a steadily progressive course, in the great work of illustrating the physical structure of the earth.

It is not my duty, on the present occasion, to notice geological memoirs or subjects which belong to years preceding that wherein I entered upon my present office. The usual practice rather confines me to the most remarkable events of the last twelve months, during which I have had the honour to fill this chair.

MUSEUM OF ŒCONOMIC GEOLOGY.

Among the most important of these events, we recognise with gratitude, and confident anticipation of great advantage, both to science and the arts, the establishment, by Her Majesty's Government, of an institution hitherto unknown in England, namely, a Museum of Œconomic Geology. This is to be freely accessible to the public at stated periods, in the Department of Her Majesty's Woods and Forests, and Public Works, for the express object of exhibiting the practical application of geology to the useful purposes of life. In this Museum a large store of valuable materials has already been collected and arranged, chiefly by the exertions, and under the direction of Mr. De la Beche. In it will be exhibited examples of Metallic Ores, Ornamental Marbles, Building-stones and Limestones, Granites, Porphyries, Slates, Clays, Marls, Brickearths, and Minerals of every kind produced in this country, that are of pecuniary value, and applicable to the arts of life. Information upon such subjects, thus readily and gratuitously accessible, will be of the utmost practical importance to the miner and the mechanic, the builder and the architect, the engineer, the whole mining interest, and the landed proprietors. The establishment will contain also examples of the results of Metallurgic processes obtained from the furnace and the laboratory, with a collection of Models of the most improved machinery, chiefly employed in mining. A well-stored Laboratory is attached to this department, conducted by the distinguished analytical chemist, Mr. Richard Phillips, whose duty it already is, at a fixed and moderate charge, to conduct the analysis of metallic ores, and other minerals and soils submitted to him by the owners of mines or proprietors of land, who may wish for authentic information upon such matters.

The pupils in this laboratory are already actively employed in learning the arts of mineral analysis, and the various metallurgic processes.

A second department in the Œconomic Museum will be assigned to the promotion of improvements in Agriculture, and will contain sections of strata, with specimens of soils, sub-soils, and of the rocks from the decomposition of which they have been produced.

To this last-mentioned collection proprietors of land are solicited to contribute from their estates labelled examples of soils, with their respective sub-soils; and all persons who wish for an analysis of any sterile soil, for the purpose of giving it fertility, by the artificial addition of ingredients with which nature had not supplied it, may here obtain, at a moderate cost, an exact knowledge of its composition, which may point out the corrective additions which it requires. This portion of the Museum will more especially exhibit the relations of geology to agriculture, in so far as a knowledge of the materials composing the sub-strata may afford extensive means of permanent improvement to the surface.

[To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xvi. p. 148.]

Jan. 10, 1840.—The following communications were read:—

Ephemeris of the Comet now visible. By Mr. C. Rumker, of Hamburg. Communicated by Dr. Lee.

A Letter from Mr. Henry Lawson to the Secretary describing the appearance of the Comet, as seen at Hereford.

The comet was observed by Mr. Lawson on the mornings of the 23d and 29th of December, and of the 8th January instant. It had a tolerably well-defined nucleus, with a brushy tail on the side opposite to the sun. The nucleus subtended an angle nearly equal to half the visual angle subtended by *Jupiter*; and the tail filled the whole field of view, the diameter of which was three minutes of time.

Apparent Positions of the Comet observed at Edinburgh. By Professor Henderson.

Observations of the Comet made at Ashurst and Dulwich. By Robert Snow, Esq.

Mr. Snow found the comet on the 28th of December. The observed diameter of the head was then 58″, and the tail extended beyond the field of view. It was again observed on the 29th, and also on the 5th and 6th of the present month, when it was very bright and easily found. The nucleus was large, but not stellar.

Occultations of Stars by the Moon to the end of 1839. By Robert Snow, Esq.

Catalogue of the *Pleiades*. By Robert Snow, Esq.

The author states that this catalogue does not lay claim to strict accuracy, but was constructed in order to form a chart, which might be consulted with advantage when occultations of stars in the *Pleiades* by the moon take place. Piazzi's stars falling within the limits of the chart were taken as standards, and the differences between them and the other stars determined by a wire micrometer. For some stars, too faint to allow of illumination, the ring micrometer was used; but they were more usually put down by estimation, which may be done with nicety when many are in the field together. This communication was accompanied by a chart.

On the Variability of *α Cassiopeiæ*. By Robert Snow, Esq.

In the Monthly Notice for May last (vol. iv. p. 195.), the attention of the Society was directed to the supposed variability of this star; and it has, accordingly, been watched, with the naked eye, since June 9, 1839, up to the present time, January 8, 1840. The relative brightnesses of *α*, *β*, *γ Cassiopeiæ* have been registered on sixty-eight evenings. The result at present is that γ has been generally put down as brightest, and never faintest; β generally as faintest; α faintest twelve times out of sixty-eight, and generally so about the 5th day of the month. In the Society's Catalogue, the order of magnitude is β, $\alpha = \gamma$. Mr. Snow remarks that the star α appears to his eye at all times sharper and better defined than γ or β; and it is also more readily obscured by fog or haze, although it is a reddish star.

Observations of *α Cassiopeiæ* in 1831 and 1832. By Mr. W. R. Birt, Librarian and Assistant Secretary to the Metropolitan Institution. Communicated by the President.

These observations were commenced in April 1831, and extend to November 1832; and the earlier part of them, from April to December 1831, have already, with some others, been communicated to the Society. See Monthly Notices, vol. ii. No. 11. In a letter to Sir John Herschel, Mr. Birt states, that since 1832 his attention had not been directed to this star until he read the Monthly Notice for May last, when it immediately occurred to him that his observations might probably assist in determining the period in which the brightness of the star completes the circle of its gradations. When Mr. Birt commenced observing the star in April 1831, the lustre appeared to be at its minimum. In December of the same year, he again observed it to be less than β. His observations were then discontinued until June 1832, when it again appeared less than β. Taking the extreme observations, we have thus two periods completed in about fifteen months, or one period in about 225 days. Assuming this as the period of the variation, and computing from April 26, 1831, the number of days elapsed until April 28, 1839, is 2924, which gives thirteen periods of about 225 days each. Sir John Herschel's observations give the maximum from November 12, 1838, to January 22, 1839; and calculating from July 7, 1831, twelve periods, the maximum would be obtained on December 4, 1838. On the whole, Mr. Birt concludes that the period of 225 days may be regarded as a first approximation, which may receive correction from a comparison with the earlier observations of Sir William Herschel.

On the Variability and Periodic Nature of the Star *α Orionis*. By Sir John F. W. Herschel, Bart. President.

"In a communication which was read to this Society on the 10th of May last, I pointed out the star *α Cassiopeiæ* as variable and periodical. That the fluctuations in splendour of this star should have escaped general notice is not extraordinary, since the difference between its greatest and least brightness can hardly be estimated at more than half a magnitude. But that a periodical variation to a very much greater extent, in so important and remarkable a star as *α Orionis*, should, up to this time, have been completely unnoticed by astronomers, does appear to me, I confess, not a little extraordinary, and might be taken as an argument to show, more than any thing, the comparatively neglected state of this highly interesting branch of Phyical Astronomy. Perhaps, however, in this, as in many other cases, the very prominence of the object has been the cause of its being neglected; as it might easily be supposed by any one entering on this research, that had a star so familiar to every practical astronomer presented any striking peculiarity of this kind, it could not *but* have been observed. Hence, while the attention of observers has been directed, and with success, to much inferior stars, it seems to have been taken for granted, that among stars of the first magnitude nothing, in fact, remained to be discovered.

"Having bestowed much attention, during my residence at the Cape, on the estimation of the magnitudes of the southern stars, both by direct photometrical measurements, assigning numerical values to about sixty or seventy of them, selected as offering convenient gradations of brightness, and also by very assiduous and often-repeated comparisons by the naked eye, with the view to completing a graduated scale down to the fifth magnitude, at least, it became important to connect these magnitudes by similar comparisons with those of the northern hemisphere, by means of stars in the vicinity of the equator admitting of observation at both stations. My method in these observations has been invariably on each night to establish, in the first instance, a sort of skeleton-scale, beginning with the stars of the first magnitude actually visible, and extending as far as was judged convenient for the occasion, then filling in this scale by the insertion of fresh stars between the members. The stars of the first magnitude actually above the horizon at the time of commencing observation were first arranged, and others of that magnitude inserted among them as they rose and gained altitude.

"On the very clear and brilliant night of the 26th November last, being engaged in a process of this kind, I was surprised, and I may almost say startled, by the extraordinary splendour of *α Orionis*, which far exceeded my idea at the moment of what was its natural state. Proceeding to compare it with other stars of the first magnitude (*Sirius* being, of course, out of the question), their arrangement for the night was found to be as follows:

Capella || α Orionis | Rigel || Procyon ||| Aldebaran | Pollux.

"In this and subsequent arrangements of the same sort, the number of vertical strokes between the names indicates the estimated amount of interval, or the *grades* or steps of magnitude by which the stars differ. Thus, the step from *Capella* to *α Orionis* is a great one; that from *α Orionis* to *Rigel*, a distinct but moderate one; from *Rigel* to *Procyon*, a great one, admitting of the easy insertion of a star decidedly inferior to one, and superior to the other, between them; from *Procyon* to *Aldebaran*, a very great step, admitting, at least, two such insertions of imaginary stars decidedly diverse in lustre between them; and so on. Now as I distinctly recollected having, on a great many occasions, placed *α Orionis* nearly on a par with *Aldebaran*, there could be no doubt of a change. Referring next morning to my father's Catalogues of Comparative Brightness, I find that he makes the star in question slightly inferior, or at most equal, to *Procyon*, and much greater than *Aldebaran*.

"In consequence of this observation, I proceeded forthwith to draw out in order all the comparisons of *α Orionis* with other stars made at the Cape, on the voyage homewards, and since my return. In so doing, I must confess I was hardly less surprised than at the sight of the star itself to find in my star-lists, containing the results of a partial reduction and arrangement of my Cape observations, *α Orionis* not merely marked as *variable*, but distinct entries made of it in that list at its maximum and minimum,—the maximum being stated as above *Rigel*, the minimum below *Aldebaran*. This, however, had entirely escaped my memory, but being thus recalled, and so forcibly corroborated, I resolved to watch the star more narrowly in future; the more especially as it seemed to follow, from the tenour of the observations, that its diminution of brightness was likely to be rapid: and so, in fact, it has proved to be."

The author then proceeds to give the observations on which the evidence of the former changes of the star is grounded. They extend over the years 1836, 1837, 1838, and 1839, and are as follows (denoting, for brevity, *α Orionis* by the word *Orion*):—

1836.
March 22. Rigel, Procyon, α Crucis, Orion, Regulus, Pollux.
Nov. 12. $\left\{\begin{matrix}\text{Orion,}\\ \text{Rigel,}\end{matrix}\right\}$ Procyon, Achernar, α Crucis, Aldebaran, Pollux.
13. Orion = Rigel.
26. Rigel, Orion, Achernar.
1837.
Oct. 24. Orion (high), Achernar, Orion (low), Rigel, Aldebaran.
Dec. 16. Rigel, Achernar, Orion.
29. Rigel, Achernar, Procyon, Orion, Aldebaran.

1838.
Jan. 2. Rigel, Procyon, Achernar, Orion, Pollux, *α* Crucis.
6. Rigel, Procyon, Achernar, Orion, Aldebaran, *α* Crucis, Pollux.
13. Rigel, Procyon, Achernar, Orion, Aldebaran, *α* Crucis.
Feb. 25. Rigel, Procyon, Orion, *α* Crucis, Pollux, Regulus.
April 14. Procyon, Rigel, Orion, Aldebaran, Pollux, Regulus.
1839.
Jan. 17. Procyon, Aldebaran, Orion, Pollux, Regulus.
22. Rigel, Procyon, Aldebaran, Orion, Pollux, Regulus.
Nov. 26. Orion, Rigel, Procyon, Aldebaran, Pollux.

On examining the above series, the general order of arrangement (leaving out Orion) is found to be *Rigel, Procyon, Achernar, Aldebaran, α Crucis, Pollux, Regulus*; and the instances in which the arrangement is different are accounted for by some peculiar circumstances connected with the observations. Thus, with respect to the observation of October 24, 1837, the author states that the misplacement of *Achernar* is accountable for by the circumstance of the two comparisons of *Orion* having been made (as appears by the notices *high* and *low*) first when rising with *Achernar* then high, and *Rigel* low; and at a later period of the night with *Rigel* then high, and, consequently, *Achernar* low. On January 2, 1838, *α Crucis* is set down as inferior to *Pollux*; but these two stars are difficult of comparison, both from situation and difference in colour, and from being, in fact, not very different in lustre. The transposition of *Procyon* and *Rigel* in the observation of the 14th of April, 1838, is unaccountable, except from some unsuspected partial haziness in that part of the sky. This observation was made at sea.

With regard to *Orion*, the observations evidently show three maxima, viz. in Nov. 1836, Oct. 1837, and Nov. 1839; and also three minima, viz. those of March 1836, Jan. 1838, and Jan. 1839. "Reasoning from this, the most obvious conclusion is that of an annual, or nearly annual period. But in that case, we must admit the decrease to be comparatively sudden, and the increase slow; whereas, if we admit of a period of about six months, this supposition will not be necessary, and as the star cannot be observed (for this purpose) in the summer months, there is no *primâ facie* reason against adopting the latter period; respecting which, however, further observation will soon enlighten us."

The observations subsequent to Nov. 26, 1839, confirmed the expected decrease of the star in a very decided manner:—

1839. Nov. 30. Rigel | Orion, Procyon || Aldebaran.
Dec. 11. Rigel | Orion | Procyon || Aldebaran.
29. Rigel, Procyon, Orion, Aldebaran.
1840. Jan. 2. Rigel || Procyon | Orion || Aldebaran.
5. Rigel || Procyon | Orion || Aldebaran.
6. Rigel || Procyon || Orion || Aldebaran.

In a note to this last observation, it is stated that "the difference between *Orion* and *Aldebaran* is evidently and rapidly on the decrease. The stars are all high, at nearly equal altitudes, and admirably arranged for comparison."

Jan. 7, 1840. "*Procyon, Orion, Aldebaran*, form a succession by nearly equal steps."—"Upon the whole, I think it may be stated, that in the interval from November 26 to the present date (January 8), *Orion* has sustained a loss of nearly half its light. It may easily be supposed that a diminution, thus evidently still in rapid progress, will, in no long time, carry down the rank of this star below that of *Aldebaran*, and that the confirmation or disappointment of this expectation is awaited with no small interest."

The author concludes with the following remarks:—

"The subject of variable and periodical stars has been of late rather unaccountably suffered to lie dormant; a state of neglect in which, as I have already observed, it ought not to be suffered to remain, and from which I have endeavoured to rescue it on two former recent occasions, by pointing out the stars *α Hydræ* and *α Cassiopeiæ*, both large and conspicuous stars, as belonging to the latter class. A periodical change, however, existing to so great an extent in so large and brilliant a star as *α Orionis*, cannot fail to awaken attention to the subject, and to revive the consideration of those speculations respecting the possibility of a change in the lustre of our sun itself which were put forth by my father. If there really be a community of nature between the sun and fixed stars, every proof that we obtain of the extensive prevalence of such periodical changes in those remote bodies, adds to the probability of finding something of the kind nearer home. It is only in comparatively very recent meteorological observations that we can expect to find that precision in the determination of temperatures which is necessary to establish the absence or presence of periodical change in the intensity of solar radiation; and if the period be not annual (as there is no reason why it should be), the usual mode of combining observations of temperature followed by meteorologists is altogether inappropriate to the research, which can only be carried on either analytically, by the introduction of a periodical term with unknown coefficient, epoch, and period, or graphically, by projecting in a continuous curve the *mean daily* temperatures during a long series of years. For the detection of a period of great length, extending over more than a year, the *continued* observation of the temperature of the water a few feet below the surface in open sea, under the equator, on the principles pointed out by M. Arago in his instructions for the voyage of the Bonite, would suffice. But we are far from possessing as yet sufficient records of such observations to be worth discussing in this point of view. Such observations must of their nature be casual. Even granting that in every ship which traversed the equator the requisite observations were made, the identity of their thermometric standards would be still open to question.

"The assiduous observation in fixed physical observatories of the temperature of the earth, at several depths below the surface, extending from three to thirty feet—an element which we know to be (in its mean amount) *solely* dependent on solar radiation—would be in every respect more immediately and practically applicable to the inquiry, and we may expect to see it carried out into effect. The direct measure of the solar radiation, too, by the actinometer*, ought by no means to be neglected in this inquiry.

"M. Poisson, in a late memoir, has considered the possible consequences, in a geological point of view, of the sun and solar system having, in long by-gone ages, passed through a region in which the actual *temperature of space* should be much greater than in its present locality †. The great authority justly attributed to every idea thrown out by this philosopher, must render it a matter of diffidence and difficulty to maintain a contrary view. Without, however, as a matter of abstract speculation, denying this possibility, I would observe that the temperature at any given point of space can arise only from two sources: 1st, That of the æther, as a fluid susceptible of increase and diminution of temperature; and, 2ndly, The radiation of the stars. Of the temperature of the æther as a fluid, I confess I have no conception. Of the existence of such a fluid as the efficient cause of *light*, we have demonstrable evidence. But the properties of *heat* are so linked and interwoven with those of light, that it is asking more than can be granted to demand our admission that the æther is a fluid *capable of being heated and cooled*, while it is yet undecided (with a leaning to the affirmative side) whether it be not the *efficient cause* of heat itself.

"As regards the radiation of the stars.—There is a region in the heavens where starlight is decidedly more dense than elsewhere—the milky way. And we have, I may almost say, ocular evidence that our system is excentrically situated within that zone, and nearer to its southern than to its northern portion. Granting a perfect transparency of the celestial spaces, the brightness of any given region of the sky must be alike at all distances, whether we conceive that brightness to be uniformly diffused over its surface, or to emanate from a finite number of undistinguishably small points. Now, although the brightness of the southern regions of the milky way may, for argument's sake, be admitted to be three or four times that of the northern, yet, as that light is *almost* completely obliterated by the presence of a full moon in *any* part of the sky above the horizon, it follows that the brightness of *the general firmament* to a spectator placed within the brightest part of the milky way (supposing him not within the range of an individual *sun*), must be less than that of (*not the full moon itself, but*) that general illumination which the moon communicates to the whole sky by atmospheric reflexion; i. e. an almost infinitesimal quantity compared to the direct light of the lunar disc, the intensity *of which* can hardly be to that of the sun in a higher ratio than one to half a million.

"The brightest *regions in the sky*—i. e. the brightest spaces having a visible area—are those occupied by the planetary nebulæ. Of these, there is none which can be compared to *Uranus* in intrinsic brightness, to say nothing of the moon. Supposing, then, our system to be suddenly plunged into the bosom of one of these nebulæ, an increase of temperature would take place less than that which would arise from superadding to our own that which the surface of *Uranus* receives from the sun, or less than the 400th part of that which we actually receive from it; and this supposes *Uranus* to reflect *all* the light incident on it.

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* "This instrument was devised by me for the dynamical measure of the solar radiation in the spring of 1824; and I have had it in use ever since, with continually increasing confidence in its indications." [See p. 78 of the present volume.—EDIT.]

[† A Translation of M. Poisson's memoir will be found in the SCIENTIFIC MEMOIRS, vol. i. p. 122.—EDIT.]

"Leaving to others to judge, however, how far these arguments are to be considered as militating against the view of climatological changes in remote antiquity above alluded to, I may remark that it is a matter of observed fact, that many stars *have* undergone in past ages, within the records of astronomical history, very extensive changes in apparent lustre, without a change of distance adequate to producing such an effect. If our sun were ever *intrinsically* much brighter than at present, the mean temperature of the surface of our globe would, of course, be proportionally greater. I speak now not of periodical, but of secular changes. But the argument is complicated with the consideration of the possibly imperfect transparency of the celestial spaces, and with the cause of that imperfect transparency, which may be due to material non-luminous particles diffused irregularly in patches analogous to nebulæ, but of greater extent—to *cosmical clouds*, in short—of whose existence we have, I think, some indication in the singular and apparently capricious phænomena of temporary stars, and perhaps in the recent extraordinary sudden increase and hardly less sudden diminution of *η Argus*."

Elements of the comet visible at this period, computed by Dr. Petersen, and received from Prof. Schumacher; and parabolic elements of the same computed by Prof. Henderson, are given in the Monthly Notice for January.

XLV. *Intelligence and Miscellaneous Articles.*

DETECTION OF IODATE OF POTASH IN IODIDE OF POTASSIUM. BY MAURICE SCANLAN, ESQ.

It appears that hydriodic acid is sometimes exhibited as a therapeutic agent, and the method resorted to for its extemporaneous preparation is that recommended by Dr. Andrew Buchanan, of Glasgow. It consists in mixing together, in proper proportion, iodide of potassium and tartaric acid, both in solution.

Now, the quantity of free iodine liberated from this salt, which I have under examination, when treated with tartaric acid, in the way just mentioned, has led some dispensing chemists to suppose that it contains more iodine than other specimens of iodide of potassium, which, when treated in a similar way, afford a solution that is colourless, or, at most, of a very pale yellow colour; and hence, as I am informed, some actually look upon tartaric acid as a test of the value of commercial iodide of potassium, assuming the salt of which we are now speaking as a standard of comparison. How far it may be depended upon as a test, will appear from what follows.

If tartaric acid in solution be added to a solution of pure iodide of potassium, the commixed solutions are at first colourless, but quickly become slightly yellow, owing to the action of atmospheric oxygen on the hydriodic acid which is thus generated.

On making this experiment with the salt in question, I found, to my great astonishment, that free iodine, in quantity, was instantly developed. I was at first at a loss to account for so great a difference in the behaviour of this salt to that which I had prepared myself, and knew to be pure iodide of potassium; but from the appearance of the crystals of this salt, and from the circumstance of its not being soluble in water to the extent that it should be, I suspected the existence of iodate of potash in it, and I have since convinced myself of the fact of its presence.

I find, if we add tartaric acid solution to a solution of iodate of potash, no change of colour takes place, but that bitartrate of potash is deposited in abundance, and, as a matter of course, iodic acid set at liberty: this solution instantly decomposes iodide of potassium in solution, giving rise to free iodine in great abundance; or, if we add a drop of solution of tartaric acid to a solution of pure iodide of potassium, to which even a minute quantity of iodate of potash has been added, free iodine is instantly developed.

Tartaric acid appears, then, from the experiments I have made, to be a very delicate test of the presence of iodate of potash in iodide of potassium, and will be found a very ready and useful one for this purpose in the hands of the dispensing chemist, showing him that any specimen of this salt in which free iodine is thus developed, is actually of less value than one in which no trace of iodine appears on the instant of its application; inasmuch as iodide of potassium, in a given weight, includes more iodine than iodate of potash does; as is seen at once by the atomic composition of these two salts.

It is well known to every chemist, that one of the methods very commonly resorted to for the production of iodide of potassium is that of acting upon iodine with potash water. In this way we form iodate of potash at the same time; six atoms of potash and six atoms of iodine giving birth to five atoms of iodide of potassium, and one atom of iodate of potash; which latter, if suffered to remain mixed with the iodide, would increase the produce of the manufactured salt nearly five per cent., at the expense of its purity and crystalline beauty.—*Lancet*, Aug. 29, 1840.

ON PEPSIN—THE PRINCIPLE OF DIGESTION.

M. Wasmann has succeeded in isolating pepsin, the peculiar principle of the gastric juice, described by M. Schwann, in the following manner:—

The glandular membrane of the stomach is to be separated without cutting it; it is to be washed and digested in distilled water at a temperature of 86° to 95° Fahrenheit; after some hours the liquid is to be poured off, the membrane is to be again similarly digested, and to be treated with cold water till it exhales a putrid odour; it is then to be filtered; the filtered liquor is transparent, slightly viscid, and exhibits a remarkable digestive power when a small quantity of hydrochloric acid is added to it. In order to extract the pepsin in a pure state, acetate of lead is to be added to this liquor; the precipitate is washed, diffused in water, and decomposed by a current of hydrosulphuric acid. The filtered liquor is colourless, and has an acid action, owing to the acetic acid.

When this liquor is evaporated at 95° Fahrenheit, to the consistence of a syrup, and absolute alcohol is added to it, an abundant flocculent precipitate is formed, which on drying leaves a yellow gummy matter, which does not attract moisture, and is pure pepsin.

This substance easily dissolves in water, and the solution, even though it contains only 1-5000, dissolves slightly acidulated white of egg, in about six or eight hours. The aqueous solution has an acid action owing to some acetic acid which remains intimately combined with it; it cannot be separated from the pepsinate of lead, even by repeated washings. By ebullition this liquor loses its digestive powers. If the free acid which it contains is cautiously saturated by potash, a small quantity only of which is requisite, flocculi are deposited, and the digestive power is also lost.

The alkalies cautiously added to the solution of pepsin, till the free acid is saturated, occasion the formation of flocculi, and the liquor has no acid action. Sulphuric acid in small quantity produces white flocculi, which redissolve in a slight excess of the acid; by the addition of a further quantity, fresh flocculi are produced; hydrochloric and nitric acids produce the same effects.

Perchloride of mercury occasions a precipitate which is redissolved by an excess of it; the proto- and persulphates of iron and the sulphate of copper precipitate pepsin. Alcohol precipitates it from a concentrated solution. According to M. Pappenheim, this precipitate dissolves in hydrochloric acid, and dissolves boiled white of egg. M. Wasmann confirms this statement of the digestive power of the precipitate formed by alcohol, while, according to M. Schwann, alcohol destroys the digestive property of pepsin.

Pepsin is recognized by the precipitates which its solution gives with diluted acids, and which redissolve in an excess of the acids, and by its giving no precipitate with ferrocyanide of potassium. It is distinguished from albumen by the precipitates which its solution yields on the addition of water and hydrochloric acid; and from caseum, by its acid solutions yielding no precipitate with ferrocyanide of potassium.—*Journal de Chimie Médicale,* Août, 1840.

DECREPITATING SALT OF WIELICZKA. BY H. ROSE.

This salt was first noticed by M. Boué, who sent a specimen of it to M. Dumas; it is distinguished from common salt by decrepitating not only when it is heated, but when dissolved in water; during solution decrepitation occurring with the disengagement of gas. It is evident that this gas was confined in the salt in a state of strong condensation; and this is the cause which occasions the decrepitation both by heat and solution in water.

M. Dumas found that the gas extricated from this variety of salt, when mixed with oxygen gas, detonated like hydrogen; nevertheless he supposed carbon to exist in it. He had not a sufficient quantity of the salt to examine more minutely the gas condensed in these crystals.

M. H. Rose received from Professor Zeuschner of Krakaut, a considerable quantity of the detonating salt, and he has been enabled to repeat and verify the experiments of M. Dumas. The different portions of salt did not all give the same quantity of gas when dissolved in water. The maximum, as stated by M. Dumas, amounted to about half the volume of the salt.

The gas, when burnt with oxygen, gave nearly the same composition as pond gas ($C H^4$). This product is probably so condensed as to exist as a liquid or solid in the interior of the salt, and resumes the state of an elastic fluid at common pressures.

The property which this salt possesses ought in future to direct the attention to a great number of minerals which occur in nature, and which decrepitate in the fire without our being able to attribute it to the disengagement of moisture. It may be that the cause of the decrepitation is the disengagement of a gas condensed in the mineral.—*Ann. de Chim. et de Phys.*, Mars, 1840.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1840.

Chiswick.—Aug. 1, 2. Very fine. 3—9. Hot and dry. 10. Very fine. 11. Showery. 12. Cloudy: rain. 13. Cloudy. 14. Rain. 15. Very fine: showery. 16. Fine. 17. Boisterous with heavy rain. 18. Cloudy. 19. Heavy rain: cloudy and fine. 20. Fine. 21. Foggy: very fine. 22. Foggy. 23—26. Very fine. 27. Foggy: fine. 28. Slight fog: rain. 29. Foggy. 30, 31. Cloudy and fine. The mean temperature of the month was nearly 2° above the average.

Boston.—Aug. 1—3. Fine. 4. Cloudy. 5—10. Fine. 11. Rain. 12, 13. Fine. 14. Cloudy. 15. Stormy: rain P.M. 16. Fine. 17. Stormy: rain early A.M.: rain with thunder and lightning P.M. 18. Stormy. 19, 20. Cloudy. 21. Fine: quarter past three P.M. thermometer 80°. 22. Cloudy: rain P.M.: lightning at night. 23, 24. Fine. 25. Fine: rain P.M. 26, 27. Cloudy. 28. Fine. 29. Cloudy. 30. Fine: rain P.M. 31. Cloudy: rain A.M.

N.B. The warmest August since 1826.

Applegarth Manse, Dumfries-shire.—Aug. 1, 2. Very fine. 3. Mild: showery A.M. 4. Fine. 5. Sultry. 6. Sultry: heat oppressive. 7—9. Sultry. 10. Wet and boisterous P.M. 11. Showery. 12—14. Occasional showers. 15. Fair throughout. 16. Much rain P.M. 17. Heavy rain: thunder: high flood. 18. Fine drying day. 19. Fine, with one slight shower. 20. Drizzling all day. 21. Fine: rain P.M. 22, 23. Fine and fair all day. 24, 25. Showery. 26. Fair all day and clear sky. 27. Wet P.M. 28. Fair all day. 29. Drizzling all day. 30. Fine and fair all day. 31. Remarkably fine harvest day.

Sun shone out 27 days. Rain fell 15 days. Thunder 1 day.

Wind north-west 5 days. East-south-east 1 day. South-east 4½ days. South 7 days. South-south-west 4 days. South-west 8½ days. Variable 1 day.

Calm 12 days. Moderate 11 days. Brisk 5 days. Boisterous 3 days.

Mean temperature of the month............ 57°·60
Mean temperature of August, 1839 ...... 55 ·70
Mean temperature of spring water ...... 52 ·33

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Aug.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max.	Min.		Max.	Min.									
1.	30·278	30·205	30·133	29·65	30·06	30·00	63·7	72·6	53·2	73	46	64	67	50	NW.	NE.	NW.	SW.	...	...	...	...	56
2.	30·254	30·171	30·135	29·55	30·00	30·00	66·7	79·0	55·3	83	51	69	66	52	S.	W.	W.	SW.	...	...	...	...	60
3.	30·244	30·161	30·088	29·53	29·97	30·00	70·0	79·3	61·0	87	48	70	68	53½	S.	NW.	W.	SW.	...	...	...	...	62
4.	30·188	30·117	30·082	29·50	30·04	30·05	67·5	80·0	61·0	86	56	69	67	55	NW.	E.	calm	SW.	...	...	...	...	58
☽ 5.	30·150	30·081	29·991	29·52	30·06	30·05	67·2	68·5	59·0	82	55	71	75	53	SE.	E.	E.	SE.	...	...	...	...	61
6.	30·034	29·959	29·917	29·44	30·04	29·98	69·6	73·0	60·0	84	62	68	73	54	ENE.	E.	E.	SE.	...	...	...	...	60
7.	30·020	29·991	29·930	29·40	30·00	30·03	71·3	78·5	64·0	80	56	70	73	54½	NE.	E.	E.	SE.	...	...	...	0·03	61
8.	30·150	30·129	30·068	29·54	30·12	30·14	65·4	76·0	59·0	80	41	67	73	55½	NNE.	E.	calm	SE. SW	...	...	...	...	59
9.	30·292	30·223	30·106	29·65	30·18	30·10	60·2	75·5	52·4	83	51	67	73½	53	N.	NE.	calm	S.	...	...	...	...	54
10.	30·098	30·052	29·779	29·42	29·84	29·49	69·5	77·0	60·0	86	58	67·5	64	52	SW.	S.	calm	S.	....	·07	...	...	58
11.	29·614	29·566	29·516	28·94	29·40	29·40	63·5	79·7	62·0	85	55	60	64	52	S.	W.	S.	S.	·058	·07	·06	...	60
12.	29·654	29·589	29·555	29·00	29·38	29·30	65·5	70·5	57·0	73	51	63	60	56	WSW.	W.	NW.	S.	·069	·02	·13	...	57
○13.	29·656	29·592	29·551	29·03	29·32	29·35	63·0	70·0	55·0	73	49	59	63½	46	SW.	SW.	W.	SW.	·027	·03	...	...	54
14.	29·530	29·672	29·479	28·97	29·31	29·36	61·0	70·0	56·0	66	50	55	62	51½	W.	SW.	calm	SW.	·058	·06	...	...	54
15.	29·818	29·663	29·752	29·17	29·50	29·66	61·2	67·0	53·5	59	51	60	63½	52	SW var.	SW.	W.	NW.	·213	·03	...	0·77	54
16.	29·968	29·911	29·750	29·35	29·64	29·30	63·0	67·5	54·5	73	54	63	59½	51½	W.	W.	W.	SW.	·072	·22	·13	...	55
17.	29·206	29·326	29·169	28·66	28·81	28·70	59·8	72·0	58·0	63	49	57	61	50½	SE var.	SW.	S.	S.	·159	·47	·18	...	59
18.	29·374	29·654	29·315	28·66	29·19	29·45	59·5	63·2	52·5	62	50	58·5	63½	48	W.	SW.	NW.	NW.	·175	·38	·40	...	54
19.	29·712	29·799	29·661	29·11	29·52	29·66	57·0	63·8	57·0	70	56	56	64	51½	SE.	W.	calm	SSW.	·369	·24	...	...	57
☾ 20.	30·016	29·957	29·915	29·14	29·69	29·75	66·0	69·0	60·0	79	56	59	64½	53	S.	W.	calm	SE.	·061	...	...	...	56
21.	29·954	29·891	29·775	29·30	29·73	29·68	69·0	70·5	60·0	80	59	71	71½	58	SE.	S.	calm	S.	...	...	...	...	58
22.	29·836	29·818	29·755	29·40	29·68	29·66	63·5	77·8	62·5	76	50	66	67	50½	SW.	W.	calm	NW.	...	...	...	...	62
23.	29·998	29·932	29·893	29·30	29·79	29·83	63·0	72·5	56·0	77	46	65	64½	51	WSW.	SW.	calm	NW.	...	·01	·12	1·77	59
24.	30·050	29·976	29·938	29·42	29·78	29·82	62·5	72·5	56·0	76	43	65	63½	47½	SW.	S.	calm	SSW.	...	...	...	...	57
25.	30·038	29·968	29·907	29·38	29·74	29·75	63·5	73·5	54·4	72	58	63·5	60	48	WSW.	SW.	calm	SSW.	...	...	...	...	55
26.	30·060	30·001	29·964	29·42	29·88	29·88	65·0	73·0	61·0	72	49	62	64	42	NW.	S.	calm	S.	...	...	·03	...	61
●27.	30·110	30·011	29.936	29·41	29·80	29·80	61·5	70·8	56·7	73	55	66	65½	53	W.	W.	calm	var.	...	...	...	...	59
28.	30·060	30·044	29·991	29·33	29·92	30·00	63·5	72·0	60·0	74	55	68	61	54	W.	W.	calm	SW.	...	·02	...	...	61
29.	30·204	30·140	30·074	29·57	30·01	29·95	60·9	72·0	60·0	73	57	64·5	63½	54	E.	E.	NE.	NW.	·011	...	...	1·12	60
30.	30·128	30·053	29·920	29·45	30·02	30·15	66·8	69·0	58·0	82	58	69	63½	54½	E.	W.	calm	SSW.	...	...	...	...	62
31.	30·184	30·116	30·040	29·60	30·18	30·15	63·0	79·5	60·0	74	58	59	64	41½	NE.	NE.	calm	ESE.	...	...	·17	...	59
Mean.	29·964	29·928	29·841	29·31	30·406	29·757	64·3	72·7	57·9	76·00	52·68	64·2	61·7	51·5					Sum. 1·272	1·62	1·22	3·69	Mean. 58

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XLVI. *On the mean Level of the Sea. By the Rev.* W. WHEWELL. *F.R.S., Professor of Moral Philosophy in the University of Cambridge*[*].

IN the Philosophical Magazine for August last, page 134, and the following, are some remarks by Mr. Richard Thomas, on the Account of a Level Line, published in the Transactions of the British Association for 1838. The level line was carried from the Bristol Channel to the English Channel by Mr. Bunt, under my direction: the conclusion which I drew from the operation was, that the *mean* height of the sea was at the same level at the two extremities of the line; and this conclusion Mr. Thomas disputes. I beg to offer a few observations on the subject, suggested by his remarks.

In the first place, I will make one or two observations on the theoretical view of the subject. It is certain (although this view does not appear to be familiar to most of those who turn their thoughts to the question) that according to theory, *the mean surface of the sea* (in open water) *is invariable* in its height; and that however different may be the level of high water, or of low water at different places, the line of mean water is a level line. This will appear from the following considerations.

The time of high water is different at different places, and thus there are, at any given moment, unequal pressures operating upon different portions of the sea. It is high water at the Land's-end when it is low water at Beachy Head; and therefore the fluid pressures in different parts of the English Channel are not in equilibrium at any moment. And this is quite consistent with mechanical principles; for the difference of pressures in different parts of the channel is precisely the force by which the tide-wave is transferred along the channel. These oscillations of the pressure and of the surface, correspond to each other; being respectively the changes of force and the motion produced. But the deviations above and below the mean pressure and the mean surface, balance each other in the course of a few hours. If the mean surface were not a level surface, the mean pressures would not be in equilibrium, and there would be in one part of the fluid a *permanent* resulting excess of mean pressure; which excess would tend to carry the fluid from the place where its mean level was highest, and to restore the mean surface to a level, just as if the fluid were at rest.

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[*] Communicated by the Author.

This being understood, it will not be difficult to see what will be the state of the levels of high, low and mean water under any supposed circumstances of a port.

If the water be deep enough, and the surface of low water not much contracted, the motions of the high-water surface and of the low-water surface will be nearly equal, and the mean water will be at the constant height of the level surface: this is the case at Liverpool, as I have shown from Captain Denham's observations in my Twelfth Memoir on the Tides (in the Phil. Trans. 1840).

If the surface of low water be much contracted, the volume of water subtracted from the mean state of the sea in order to make low water, will (in the interior of the port) be nearly equal to the volume added, in order to make high water; and as the horizontal surface is smaller, the depth will be greater: this is the case at Plymouth; where, as I have shown in my Tenth Memoir, the motions of the low-water surface are greater than those of the high-water surface.

If the inlet be a river, in which, by various causes, the tide-wave is gradually extinguished in its progress upwards, and in which, without the tide, the water would flow towards the sea, the high water will be most nearly a level surface, and the mean water line will slope towards the sea. This is the case in the river Thames, in which, as appears by the researches of Messrs. Rennie, the surface of high water is nearly a level surface, while the low water surface slopes considerably.

Finally, if in consequence of shallowness, or a bar, the water cannot run out to the level of low water in the sea, the height of apparent low water will be constant, and the apparent mean water will be charged with half the changes of the high water

Bearing in mind these results, it will be easy to reply to Mr. Thomas's remarks. Mr. Thomas thinks that the sea is probably kept above its average height at Axmouth, one of our stations, by the projection of Cape la Hogue into the English Channel. But from what has been said, it must be plain that this circumstance cannot alter the mean height of the sea on either side of this supposed barrier. A channel fifty miles wide, between Cape la Hogue and the Bill of Portland, is certainly enough to allow the water to assume its mean level, as determined by the mean forces; especially when we consider that it has had an indefinite time allowed to do this in. Indeed, we may satisfy ourselves that this contraction of the surface is no valid reason for the mean surface being higher on the eastern side, if we recollect that it is just as valid a reason for the surface being lower on that side; since the contraction prevents the water finding its level one way as much as the other.

It is true that a contraction of a channel in some cases elevates *high water*; but this is precisely because high water is a case of fluid *motion*, not of fluid *equilibrium*. We may apply to such cases the principle of the conservation of *vis viva*. Each particle of fluid can temporarily ascend to the height due to its velocity; and hence an increased velocity, arising from a contracted channel, makes an increased temporary elevation possible. This consideration also explains the rapid changes in the amount of tide, which take place as we proceed from one part of the sea to another. And hence we see how little we are justified in drawing any inference concerning levels, from any facts of high or low water. The total tide on the coast of Wexford and Wicklow is not more than three feet; on the opposite coast of Wales, (the sea being very narrow,) it is between 20 and 30 feet. If the surface of low water were a level surface, as Mr. Thomas supposes, we should have the mean surface of one side of this narrow sea *constantly* 10 feet higher than the mean surface of the other side; a result quite impossible: whereas a *temporary* elevation or depression in one part of a fluid surface seven or eight times as great as in another part, is quite familiar to us, and offers no difficulty.

In order to show that the mean surface of the sea is not constant, Mr. Thomas refers to observations of his own, made at Falmouth, Bristol, and other places. Upon these I would beg to remark, that he appears to me to consider principally detached observations; whereas we cannot hope to arrive at any satisfactory result, in any other way than by taking the *means* of *series* of observations. We know well that special causes, some of them known and some yet unreduced to rule, perpetually affect particular high waters and low waters; and therefore affect the mean waters as deduced from these. Thus, for instance, the diurnal inequality at Bristol might make two successive high waters differ by four feet; and thus would make the height of mean water differ by two feet, according as the one or the other of these high waters was compared with the intermediate low water. Add to such inequalities those arising from the varying pressure of the atmosphere, the wind, and the like, and we may easily conceive the possibility of such deviations from constancy in the half-tide mark as Mr. Thomas records. Yet these deviations do not prevent the mean of any fortnight's observations from being constant within a very few inches; as it will be found to be at Liverpool, Plymouth, and any other place similarly circumstanced.

It will be observed, that I allow that in a *river* the line of mean water slopes towards the sea; but this condition does not affect either end of our level line; and therefore I consider that the result which we obtained, namely, that the height of mean water is the same at its two extremities, represents the general condition of the ocean.

Mr. Thomas justly remarks, that we have no authority, in our operations, for placing the mean water at Plymouth and at Axmouth at the same level, since our leveling did not extend to Plymouth. Accordingly, we have not drawn any inference from this supposed identity of mean level. The Plymouth observations were referred to, as is stated in the Report, merely in order to ascertain that there were no unestimated inequalities in the tides on that coast at the time of our observations. But if *we* have as yet no authority, except that of theory, for assuming the mean waters at Plymouth and Axmouth to coincide, Mr. Thomas has no authority, either from theory or observation, for assuming the high waters to coincide. And all his other inferences from high waters and from low waters, respecting the level of the sea, are equally gratuitous.

For the reasons stated here and in the Report, I consider it as certain, that on all open coasts the heights of mean water form a level surface. But I entirely agree with Mr. Thomas, that an extension of this leveling operation to other points of the coast would be desirable. In the conclusion of my Report, I have stated several other advantages, besides the confirmation of the above doctrine, which would be placed within our reach by such an extended system of levels. It must, however, be obvious to every one, that such an operation, conducted with that care and exactness which alone could render it valuable, would be business of very serious trouble and expense. To have afforded one good example of such an operation is, I conceive, a merit which gives the British Association a just claim to the gratitude of the scientific world.

Trin. Coll., Cambridge, Oct. 2, 1840.

XLVII. *Observations on certain Peculiarities of form in the Blood Corpuscles of the Mammiferous Animals. By* GEORGE GULLIVER, *F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards*[*].

IN the course of an extensive series of observations[†], I have had occasion to remark, that the red particles of the mammiferous animals are singularly susceptible of change in size and form, as if from the effect of organic contractility[‡]. My attention had long been directed to this subject, when it became still more interesting to me from the results of the examination of the blood of certain deer. A notice of the singular appearances assumed by the blood corpuscles of these animals was given in the Dublin Medical Press of December 18, 1839, and in the Philosophical Magazine for January, 1840; and more fully described, with an illustrative drawing, in a paper read before the Royal Society in February, 1840, and which forms part of the present communication.

In that paper I expressed a doubt whether the remarkable forms presented by the blood particles might not have resulted from changes in the ordinary blood discs; for it was observed that the peculiar figures were more numerous after the blood had been allowed to stand for a short time, while they all disappeared and returned to the circular form when treated with a small quantity of water. In continuing my observations I soon had further reason to confirm this view, as I mentioned in a note on the subject written in the beginning of last March to Dr. Davy. In some specimens of blood which I had recently obtained from the Mexican, Porcine, and Persian Deer, the peculiar corpuscles were by no means so numerous as on former occasions, and in some of the trials were seen but very sparingly, while in other instances the singular particles were present as abundantly as ever.

Hence it became important to ascertain the conditions under which these particles were produced: and although my researches have not hitherto proved perfectly satisfactory in this respect, yet it appears to me that they afford evidence that the blood corpuscles have a contractility or irritability inherent in themselves, which may continue some time after their removal from the body, and enable them to assume permanently the very anomalous forms which I have described and figured. In some instances a large number of the red granulated particles appeared to be produced by the irregular shrinking of the smooth discs*, in others there was a manifest contraction of the particles while under examination in their own serum†, and the corpuscles were very quickly and remarkably modified in figure when extravasated and subjected to contact with the neighbouring tissues‡, evincing the readiness with which the blood discs assume new forms, perhaps connected with the plastic force. It has appeared to me also, that in some cases modifications in the shape of the blood discs might be attributable in great measure to violent action of the heart produced by the fright and struggles of the animal; for when the blood was obtained without at all alarming him the irregular corpuscles were not numerous, while these particles were seen abundantly in blood which was taken when the deer was confined, and made every effort to struggle and get loose, so that the circulation became greatly accelerated. In the Muntjac, a very shy and wild animal, the peculiar particles were always seen in great numbers; but from the more docile Mexican, Porcine, and Persian Deer, a drop of blood could occasionally be obtained before the animal was aware of the operation, and in such cases the irregular shapes were observed but sparingly.

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[*] Communicated by the Author.

[†] Lond. and Edinb. Phil. Mag. for Dec. 1839, and in the Numbers for January, February, March, and August, 1840.

[‡] Mr. Ancell, in his excellent "Lectures on the Blood and other Animal Fluids," mentions that Schultz describes the blood corpuscles as possessed of very remarkable organic contractility. See Lancet, 1839, p. 147 and 386. Eben and Mayer regarded the red particles as infusory animals.

I take this opportunity of expressing a hope that we may soon have an English version of the interesting papers entitled, "*Untersuchungen über die Structur und Veränderungen der Chylus- Lymph- und Blutkörperchen,*" by Dr. Herman Nasse, and published in the valuable "*Untersuchungen zur Physiologie und Pathologie;*" by Drs. Friedrich and Herman Nasse, zweiter Band, Heft 1 and 2.

However, although I am disposed to think that in some ruminants, particularly in those just mentioned, the peculiarities in the particles were more or less influenced by the state of the circulation; yet, from the result of a few trials on other animals, it did not appear that fright or violent muscular exertion was always capable of producing the singular forms in question, although it seems to me that the previous observations render it worthy of inquiry, how far the blood discs may be affected by the state of the circulation, or their contractility influenced through the medium of the nervous system. That the blood particles do contract and assume various forms, is certain; but whether this be a vital or merely a physical phænomenon, is a subject of much interest, and well deserving of new researches. There appears to me to be some good grounds for believing that the blood corpuscles do possess an inherent power of contractility.

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* See Lond. and Edinb. Phil. Mag. for February, 1840.

† *Ibid.*

‡ *Ibid.*

The following paper, already referred to, contains a description and figure of the peculiar corpuscles before mentioned. I have since seen similar forms in the blood of some other animals; and recently in one of the carnivora (*Genetta tygrina*) I observed the crescentic, spear-shaped, and sigmoid particles in great numbers. The animal was so difficult to secure, that its circulation must have been much excited before the blood was obtained. In a second trial, when the blood of this genet was examined immediately, the peculiar corpuscles were not abundant, although in the course of a few hours there were scarcely any other particles to be observed, and the forms shown in the wood-cut remained until putrefaction began.

It will be seen from the figure, that the appearance of many of these corpuscles is very similar to that of the elongated cells represented by Schwann as concerned in the growth of certain tissues. Dr. Martin Barry has lately noticed that the blood corpuscles in certain cases undergo rapid changes; and he announces that the muscular tissue, and the cells of the chorion, are formed from the corpuscles of the blood*. The pus globules, which have recently been regarded as organic cells, have been frequently considered in this country as transformed blood discs, and on the continent M. Gendrin announced that he had actually seen the blood corpuscle changed into the pus globule †.

Observations on the Blood Corpuscles of certain Species of the Genus Cervus. *By* George Gulliver, *F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards.*

(Read before the Royal Society, Feb. 6, 1840.)

The blood corpuscles hitherto described in the vertebrate animals, have either a circular or an elliptical form. Till the late discovery by M. Mandl, of the latter shape in the particles of the Dromedary and Alpaca, and my more recent observation of the same form in the Vicugna and Guanaco ‡, the blood discs were supposed to be circular in all the mammalia, and the oval corpuscles to be confined to the lower divisions of the vertebrate animals.

I have now to describe some peculiar forms of the blood corpuscle, which I believe have not hitherto been observed in any class of animals. These corpuscles I have examined particularly in the Muntjac, Porcine, and Mexican Deer.

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* Proc. Royal Society, May 7, 1840.

† See my Researches on Suppuration, Phil. Mag., Sept. 1838; on the Softening of Fibrine, Med. Ch. Trans. v. xxii.; on Pus, London Medical Gazette 1839—1840, pp. 201 and 415.

‡ See Dublin Medical Press, Nov. 27, 1839. My paper on the Blood and Pus particles of the *Camelidæ*, just published in the twenty-third volume of the "Transactions of the Royal Medical and Chirurgical Society," was read before that Society, Nov. 26, 1839.

I observed in the blood of these animals a large quantity of crescentic or lunated particles, besides a few of the common circular figure. The former are very remarkable from their great number and distinct shape; they are acutely pointed at the ends, gibbous in the middle, with a convex and concave margin; or being without the concavity, they merely present the figure of the segment of a circle.

But there are other forms equally singular: frequently they are not curved, but straight, and gibbous at the sides—lanceolate, to use a botanical term; occasionally they are obtuse at one end, something like a comma in shape; or, from an acute projection of the convex part, approaching to a triangular figure. I have seen them also nearly square, and not uncommonly with elongation of the angles and concavity of the margins, the latter peculiarities being also sometimes observable in the triangular particles. Finally, they may present a sigmoid figure, as if from twisting of the ends of the lanceolate forms.

Like the common blood discs, these peculiar corpuscles are easily deprived of their colouring matter and rendered invisible by water; but if only a very small quantity of this fluid be added to them, they quickly swell out and assume an oval or circular figure, forming, by the approximation of their edges, long bead-like strings. When treated with saline solutions, the oblong particles become rather smaller, but preserve their figure tolerably well.

These singular particles constitute the greater number of those present in the serum, particularly if the blood be kept three or four hours after having been taken from the animal; and the forms are often well preserved in cool weather for nearly a week; in perfectly recent blood they seem to be rather less numerous, although in a short time all the shapes may be recognized abundantly*.

If these singular corpuscles are merely the result of changes of form in the circular discs, such transformations would appear to be altogether peculiar, and at variance with all our previous knowledge of the blood corpuscles of the mammalia.

The first impression will probably be that some of the forms I have described are those which may be presented by different views of the circular discs as they revolve on their axes. Hence it may be necessary to observe, that the crescentic corpuscles are sometimes seen to turn over in the field of vision, and that the lanceolate particles often so revolve. Besides, their length much exceeds the diameter of the circular discs; the extremities of the oblong corpuscles are acutely pointed, and their form and appearance is altogether remarkably distinct and peculiar. Finally, I have preserved dried specimens, which I shall be happy to show to any gentleman who may feel an interest in the subject; and as the animals are alive in the collection of the Zoological Society, no difficulty is likely to remain in the way of inquiry into the facts recorded in this paper.

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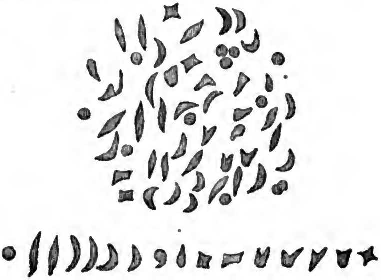
* The blood was constantly examined as soon as possible after it was taken from the animal—often within twenty minutes, and always in less than an hour; and in every case directly the blood flowed through the wound specimens were dried instantaneously on glass, with the precautions mentioned in my paper in the Phil. Mag. for Feb. 1840.

For the accompanying drawing (see wood-cut) I am indebted to my friend Mr. John Dalrymple, who executed it from two portions of blood which I sent to him for the purpose; one being dried on glass, and the other preserved in a weak solution of common salt. The larger group represents the corpuscles as he saw them with a deep achromatic object-glass adapted to a compound microscope. The smaller group exhibits the most remarkable forms of the corpuscles selected and compared together.



The following observations were made at the time the blood was examined.

1. Reeves's Muntjac Deer (*Cervus Reevesii*). The circular discs 1-7200th to 1-6000th of an inch in diameter; the oblong particles from 1-4000th to 1-2666th of an inch in length, and 1-12,000th to 1-8000th in breadth, at the gibbous part. Examined in the recent state, also in urine, in a weak solution of muriate of soda, and dried on glass. In the fresh specimens, the spear-shaped, lunated, and common circular corpuscles often seen turning over in the field of vision. There appeared to be a very great variety in the form of the corpuscles; for besides those already described, there were many oval and egg-shaped, and even triangular or square particles, the two latter indeed not so accurately defined as the former. All these figures may be seen in the corpuscles of the Mexi-

can and Hog Deer, but not so numerously as in this specimen of blood from the Muntjac. It was obtained from a vein of the ear; the animal a male, nearly full-grown, and bred in the Zoological Gardens, where its parents died, and are preserved in the museum of the Society.

2. Mexican Deer (*Cervus Mexicanus*). Circular corpuscles not so numerous as some very singular oblong particles, similar to those seen in the blood of the Muntjac. The circular corpuscles generally about 1-6000th of an inch in diameter; the oblong corpuscles 1-3200th to 1-2400th of an inch in length, and 1-12,000th to 1-8000th in breadth, at their gibbous part. In this situation a few of them are as large as the circular discs. The blood examined was obtained from a vein of the ear of a female, apparently full-grown. The observation was the same whether made on the recent or dried blood. The veins in the ear of the animal are numerous and very apparent. From another bleeding, a week after the first, the same result obtained.

3. Hog Deer (*Cervus porcinus*, albino var.). In some blood obtained from a prick of the upper lip, the corpuscles were found to be similar in size and shape to those described in the Muntjac and Mexican Deer, except that some of the circular discs appeared a little larger, and they were generally as numerous as the singular oblong corpuscles. For a second trial, some blood was obtained from an incision of the ear, about a fortnight after the first bleeding, and the former result confirmed. The animal was a male, apparently full-grown.

In conclusion, it may be remarked that the peculiar particles now described, so singularly variable and anomalous, and susceptible of such remarkable mutations in their figure, afford a very interesting subject for further inquiry, which may probably illustrate the physiology of the blood corpuscles. At present so little is known respecting these curious bodies, and so much is to be expected from future research, that we know not what degree of importance may belong to them, although there is reason to believe that the discovery of their use and method of formation would be of great value to science.

An Appendix to the "Observations on the Blood Corpuscles of certain Species of the Genus Cervus." *By* GEORGE GULLIVER, *F.R.S.*

(Read before the Royal Society, Feb. 6, 1840.)

Since I had the honour of transmitting my communication to the Royal Society "on the Blood Corpuscles of certain species of the genus *Cervus*," I have had an opportunity of examining some blood obtained from a deer lately received at the Zoological Gardens from the Persian mountains. I am not aware that the species of this animal has yet been determined, or indeed whether it has ever been described. It is about as large as a full-grown fallow deer.

The blood was procured from a puncture in the upper lip of a female, under circumstances very favourable for the preservation of the size and figure of the corpuscles; they were quickly dried on slips of glass, and submitted to observation with as little delay as possible.

The greater part of the blood corpuscles presented the usual circular shape, although often irregularly triangular, quadrangular, or polygonal; and there were many of the singular forms described in the Muntjac, Porcine, and Mexican Deer. The latter particles corresponded very nearly in size with the measurements already given; the former were rather larger, having an average diameter of 1-5066th of an inch, the small and large discs measuring respectively 1-6000th and 1-4000th of an inch.

Although I would not venture to communicate to the Society any account of the blood corpuscles from a single observation, yet it appears to me that the present one may be considered as an addition to those which I have already made, with as much care as I could, concerning the blood of the other species of deer.

Whether the peculiar corpuscles exist in the circulation of the animals, or may be the effect merely of changes in the form of the circular discs immediately after their abstraction from the vessels, is a subject for further and special inquiry, and one which may tend to throw some light on the nature of the blood corpuscles. But whatever may be the result, the facts will hardly be less singular and remarkable, and I am not aware that they have hitherto attracted the attention of physiologists, notwithstanding the minute examination to which the blood particles of different animals have lately been subjected.

XLVIII. *On some Combinations of Arsenic with Cobalt.* By THEODORE SCHEERER, *and* WILLIAM FRANCIS*.

THE combinations which we have to describe in the present paper, were formed in the course of a smelting process which was introduced some years since by Inspector Roscher, into the smalt-works at Modum in Norway. The object of this process is to deprive the prepared and roasted cobalt ores of a great part of their iron and arsenic by smelting, in order to obtain higher and purer kinds of smalt from them.

First Combination.—This forms long fascicular conglomerated crystals, terminated at the extremities by an oblique plane. Its dull surface, arising from a film of oxide, together with the fascicular arrangement, does not allow of a more accurate crystallographical determination; it is probable that they belong to the mono-dimetric system. Some of the crystals, or rather of the bundles of crystals, were above two inches long, and about two lines in thickness. The analysis was performed in the mode usually employed for the arsenic metals. Cobalt and iron were separated from each other by approximate neutralization of the sulphate solutions of the two metals, by dilution and then boiling them.

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* Communicated from Berlin by the Authors.

The result of the analysis was

Sulphur	0·16	
Arsenic............	36·02	
Cobalt	53·71	
Iron	10·05	
Copper	0·86	——100·80.

The peroxide of iron here obtained was found before the blow-pipe to be perfectly free from oxide of cobalt; the oxide of cobalt, however, contained a slight trace of iron, which was separated by dissolving the oxide in hydrochloric acid, and precipitating with an excess of ammonia. It may have amounted, at the utmost, to a few milligrammes.

To be certain that these crystals contained no admixture of foreign substances, but were a pure chemical combination, the following experiments were made with other portions of them. In the first place, the amount of arsenic, the accurate determination of which was most essential, was ascertained in two other additional cases, and found to be as follows:—

1st, 36·41 per cent.
2nd, 35·34 ——

approximating, therefore, to the quantity first found, 36·02.

The sum of the oxide of cobalt and peroxide of iron, which in the first analysis had amounted to 2·061 grammes, was, in a second examination, for which a quantity differing from the first only by 0·005 was employed, equal to 2·067 grammes. In both experiments the oxides were kept at a red heat in an open platinum crucible, until they no longer increased in weight*. Lastly, the quantity of cobalt was again determined by a second trial, and found to be 54·90; whereas the first gave it 53·71. The differences occurring between these several results are certainly not such as to give rise to any doubt as to the homogeneity and purity of this combination. If, therefore, it be admitted that the above-mentioned quantitative composition affords a perfect symbol of the nature of the combination, then the atomic relation of arsenic to the other metals would be as the numbers

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* When a larger quantity of the peroxide of iron is heated with a smaller quantity of the oxide of cobalt for a short time in a closed platina crucible, the latter scarcely becomes more highly oxidized, and in analyses not requiring extreme accuracy it may be admitted that no superoxide is formed.

7·74 : 17·76,

that is, if it be assumed that the very slight quantity of sulphur occupies the place of a portion of the arsenic, and that copper, cobalt and iron are here isomorphous. That this actually is the case, will very clearly appear from the experiments subsequently detailed.

Second Combination.—This is crystallized in large laminæ of metallic lustre, which for the greater part are so rooted in the mass, that it was not possible to determine from the termination of the laminæ the crystallographic system to which they belong. Frequently several such laminæ are superposed one upon the other, and project with dull superficies beyond the surface of the rest of the metal. They have then an appearance quite similar to that of the above-described combination. The analysis was performed differently from the former, inasmuch as the cobalt and iron were separated from each other by the succinate of ammonia.

The result was as follows:—

Sulphur	0·50	
Arsenic	35·20	
Cobalt	31·35	
Iron	23·15	
Copper	8·90	——99·10.

The peroxide of iron separated by succinate of ammonia evinced, indeed, before the blow-pipe the action of cobalt, but in so slight a degree that it seemed of no importance. The oxide of cobalt, however, on being dissolved in muriatic acid, and treated with ammonia in excess, left behind 0·033 gram. of the peroxide of iron; the quantity of which, however, was of course increased by some oxide of cobalt precipitated with it *.

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* More accurate results, as to the application of succinate of ammonia for the separation of cobalt from iron, are, as far as my experience goes, extremely difficult to obtain. If it be attempted more perfectly to neutralize the fluid previously to the addition of the succinate of ammonia, the peroxide of iron may very likely prove to contain somewhat too much cobalt. The method that I have recommended for the separation of these two metals, it is true, has also its difficulties; however, in most cases I have had better success with it, and scarcely ever worse, than with succinate of ammonia. At the same time this very great advantage should be considered, that the solution containing cobalt, filtered from the iron, can be immediately thrown down with potash; while, in the separation by

If we calculate the atomic relation for this combination in the same manner as for the former, we obtain

$$7{\cdot}74 : 17{\cdot}74,$$

therefore exactly coinciding with that of the first combination. From the above-mentioned composition of the latter, it appeared probable that the small quantity of sulphur in it was combined with the likewise only small quantity of copper forming the sulphuret of copper; and that this was merely mechanically mixed with the remainder of the combination. This seemed to be the more admissible, as small quantities of the sulphuret of copper often collect as slag (speise) on the floor of the smalt-ovens at Modum*. The analysis of the second combination proves, however, that copper and sulphur do not stand in any relation, for here the quantity of the first is increased to 8·90 per cent., while that of the latter amounts merely to 0·50 per cent.

With respect to the formula for the two combinations, it must turn out to be the same for both, as the atomic relations

$$7{\cdot}74 : 17{\cdot}76$$

and

$$7{\cdot}74 : 17{\cdot}74$$

approach so very near to each other. It is almost perfectly coincident with the composition ascertained, if we adopt it at

$$\left.\begin{matrix} Co^7 \\ Fe^7 \\ Cu^7 \end{matrix}\right\} As^3$$

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means of succinate of ammonia, the oxide of cobalt is obtained dissolved in an ammoniacal fluid, from which it has to be precipitated by sulphuret of ammonium. The filtration of the sulphuret of cobalt is, however, as is well known, one of the least pleasant operations of analytic chemistry. That the separation by my method sometimes proves unsuccessful, arises, perhaps, not so much from the point of neutralization not being closely enough attained, but rather, 1st, from the dilution after saturation not being carried far enough; or, 2ndly, from the solution not being sufficiently stirred during saturation upon the addition of each fresh portion of caustic potash; or, 3rdly, from a potash being employed containing carbonic acid, thus causing the fluid (from the carbonic acid dissolved therein) to act acidly, even when the point of neutralization has been already exceeded. Care must also be taken, that on the stirring round, no flocks of the oxide of cobalt, which are precipitated at the first moment upon the addition of the caustic potash, but soon redissolved, adhere to the margin of the porcelain basin, and, drying upon it, escape solution. These, indeed, are all circumstances which must be attended to, and which occasion some difficulty in the employment of my method; but which are easily overcome by some practice. Whenever we shall be able to determine in analytic chemistry all bodies as easily and strictly as barytes and sulphuric acid, then, indeed, such methods of separation as that just described will become superfluous.—Sch.

* Nickel slag (*Nickelspeise*) does not occur here at all.—Sch.

in which case the atomic relation should be as

7·74 : 18·06.

Such a numerical relation appears at first sight, it is true, somewhat unusual. The formula

$$\left.\begin{matrix}Co^5\\Fe^5\\Cu^5\end{matrix}\right\}As^2$$

would, perhaps, be more probable; however, the atomic relation corresponding to it, viz.

7·74 : 19·35

differs still more from the ascertained result.

There sometimes occurs a third kind of crystallized arsenical cobalt in the smelting processes at Modum. It forms dendrites of a kind quite similar to those of speiss-cobalt, or such as are obtained as pure copper in the [*aussaigerung*] of gun-metal*. This, therefore, would prove that its crystalline form would belong to the regular system. An analysis which was made about seven years since gave approximately the formula

$$\left.\begin{matrix}Co^5\\Fe^5\\Cu^5\end{matrix}\right\}As$$

or, more correctly,

$$\left.\begin{matrix}Co^5\\Fe^5\\Cu^5\end{matrix}\right\}S + 2\left\{\begin{matrix}Co^5\\Fe^5\\Cu^5\end{matrix}\right\}As$$

since beside 14 per cent. of arsenic, it contained 3 per cent. of sulphur. The copper amounted to 7·86 per cent. However, the iron and cobalt were separated in a defective manner, whence the former contained a considerable amount of cobalt. The analysis needs therefore repetition; although, since iron and cobalt do not differ much in their atomic values, scarcely any essential change in the formula might ensue.

XLIX. *Examination of a crystallized Nickel Ore. By* WILLIAM FRANCIS, *A.L.S.*†

THE combination which is the subject of the present paper, although consisting mainly of arsenic and nickel, yet has to be distinguished from the kind of nickel ore analysed some time since by Professor Wöhler. Whilst the latter is crystallized in octahedrons, and is deposited from the smalt furnaces; the former is crystallized in large laminæ, very similar to the second cobalt ore described in the preceding Article, and is evidently the product of a further process of purification: nor was it obtained directly from a smalt-work, but from the German silver manufactory of Mr. Henniger in Berlin. This process of purification seems to resemble that which is practised at Modum with the cobalt ores.

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* I obtained of Dr. Marchand, who is at present engaged in very interesting experiments on alloys for fire arms, a piece of dendritic copper bearing very great resemblance to the native.—SCH.

† Communicated from Berlin by the Author.

The analysis was performed in the laboratory of Professor Henry Rose; and the course pursued quite similar to that employed for combinations of arsenic. The nickel and iron, however, were not separated by succinate of ammonia, but determined according to Dr. Scheerer's method. The result of the analysis was

Sulphur	1·01
Arsenic	34·07
Nickel..............	52·58
Cobalt	3·28
Iron	10·06——101·00.

The peroxide of iron obtained was again dissolved in hydrochloric acid, and precipitated by ammonia in excess. Sulphuret of ammonium produced no perceptible alteration in the filtered ammoniacal fluid. From the obtained oxide of nickel, however, 0·009 grammes of nickeliferous peroxide of iron were separated by solution and the addition of ammonia: which quantity had not been separated by the above method.

In this separation, therefore, Dr. Scheerer's method gave a better result than that with succinate of ammonia in the analysis of the cobalt ore described in the preceding Article.

If it be admitted that the slight quantity of sulphur takes the place of a part of the arsenic, the atomic weight of the arsenic is to that of the other metals as

$$7·74 : 18·10,$$

therefore, exactly as in the combinations detailed in the preceding Article. The formula for this nickel ore would accordingly likewise be

$$\left.\begin{matrix}\mathrm{Ni}^{7}\\\mathrm{Co}^{7}\\\mathrm{Fe}^{7}\end{matrix}\right\}\mathrm{As}^{3}$$

In this case the atomicp roportion should be

$$7·74 : 18·06,$$

coinciding, therefore, very nearly with that given by the analysis. If, on the other hand, the formula

$$\left.\begin{matrix}\mathrm{Ni}^{5}\\\mathrm{Co}^{5}\\\mathrm{Fe}^{5}\end{matrix}\right\}\mathrm{As}^{2}$$

were adopted, the proportion

$$7{\cdot}74 : 19{\cdot}35$$

would differ more from the result of the analysis. According to the first supposition, the combination, in 100 parts, should consist, admitting only nickel and arsenic as essential constituents, of

Arsenic	35·25
Nickel.........	64·75

and with this the analysis agrees exceedingly well. According to the second formula ($Ni^5 As^2$), on the contrary, the composition should be

Arsenic	33·69
Nickel	66·31.

Now, whichever formula may be considered as most correct, thus much is certain, that the combinations described in the preceding Paper, and in this, are by no means composed according to simple relations, as 1 : 2, or 2 : 3; nor will this appear at all extraordinary, in a metal such as arsenic, which forms an acid consisting of two atoms of radical and five atoms of oxygen. It is, indeed, questionable whether the formula $Ni^3 As^2$, laid claim to by Prof. Wöhler for the nickel ore analysed by him, does not likewise possess an atomic relation similar to that of the combinations here treated of.

Now the proportion 2 : 3, 3 : 5 approach one another very nearly, as the following calculation shows:—

	$Ni^3 As^2$	$Ni^5 As^3$
Nickel	54·15	56·75
Arsenic ...	45·85	43·25.

Professor Wöhler, however, found the composition to be

Nickel	55
Arsenic	44,

approaching, therefore, nearer to the formula $Ni^5 As^3$ than to $Ni^3 As^2$. It is therefore very probable that the number 5 also occurs in the atomic relation of the constituents in this combition; and there would then have been found on the whole the following proportions between cobalt (or nickel) and arsenic:

Third cobalt-combination $Co^5 As$ (?)

First and second do. as likewise purified nickel ore $\left\{\begin{matrix} Co^5 \\ Ni^5 \end{matrix}\right\} As^2$, or, $\left\{\begin{matrix} Co^7 \\ Ni^7 \end{matrix}\right\} As^3$

Common nickel ore .... $Ni^5 As^3$.

The middle combination will, according to theory, be regarded as far more probably $Co^5 As^2$; although the results of the analyses bring it nearer to the formula $Co^7 As^3$.

L. *On the Variation of the Semi-axis Major of the Moon's Orbit.* *By* John William Lubbock, *Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

POISSON, in his *Mémoire sur le Mouvement de la Lune autour de la Terre*, has considered the following theorem, that the expression for the variation of the semi-axis major contains no argument of long period, accompanied by a multiple of m less than m^4. In this paper he has taken into account the terms which arise from the second approximation, or that in which the squares and products of the quantities $\delta\zeta$, δa, δe, $\delta\varpi$, $\delta\gamma$ and $\delta\alpha$ may be neglected. It is evident that terms may arise in the next, and indeed in every succeeding approximation of the order m^3, which must be taken into account in order to prove the proposition with sufficient generality. Thus the variation of the eccentricity contains terms multiplied by m, as of the argument 9, $(2\tau - 2\xi)$; these, multiplied by others of the order m^2, give in δe^2 terms of the order m^3, and these multiplied by $\frac{\mathrm{d}^3 R}{\mathrm{d}\,e^2\,\mathrm{d}\,c}$ give in $\frac{\mathrm{d}\,R}{\mathrm{d}\,c}\,\mathrm{d}\,t$ terms multiplied by m^5, which after integration become of the order m^3; if the argument being of the kind under consideration, the divisor introduced by integration is of the order m^2. It is true that Poisson refers to his *Mémoire sur la Variation des Constants arbitraires Mém. de l'Academie*, tom. i., for an extension to the third approximation, that is, to terms depending upon the cube of the disturbing force. In this paper, however, expressions are employed, which take for granted that the disturbing force can be exhibited in the same form, developed in terms of the initial values of the coordinates x, y, z, and of the initial values of their first differential coefficients $\frac{\mathrm{d}\,x}{\mathrm{d}\,t}$, $\frac{\mathrm{d}\,y}{\mathrm{d}\,t}$, $\frac{\mathrm{d}\,z}{\mathrm{d}\,t}$. This development has never been executed, nor has it been shown to be possible. When this system of constants is employed, the quantities which are equivalent to $[a, \varpi]$, $[a, e]$, &c., become equal to unity and rigorously constant, so that it is unnecessary to consider the effect produced by their variation. Even, however, with the simplifications which recourse to this peculiar system of constants affords, Poisson admits that this demonstration would become too complicated to admit of its extension to the higher powers of the disturbing force. M. de Pontécoulant has made objections to the proof given by Poisson, but as he differentiates* in a manner at variance with that intended in the expressions of which Poisson makes use, and does not allude to the Memoir in question, in which a further approximation is attempted, these objections are not to be considered as exactly identical with those of that distinguished astronomer.

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* *Conn. des Temps*, 1840, p. 21.

In the Lunar Theory terms of the order m^3, and of the nature of those under consideration, may arise not only in the third but in every succeeding approximation, and hence it becomes absolutely necessary to seek some mode of proof which admits of unlimited extension, and I have therefore endeavoured to modify the proof given by Poisson, so as to include all terms of the order m^3, however far the approximation be pushed, and without introducing any peculiar system of constants.

For this purpose it is necessary to suppose the disturbing function R

$$= (R) + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\zeta}\right)\delta\,\zeta + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,a}\right)\delta\,a + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,c}\right)\delta\,c$$

$$+ \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\right)\delta\,e + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi}\right)\delta\,\varpi + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\gamma}\right)\delta\,\gamma + \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\alpha}\right)\delta\,\alpha$$

$$\zeta = n\,t \qquad \frac{\mathrm{d}\,R}{\mathrm{d}\,\zeta} = \frac{\mathrm{d}\,R}{\mathrm{d}\,c} \qquad n^2\,a^3 = \mu;$$

and to define $\delta\,\zeta$, $\delta\,a$, $\delta\,c$, &c., to represent, not the total variation of the quantities ζ, a, c, e, &c., as in Poisson's paper, but that portion only which consists of arguments corresponding to those inequalities which are not depressed by integration, as will be presently explained. The effect of the secular inequalities of the constants c, ϖ and α, and also that of all the other inequalities of which the arguments are independent of c, are supposed to be already included in the quantities

$$(R),\ \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,a}\right),\ \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,c}\right),\ \text{\&c.}$$

As the constant c in Poisson's notation always accompanies $n\,t$, all the arguments in the development of R may be properly represented by an expression of the form

$$i\,(n\,t + c) + j\,m\,t + l\,t + \beta,$$

where i and j are whole numbers or zero, l a certain multiple of m^2 depending upon the secular variation of the angles c, ϖ, α, and β a quantity rigorously constant, which accompanies $l\,t$, but which I shall in future omit to write down. Whenever $i = 0$ without j becoming $= 0$ at the same time, the corresponding inequality in the expressions for the elliptic constants is necessarily a multiple of m at least, and only when i and j are both equal to zero the corresponding inequality in each of those expressions may no longer be multiplied by m.

$$\mathrm{d}\,a = [a, c]\,\frac{\mathrm{d}\,R}{\mathrm{d}\,c}\,\mathrm{d}\,t + [a, \varpi]\,\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi}\,\mathrm{d}\,t + \text{\&c.}$$

$$\mathrm{d}\,c = [c, a]\frac{\mathrm{d}\,R}{\mathrm{d}\,a}\mathrm{d}\,t + [c, \omega]\frac{\mathrm{d}\,R}{\mathrm{d}\,\omega}\mathrm{d}\,t + \&c.$$

The quantities between brackets may be considered as constant in the first approximation, and such that

$$[c, a] = -[a, c] \qquad [\omega, c] = -[c, \omega].$$

Similar theorems exist with respect to all the constants, and it is upon this property that the theorem in question may be said to depend. If a, e, γ, c, ω, α, have the same signification as in Poisson's memoir (*Mémoire de l'Institut*, tom. xiii.)

$$[a, e] = 0 \qquad [a, \gamma] = 0 \qquad [e, \gamma] = 0$$

$$[e, \omega] = 0 \qquad [c, \gamma] = 0 \qquad [\omega, \alpha] = 0;$$

but these latter theorems do not appear to influence the proposition of which the proof is required.

In differentiating the disturbing function with regard to c, as indicated in the expression

$$\mathrm{d}\,a = -\frac{2\,\mathrm{d}\,R}{a\,n\,\mathrm{d}\,c}\mathrm{d}\,t,$$

a being the semi-axis major, R must be differentiated only with respect to c, inasmuch as it was contained in R primitively, and not as it is introduced in further approximations by the variations of the elliptic elements. As, however, the secular variations and those multiplied by m and m^0, which I have already included in

$$(R), \quad \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,a}\right), \quad \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\right), \quad \&c.,$$

do not contain i or c, and as therefore c only occurs in the quantities

$$(R), \quad \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,a}\right), \quad \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\right), \quad \&c.,$$

as it was introduced primitively *,

$$\frac{\mathrm{d}\,R}{\mathrm{d}\,c} = \frac{\mathrm{d}\,(R)}{\mathrm{d}\,c} + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,c^2}\right)\delta\,\zeta + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,a\,\mathrm{d}\,c}\right)\delta\,a + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,c^2}\right)\delta\,c$$

$$+ \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,e\,\mathrm{d}\,c}\right)\delta\,c + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,\omega\,\mathrm{d}\,c}\right)\delta\,\omega + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,\gamma\,\mathrm{d}\,c}\right)\delta\,\gamma + \left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,\alpha\,\mathrm{d}\,c}\right)\delta\,\alpha$$

$\delta\,\zeta$, $\delta\,a$, $\delta\,e$, &c., denoting here only that portion of the variations of ζ, a, e, &c., which consists of periodical terms not independent of i, and therefore multiplied by the square of m at least.

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* This remark applies solely to the constant c, and to no other.

$$\delta \zeta = - \frac{3\,n}{2\,a} \int \delta\, a\, \mathrm{d}\, t,$$

and it is sufficient to take

$$\frac{\mathrm{d}\,a}{\mathrm{d}\,t} = \frac{1}{a^2\,n} \left(3\,n \frac{\mathrm{d}^2 R}{\mathrm{d}\,c^2} \int \delta\, a\, \mathrm{d}\, t - \frac{\mathrm{d}\,R}{\mathrm{d}\,c} \delta\, a \right)$$

$$- 2 \left\{ \left(\frac{\mathrm{d}^2 R}{\mathrm{d}\,a\,\mathrm{d}\,c} \right) \frac{\delta\, a}{a\,n} + \left(\frac{\mathrm{d}^2 R}{\mathrm{d}\,c^2} \right) \frac{\delta\, c}{a\,n} + \frac{\mathrm{d}^2 R}{\mathrm{d}\,e\,\mathrm{d}\,c} \frac{\delta\, e}{a\,n} \right.$$

$$\left. + \left(\frac{\mathrm{d}^2 R}{\mathrm{d}\,\varpi\,\mathrm{d}\,c} \right) \frac{\delta\, \varpi}{a\,n} + \left(\frac{\mathrm{d}^2 R}{\mathrm{d}\,\gamma\,\mathrm{d}\,c} \right) \frac{\delta\, \gamma}{a\,n} + \left(\frac{\mathrm{d}^2 R}{\mathrm{d}\,\alpha\,\mathrm{d}\,c} \right) \frac{\delta\, \alpha}{a\,n} \right\}.$$

This equation is similar in appearance only to the equation (A) of Poisson, p. 259, for the quantities $\delta\, a$, $\delta\, c$, $\delta\, e$, &c., are here differently *defined.*

I omit here all consideration of the terms

$$\frac{1}{a^2\,n} \left(3\,n \frac{\mathrm{d}^2 R}{\mathrm{d}\,c^2} \int \delta\, a\, \mathrm{d}\, t - \frac{\mathrm{d}\,R}{\mathrm{d}\,c} \delta\, a \right),$$

because the proof which Poisson has given in his *Mémoire sur le Mouvement de la Lune,* is completely satisfactory with reference to them.

In the terms $\frac{\delta\, c}{a\,n}$, $\frac{\delta\, e}{a\,n}$, &c., it is necessary to consider the variation of the quantities $[a, \varpi]$, $[e, \varpi]$, &c., and also that of the quantity $\left(\frac{1}{a\,n} \right)$

$$\mathrm{d}\,.\,\frac{1}{a\,n} = - \frac{\mathrm{d}\,a}{n\,a^2} - \frac{\mathrm{d}\,n}{n\,a^3} = \frac{\mathrm{d}\,a}{2\,n\,a^2} = - \frac{\mathrm{d}\,R}{\mu\,\mathrm{d}\,c} \mathrm{d}\,t;$$

d e contains the term

$$[e, \varpi]\, \frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \mathrm{d}\,t,$$

which may be taken as a type of others ;

$$\frac{1}{a\,n} \int [e, \varpi] \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \right) \mathrm{d}\,t = \int \frac{[e, \varpi]}{a\,n} \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \right) \mathrm{d}\,t$$

$$+ \mathrm{d}\,.\,\frac{1}{a\,n} \int [e, \varpi] \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \right) \mathrm{d}\,t$$

$$= \int \frac{[e, \varpi]}{a\,n} \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \right) \mathrm{d}\,t - \frac{\mathrm{d}\,R}{\mu\,\mathrm{d}\,c} \int [e, \varpi] \left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\varpi} \right) \mathrm{d}\,t.$$

It must be recollected that in this expression no terms are included in δe or $\int [e, \varpi] \frac{\mathrm{d} R}{\mathrm{d} t}$ which are independent of c.

If $\left(\frac{\mathrm{d} R}{\mathrm{d} e}\right)$, as defined and limited in the expression for R in p. 339, contains any term

$$A \cos (i (n t + c) + j m n t + l n t),$$

$\mathrm{d} \varpi$ contains the term

$$[a, \varpi] A \cos (i (n t + c) + j m n t + l n t);$$

and considering now only the constant portion of $[a, \varpi]$, $\delta \varpi$ contains the term

$$[a, \varpi] \frac{A}{(i + j m + l) n} \sin (i (n t + c) + j m n t + l n t).$$

Similarly, if $\left(\frac{\mathrm{d} R}{\mathrm{d} \varpi}\right)$ contains the term

$$B \sin (i (n t + c) + j m n t + l' n t)$$

$\mathrm{d} e$ contains the term

$$[\varpi, e] B \sin (i (n t + c) + j m n t + l' n t),$$

δe contains the term

$$- [\varpi, e] \frac{B}{(i + j m + l') n} \cos (i (n t + c) + j m n t + l' n t),$$

and these terms give, after well-known reductions, in $\int \frac{\mathrm{d} R}{\mathrm{d} c} \mathrm{d} t$

$$[\varpi, e] \frac{i A B}{n (i + j m + l) (i + j m + l')} \cos (l' - l) t,$$

which is evidently of the order m^4, A and B being each of the order m^2, and i of necessity not equal to zero.

I now proceed to consider the effect of the variation of the quantity

$$\frac{[e, \varpi]}{a n},$$

which may be taken as the type of other similar quantities.

Let

$$\frac{[e, \varpi]}{a n} = C + D \cos (i n t + j m n t + l n t)$$

$$\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\right) = A\cos\,(i'\,n\,t + j'\,m\,n\,t + l'\,n\,t)$$

$$\frac{[e,\omega]}{a\,n}\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\right) = C\,A\cos\,(i'\,n\,t + j\,m\,n\,t + l'\,n\,t)$$

$$+ \frac{D\,A}{2}\cos\,(i'\,n\,t - i\,n\,t + j'\,m\,n\,t - j\,m\,n\,t + l'\,n\,t - l\,n\,t)$$

$$+ \frac{D\,A}{2}\cos\,(i'\,n\,t + i\,n\,t + j'\,m\,n\,t + j\,m\,n\,t + l'\,n\,t + l\,n\,t).$$

Let $\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\omega}\right) = B\sin\,(i''\,n\,t + j''\,m\,n\,t + l''\,n\,t)$

$$\frac{[e,\omega]}{a\,n}\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\omega}\right) = C\,B\,\sin\,(i''\,n\,t + j''\,m\,n\,t + l''\,n\,t)$$

$$+ \frac{D\,B}{2}\sin\,(i''\,n\,t - i\,n\,t + j''\,m\,n\,t - j\,m\,n\,t + l''\,n\,t - l\,n\,t)$$

$$+ \frac{D\,B}{2}\sin\,(i''\,n\,t + i\,n\,t + j''\,m\,n\,t + j\,m\,n\,t + l''\,n\,t + l\,n\,t).$$

$$\int \frac{[e,\omega]}{a\,n}\left(\frac{\mathrm{d}\,R}{\mathrm{d}\,\omega}\right)\mathrm{d}\,t$$

$$= -\,C\,B\,\frac{\cos\,(i''\,n\,t + j''\,m\,n\,t + l''\,n\,t)}{(i'' + j''\,m + l'')\,n}$$

$$-\,\frac{D\,B}{2}\,\frac{\cos\,(i''\,n\,t - i\,n\,t + j''\,m\,n\,t - j\,m\,n\,t + l''\,n\,t - l\,n\,t)}{(i'' - i + j''\,m - j\,m + l'' - l)\,n}$$

$$-\,\frac{D\,B}{2}\,\frac{\cos\,(i''\,n\,t + i\,n\,t + j''\,m\,n\,t + j\,m\,n\,t + l''\,n\,t + l\,n\,t)}{(i'' + i + j''\,m + j\,m + l'' + l)\,n}$$

$$-\int [e,\omega]\,\frac{\mathrm{d}\,R}{\mathrm{d}\,e}\,\mathrm{d}\;t$$

$$= -\,C\,A\;\frac{\sin\,(i'\,n\,t + j'\,m\,n\,t + l'\,n\,t)}{(i' + j'\,m + l')\,n}$$

$$-\,\frac{D\,A}{2}\,\frac{\sin\,(i'\,n\,t - i\,n\,t + j'\,m\,n\,t - j\,m\,n\,t + l'\,n\,t - l\,n\,t)}{(i' - i + j'\,m - j\,m + l' - l)\,n}$$

$$-\,\frac{D\,A}{2}\,\frac{\sin\,(i'\,n\,t + i\,n\,t + j'\,m\,n\,t + j\,m\,n\,t + l'\,n\,t + l\,n\,t)}{(i' + i + j'\,m + j\,m + l' + l)\,n}$$

$$\left(\frac{\mathrm{d}^2\,R}{\mathrm{d}\,e\,\mathrm{d}\,c}\right) = -\,i'\,A\sin\,(i'\,n\,t + j'\,m\,n\,t + l'\,n\,t)$$

$$\left(\frac{d^2 R}{d \omega \, d c}\right) = i'' B \cos (i'' n t + j'' m n t + l'' n t).$$

These terms give in $\frac{d a}{d t}$

$$\left(\frac{d^2 R}{d e \, d c}\right) \frac{\delta e}{a n} + \frac{d^2 R}{d \omega \, d c} \frac{\delta \omega}{a n},$$

and therefore

$$\left(\frac{d^2 R}{d e \, d c}\right) \int \frac{[e, \omega]}{a n} \left(\frac{d R}{d \omega}\right) d t + \left(\frac{d^2 R}{d \omega \, d c}\right) \int \frac{[\omega, e]}{a n} \left(\frac{d R}{d e}\right) d t$$

and hence the following terms:

$$i' C B A \sin (i' n t + j' m n t + l' n t) \frac{\cos (i'' n t + j'' m n t + l'' n t)}{(i'' + j'' m + l'') n}$$

$$+ \frac{i' D B A}{2} \sin (i' n t + j' m n t + l' n t) \frac{\cos (i'' n t - i n t + j'' m n t - j m n t + l'' n t - l n t)}{(i'' - i + j'' m - j m + l'' - l) n}$$

$$+ \frac{i' D B A}{2} \sin (i' n t + j' m n t + l' n t) \frac{\cos (i'' n t + i n t + j'' m n t + j m n t + l'' n t + l n t)}{(i'' + i + j'' m + j m + l'' + l) n}$$

$$- i'' C B A \cos (i'' n + j'' m n t + l'' n t) \frac{\sin (i' n t + j' m n t + l' n t)}{(i' + j' m + l') n}$$

$$- \frac{i'' D B A}{2} \cos (i'' n t + j'' m n t + l'' n t) \frac{\sin (i' n t - i n t + j' m n t - j m n t + l' n t - l' n t)}{(i' - i + j' m - j m + l' - l) n}$$

$$- \frac{i'' D B A}{2} \cos (i'' n t + j'' m n t + l'' n t) \frac{\sin (i' n t + i n t + j' m n t + j m n t + l' n t + l n t)}{(i' + i + j' m + j m + l' + l) n}.$$

The terms multiplied by $C B A$, which arise from the constant portion of $[e, \omega]$, have already been considered, the rest form four pairs, of which the following is one, and may be taken as a type of the rest:

$$\frac{DBA}{4n}\left\{\frac{i'}{i''-i+j''m-jm+l''-l}-\frac{i''}{i'+i+j'm+jm+l'+l}\right\}$$

$$\sin\left(i'nt+j'mnt+l'nt-i''nt+int-j''mnt+jmnt-l''nt+lnt\right).$$

In order that this argument may be of the kind under consideration, we must have

$$i'-i''+i=0$$
$$j'm-j''m+jm=0,$$

and the term becomes in that case

$$\frac{DBA}{4n}\left\{\frac{i'}{i''-i+j''m-jm+l''-l}\right.$$
$$\left.-\frac{i''}{i'+i+j'm+jm+l'+l}\right\}\sin\left(l'-l''+l\right)nt$$

if i is not equal to zero, D is of the order m^2, and the coefficient of $\sin(l'-l''+l)\,nt$, after a fresh integration, remains of the order m^4. If $i=0$

$$i'=i''$$

the coefficient becomes

$$\frac{i'DBA}{4n}\left\{\frac{j'm+jm+l'+l-j''m+jm-l''+l}{(i''+j''m-jm+l''-l)\,(i'+j'm+jm+l'+l)}\right\}$$

$$=\frac{i'DBA\,\{jm+l'-l''+2l\}}{4n\,\{i''+j''m-jm+l''-l)\,(i'+j'm+jm+l'-l)\}};$$

but unless $j=0$, D is multiplied by m, and in either case, in consequence of the reductions which the numerator undergoes, the coefficient after a fresh integration remains of the order m^4.

It remains now only to consider the terms of which the following is the type:

$$\left(\frac{d^2R}{d\,e\,d\,c}\right)\frac{dR}{dc}\int[c,\omega]\left(\frac{dR}{d\omega}\right),$$

but as in δe are here only included terms of the order m^2,

$$\int[e,\omega]\left(\frac{dR}{d\omega}\right)$$

is of the order m^2, or at least such portion of it as is supposed to be included in this expression; and as $\frac{d^2 R}{d\,e\,d\,c}$ and $\frac{d\,R}{d\,c}$ are each of the order m^2, this term is of the order m^6, which after a fresh integration remains of the order m^4.

Hence we may conclude with safety, without having recourse to any peculiar system of constants, or to any precarious induction, that however far the approximation be carried, the variation of the semi-axis major in the Lunar Theory contains no term *of long period* multiplied by a power of m inferior to the fourth.

Sept. 21, 1840.

LI. *Some Observations on the aqueous Solution of Carbonate of Magnesia with excess of Carbonic Acid, and on the Salt which it affords by spontaneous Decomposition.* By JOHN DAVY, *M.D., F.R.S.*[*]

I HAVE been induced to institute some experiments on the solution of carbonate of magnesia in water strongly impregnated with carbonic acid gas, in consequence of the high repute, on very questionable grounds, which it has lately acquired as a medicine.

The solution I have used is that prepared and sold by Mr. Dinneford of New Bond-street, with the designation of Dinneford's Solution of Magnesia, and with the following recommendation on the label: "The great advantages of this elegant preparation are, that being in a *fluid* state and possessing all the properties of magnesia in general use, it is not likely to form dangerous concretions in the bowels; it corrects acidity and heart-burn effectually, without injuring the coats of the stomach, as carbonates of potash and soda are known to do; it prevents the food of infants turning sour, and in all cases it acts as a pleasing aperient, particularly adapted for females."

Such a recommendation I should not have thought it right to notice, were it an ordinary quack eulogy, and unsupported by certificates given by respectable medical men; and moreover were I not assured that great faith is placed by many persons in the asserted virtues of the preparation, and that the use of it is rapidly extending.

The first trials I subjected the medicine to, were made with a view to test the permanence of the solution; as by exposure to the air in an open vessel, exposure to a temperature of 100° Fahrenheit, in a vessel loosely corked, and to the action of the air-pump under an exhausted receiver.

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[*] Communicated by Sir David Brewster.

The result in each instance was very similar; carbonic acid gas escaped, or was expelled, and a salt was deposited in the form of minute prismatic crystals.

This separation of the magnesia in a solid form, on the disengagement of the excess of carbonic acid, was no more than might have been expected from the known nature of the compound, and the artificial manner in which it is formed by the condensation of the gaseous acid; and must be considered as quite incompatible with the declaration of its "fluid state" in the stomach and bowels, and sufficient ground to call in question the propriety of placing confidence in the preparation as a medicine, in preference to common carbonate of magnesia or calcined magnesia, than either of which it is so much more costly an article.

The prismatic salt deposited on the escape of the excess of carbonic acid, has been examined by several chemists; resting chiefly on the results of the experiments of Berzelius, and the late Dr. Henry, it has been considered as a hydrated carbonate of magnesia, composed of one proportion of magnesia, one of carbonic acid, and three of water.

From the experiments which I have made on it, it appears to be composed as follows; viz.

29·61	Magnesia.
32·22	Carbonic acid.
10·27	Water expelled at 212° Fahrenheit.
27·90	Water expelled by a higher temperature, as by ignition.
100·00	

or of one proportion and half of magnesia, and carbonic acid, one of water expelled at 212°, and three proportions of water expelled by a higher temperature. Compared with the common carbonate of magnesia, from the results which I have obtained operating on the latter, this appears to differ chiefly from the former in possessing half a proportion more of magnesia, and one proportion less of water, being composed of

41·52	Magnesia.
33·31	Carbonic acid.
17·47	Water expelled at 212°.
7·70	Water expelled at a higher temperature.
100·00	

These results accord tolerably with those of other inquirers who have examined this compound; the variation or want of perfect accordance, probably chiefly depends on the degree of dryness of the preparation examined, or on the quantity of water retained in the powder admitting of expulsion at 212°, which water being hygrometrical, at least in part, must vary with the degree of dryness of the atmosphere to which it is exposed.

The method by which these two compounds of magnesia and carbonic acid were analysed was a simple one, admitting of considerable accuracy.

The quantity of water expelled at a temperature of 212° was determined by exposure of an hour or more to the heat of a steam-bath; the quantity of carbonic acid, by acting on the compounds, very carefully weighed, by muriatic acid, saturated with carbonic acid, over mercury in a graduated tube; and the quantities of magnesia and of water expelled at a higher temperature than 212° by the action of a red heat, continued for two or three hours, till no further loss of weight was produced by a continuance of the high temperature. In estimating the proportion of carbonic acid, the calculation was made on the ground that 100 cubic inches of this gas weigh 47·262 grains.

A few words relative to the properties of the first-mentioned carbonate. Its tendency to crystallize is remarkable: however obtained, even when rapidly separated by the expulsion of the excess of carbonic acid by heat, it has been deposited in a crystalline form. This form is not obvious to the naked eye; but when the powder is examined by the microscope, each particle is found to be a distinct prismatic crystal. And the persistence of this form is no less remarkable; it is not destroyed by decomposition; the powder after ignition, after the expulsion of the whole of the water and carbonic acid, under the microscope shows no alteration; each particle is still prismatic, and when moistened with water is transparent.

It is asserted that this carbonate readily loses the water with which it is combined. In a dry atmosphere it loses a portion of the water, which perhaps may be considered as hygrometrical, and at the same time loses its transparency; but I find, as has been already remarked, that a temperature of 212° expels only one portion, and that a high temperature is requisite to expel the three remaining proportions, and which are probably the strictly chemically combined water.

It is also said that this compound is altered by the action of cold water, and by that of boiling water; that in one instance a solution of bicarbonate of magnesia is formed, and an insoluble carbonate containing a smaller proportion of carbonic acid; and in the other, that the same insoluble subcarbonate is produced, but without the solution of bicarbonate, the proportion of carbonic acid required for this being expelled in the form of gas. The results of the trials I have made have not confirmed either of these conclusions. It has appeared to me to dissolve both in hot and in cold water, without undergoing any decomposition. I have not been able to obtain an insoluble subcarbonate of magnesia by acting on the prismatic salt by cold water, or carbonic acid gas from it by boiling water,—for instance, boiling it in distilled water in a retort connected with a mercurial pneumatic apparatus. It is true, that when this carbonate is thrown into hot water, there is a disengagement of air, but the air is common air mechanically entangled, not carbonic acid gas which had been chemically combined.

Both the hot solution and the cold, on evaporation, yielded the prismatic compound. 1000 grains of water at the temperature of 60° appear capable of holding in solution about four grains; thus 326·6 grains of the solution of carbonate, after the excess of carbonic acid gas had been expelled by the air-pump, afforded on spontaneous evaporation 1·5 grain of crystalline salt.

Whether this slight degree of solubility can be useful, considering the qualities of the compound as a medicine, or whether the crystalline spicular prismatic form which it assumes on separation of the excess of carbonic acid by which the carbonate was brought into solution can be injurious to the coats of the stomach, as a mechanical irritant, it is far from easy to determine; the probability is, reasoning analogically, that neither the one nor the other circumstance, medicinally considered, is of much consequence.

Fort Pitt, Chatham, Oct. 1, 1840.

LII. *An Abstract of Professor* Daniell's *Papers on the Electrolysis of Secondary Compounds, in the Philosophical Transactions for* 1839 *and* 1840.

IT has been long known that when a saline solution is subjected to the action of a galvanic current, both the water and the salt that it contains are resolved into their constituents; oxygen and the acid being evolved at the zincode, whilst hydrogen and the base appear at the platinode. The primary object of these researches was the determination of the relative proportions of these decompositions, and their relation to the amount of electrolytic force in action, with a view to increase our knowledge of the constitution of saline bodies in general.

For this purpose an apparatus was constructed in the following manner, which Mr. Daniell calls "the double diaphragm cell."

"It consists of two cells formed of two glass cylinders, with collars at their lower ends, fitted by grinding to a stout glass tube bent into the form of the letter U, and firmly fixed on a wooden foot. The ends of this piece project a little into the interior of the two cylinders, the upper extremities of which are furnished with bent tubes for the collection of gases. A stout piece of platinum wire is ground to the upper part of each cell, to which an electrode of platinum or any other metal can be screwed on the inside, as occasion may require: the wires pass down upon the outside, and terminate in two mercury cups, by which connexion can be made, at pleasure, with the battery. Each cell will hold about seven cubic inches of liquid, and the connecting tube two inches. When the cell is charged, the connecting tube is filled with the liquid, and a piece of fine bladder tied over each end, so as perfectly to exclude the air. The bladders are firmly confined to their places by means of circular grooves ground round the ends of the glass tube. The cylinders are then carefully fitted to their places, and filled with the proper quantities of the solutions to be acted upon, and after the operation their contents are easily decanted." The quantity of liquid in each cell during the experiment was about 4·5 cubic inches. The power employed was that of a small constant battery of Mr. Daniell's construction, containing thirty cells six inches in height, with tubes of porous earthenware, charged in the ordinary manner. (See Phil. Trans. for the year 1836.)

From this battery the current was made to pass through the apparatus just described, filled with a solution of the salt to be examined, say sulphate of soda. A common voltameter charged with dilute sulphuric acid, was also included in the circuit, so that the mixed gases evolved might be collected, in order to ascertain the exact amount of electrolytic force really in circulation.

The gas given off from each side from the double diaphragm cell was also collected, and the united bulk of the oxygen and hydrogen so evolved was found to be exactly equal to the volume of the mixed gases collected from the common voltameter. From numerous experiments it was found, that on decanting the saline liquid from each cell, and carefully neutralizing with acid or alkali, as the case might require, the quantity of salt decomposed was almost, if not exactly, equivalent to the gas evolved.

(Thus if 11·8 cubic inches of oxygen had been evolved from the zincode, and 23·6 cubic inches of hydrogen from the platinode [the results of the decomposition of 4·5 grains of water], by neutralization it was found that the zincode cell contained about 20 grains of free sulphuric acid, and the platinode 16 grains of free soda, numbers which are equivalent to the quantity of water decomposed; meantime from the single voltameter 35·4 cubic inches of mixed gases [also the result of the decomposition of 4·5 grains of water] had been collected).

These experiments were repeated upon *sulphate* of potass, *phosphate* of soda, sulphate of *ammonia*, showing the analogy of ammonia with metallic salts, and *nitrate* of potass, with corresponding results. The determination of the quantity of alkali in the latter instance was not possible, owing to the formation of a quantity of ammonia at the platinode, from the reaction of the disengaged hydrogen upon the nitric acid of the salt.

The *carbonates* were examined in a similar manner with like results. *Oxalate* of ammonia yielded nothing but pure carbonic acid at the zincode, whilst hydrogen and ammonia appeared at the platinode. The reason of this is evident from the following formula:—

$$\underbrace{(2\,C + 3\,O)}_{\text{Oxalic acid.}} + O = \underbrace{2\,(C + 2\,O)}_{\text{Carbonic acid.}}$$

the equivalent of oxygen afforded by the electrolytic decomposition of the salt being just sufficient to convert one equivalent of oxalic acid into two equivalents of carbonic acid.

Sulphovinate of potassa was decomposed also in equivalent proportions, the acid and oxygen passing to the zincode whence the gas escaped uncombined—hydrogen and potassa being as usual developed at the platinode. From the preceding experiments it appears, "that in the electrolysis of a solution of a neutral salt in water a current which is just sufficient to separate single equivalents of oxygen and hydrogen from a mixture of sulphuric acid and water, will separate single equivalents of oxygen and hydrogen from the saline solution, while single equivalents of acid and alkali will make their appearance at the same time at the respective electrodes."

These relations were found equally to hold good, whether the oxygen was allowed to escape from the zincode of the double diaphragm cell, or whether it was absorbed by an electrode of copper, or of zinc, as in the ordinary cells of the battery.

Further experiments showed that whenever dilute sulphuric acid is used, there is a transfer of acid towards the zincode, and the determination of the proportions in which such a transfer occurs led to some curious results, to which we must presently revert.

In order, however, to remove the ambiguity which might thus possibly be conceived to arise from the employment of dilute sulphuric acid as the measure of the electrolytic force, the following arrangement was substituted for the ordinary voltameter: a green glass tube (into the bottom of which, as platinode, was welded a weighed platinum wire) was filled with chloride of lead, maintained in a state of fusion by a spirit-lamp; the corresponding zincode was formed of plumbago. At the termination of the experiment the tube was broken, the wire and adhering button of lead weighed; and the result showed that "the same current which is just sufficient to resolve an equivalent of chloride of lead, which is a simple electrolyte unaffected by any associated composition, into its equivalent ions, produces the apparent phænomena of the resolution of water into its elements; and at the same time of an equivalent of sulphate of soda into its proximate principles."

Aqueous solutions of the chlorides were next tried, as the simple constitution of this class of salts promised to throw light upon the nature of the electrolysis of secondary compounds.

A weighed plate of pure tin was made the zincode of the double cell, which was charged with a strong solution of chloride of sodium, and a tube of fused chloride of lead, as before, included in the circuit; not a bubble of gas appeared on the tin electrode, and no smell of chlorine was perceptible, but hydrogen in equivalent proportion to the quantity of tin dissolved was given off at the platinode, and the cell contained an equivalent proportion of free soda. One equivalent of lead was reduced in the voltameter tube.

Muriate of ammonia treated in the same way gave precisely similar results, proving it to be "an electrolyte, whose simple anion was chlorine, and compound cathion nitrogen with 4 equivalents of hydrogen. Its electrolytic symbol, therefore, instead of being

$$(\mathrm{Cl} + \mathrm{H}) + (\mathrm{N} + 3\,\mathrm{H}), \text{ is } \mathrm{Cl} + (\mathrm{N} + 4\,\mathrm{H})."$$

Strikingly confirming the hypothesis of Berzelius of the base $(\mathrm{N} + 4\,\mathrm{H})$ called ammonium.

In discussing the results of all these experiments, we must bear in mind the fundamental principle, "that the force which we have measured by its definite action at any one point of a circuit cannot perform more than an equivalent proportion of work at any other point of the same circuit."—"The sum of the forces which held together any number of ions in a compound electrolyte, could, moreover, only have been equal to the force which held together the elements of a single electrolyte, electrolyzed at the same moment in one circuit."

In the electrolysis of the solution of sulphate of soda, and many of the other salts, "water seemed to be electrolyzed; at the same time acid and alkali appeared in equivalent proportion with the oxygen and hydrogen at the respective electrodes."—"We must conclude," from the above-mentioned principle, "that the only electrolyte which yielded was the sulphate of soda, the ions of which, however, were not the acid and alkali of the salt, but an anion composed of an equivalent of sulphur and four equivalents of oxygen and the metallic cathion sodium; from the former, sulphuric acid was formed at the anode by the secondary action and evolution of one equivalent of oxygen; and from the latter, soda at the cathode by the secondary action of the metal and the evolution of an equivalent of hydrogen."

To avoid circumlocution (but only when speaking of electrolytic decomposition), Mr. Daniell proposes to adopt the word *ion*, introduced by Dr. Faraday, as a general termination to denote the compounds which in the electrolysis of a salt pass to the zincode, and that they should be specifically distinguished by prefixing the name of the acid slightly modified, as is shown in the following table:—

	Ordinary chemical formula.		Electrolytic formula.	
Sulphate of copper	$(S+3O)+(Cu+O)$	$=$	$(S+4O)+Cu$.	Oxysulphion of copper.
Sulphate of soda	$(S+3O)+(Na+O)$	$=$	$(S+4O)+Na$.	Oxysulph. of sodium.
Nitrate of potassa	$(N+5O)+(Ka+O)$	$=$	$(N+6O)+Ka$.	Oxynitrion of potassa.
Phosphate of soda	$(P+3\frac{1}{2}O)+(Na+O)$	$=$	$(P+3\frac{1}{2}O)+Na$.	Oxyphosph. of soda.

The following experiments seem to remove all doubt that the view just sketched is correct; they were, in fact, suggested to Prof. Daniell by the theory itself.

"A small glass bell, with an aperture at top, had its mouth closed by tying a piece of thin membrane over it. It was half filled with a dilute solution of caustic potassa, and suspended in a glass vessel containing a strong neutral solution of sulphate of copper, below the surface of which it just dipped. A platinum electrode, connected with the last zinc rod of a large constant battery of twenty cells, was placed in the solution of potassa; and another, connected with the copper of the first cell, was placed in the sulphate of copper immediately under the diaphragm which separated the two solutions. The circuit conducted very readily, and the action was very energetic. Hydrogen was given off at the platinode in a solution of potassa, and oxygen at the zincode in the sulphate of copper. A small quantity of gas was also seen to rise from the surface of the diaphragm. In about ten minutes the lower surface of the membrane was found beautifully coated with metallic copper, interspersed with oxide of copper of a black colour, and hydrated oxide of copper of a light blue.

"The explanation of these phænomena is obvious. In the experimental cell we have two electrolytes separated by a membrane, through both of which the current must pass to complete its circuit. The sulphate of copper is resolved into its compound anion, sulphuric acid + oxygen (oxysulphion), and its simple cathion copper: the oxygen of the former escapes at the zincode, but the copper on its passage to the platinode is stopped at the surface of the second electrolyte, which for the present we may regard as water improved in its conducting power by potassa. The metal here finds nothing by combining with which it can complete its course, but being forced to stop, yields up its charge to the hydrogen of the second electrolyte, which passes on to the platinode, and is evolved.

"The corresponding oxygen stops also at the diaphragm, giving up its charge to the anion of the sulphate of copper. The copper and oxygen thus meeting at the intermediate point, partly enter into combination, and form the black oxide; but from the rapidity of the action, there is not time for the whole to combine, and a portion of the copper remains in the metallic state, and a portion of the gaseous oxygen escapes. The precipitation of blue hydrated oxide doubtless arose from the mixing of a small portion of the two solutions."

Nitrate of silver, nitrate of lead, proto-sulphate of iron, sulphate of palladium, and proto-nitrate of mercury, were similarly treated, and afforded analogous results, somewhat modified by the nature of the metallic base. Sulphate of magnesia was subjected to the same process, in hopes of finding magnesium, but magnesia alone was deposited.

The theory of ammonium, as proposed by Berzelius, and the hypothesis of Davy developing the general analogy of all salts, whether derived from oxyacids or hydracids, may by this evidence, especially when taken in conjunction with the recent researches on the constitution of organic bodies, be considered as almost experimentally demonstrated*.

The bisalts yield results which at first sight do not accord with the preceding deductions; a strong solution, for example, of pure crystallized bisulphate of potassa was made, and its neutralizing power carefully ascertained by the alkalimeter. Evaporation and ignition with carbonate of ammonia gave the quantity of neutral sulphate yielded by a certain measure of the solution. An equal measure was then placed in each arm of the double diaphragm cell, and the current passed through till 70·8 cubic inches of mixed gas were collected; half the solutions from the zincode and platinode were then separately neutralized, and half evaporated and ignited in the vapour of carbonate of ammonia.

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* See Additional Note at the end of the present Number among the Miscellaneous Notices.

It was then found that the zincode had gained 18 grains; the platinode lost 19 of free acid: of potassa the zincode had lost 9·9 grains, and the platinode gained an equal quantity: thus, though the solution conducted very well, not more than one-fifth of an equivalent of the potassa was transferred to the platinode, as compared with the hydrogen evolved, while half an equivalent of acid was transferred to the zincode when where a whole equivalent of oxygen was evolved. Mr. Daniell remarks upon this experiment,—

"I think we cannot hesitate to admit that, in this case, the current divided itself between two electrolytes, and that a part was conducted by the neutral sulphate of potassa, and a larger part by the sulphuric acid and water. It is a well-known fact that the voltaic current will divide itself between two or more metallic conductors in inverse proportion to the resistance which each may offer to its course; and that it does not in such cases choose *alone* the path of least resistance. I am not aware that such a division of a current between two electrolytes in the same solution has ever before been pointed out, but analogy would lead me to expect it." These considerations enable us to explain some apparent anomalies in the electrolysis of diluted sulphuric acid and alkaline solutions.

When diluted sulphuric acid was placed in the double diaphragm cell, and the current transmitted, some of the acid passed to the zincode; but from numberless experiments it appeared that this quantity scarcely ever exceeded the proportion of one-fourth of an equivalent as compared with the hydrogen evolved. Mr. Daniell thought possibly this might be owing to the acid being mechanically carried back to the platinode, as in all cases there is a mechanical convection of the liquid from the zincode to the platinode, and this is the greater in proportion to the inferiority of its conducting power. If, however, this deficiency of acid were owing to a mechanical re-transfer, mechanical means, such as increasing the number of diaphragms, would stop it; the proportion, however, was even under these circumstances still maintained. No difference was observed whether the oxygen was allowed to escape as from a zincode of platinum, or was absorbed by copper or zinc; the metals, of course, being dissolved in proportions equivalent to the hydrogen developed at the platinode. Solution of potassa, baryta, or strontia, similarly treated, exhibited a transfer of about one-fourth of an equivalent towards the *platinode*.

These curious results are easily explained by supposing that the solution is a mixture of two electrolytes; with sulphuric acid they are $H + (S + 4O)$, oxysulphion of hydrogen $(H + O)$ water; the current so divides itself that three equivalents of water are decomposed, and one equivalent of oxysulphion of hydrogen. Analogous changes occur with the alkaline solutions, the alkaline metal passing as usual to the platinode.

LIII. *On Magneto-electric Induction; in a Letter to M.* Gay-Lussac. *By* Michael Faraday, *D.C.L., F.R.S.*

[Continued from p. 289, and concluded.]

I REPRESENTED this state of things under a general form, in the figures *ij* annexed to the memoir, which, as to the arrows, the designation of the parts, &c. &c., I have made to correspond, as well as I could, with fig. 2. plate iii. of the Italian philosophers' memoir (plate ii.). I proceed to show how it agrees with the galvanometrical results obtained by them, and how far with their *conclusions*.

With regard to the galvanometrical results, my figure might be used instead of theirs, without occasioning any difference, and I have no reason to say that they are inexact. Relatively to "one of those consequences," which arises from "the immediate inspection of the arrows which mark the currents in the two regions of the disc," or from any other attentive and experimental examination, we see that the currents *n*, *n*, *n*, on entering, instead of being in a contrary direction to those which are in the parts *s s s*, which recede, follow exactly the same direction; that is, that as to the general movement near the pole they go from above below, or from the circumference towards the centre, transversely to the lines that the different parts describe in their course; and at a great distance (F. 92.) on each side of the pole they are in the contrary direction. In proportion to the nearness to the pole of a part of the line described by a point, it is traversed by a current, which commences, and increases in intensity until it reaches the shortest distance, or a little beyond, on account of time entering as an element into this effect. Afterwards, by reason of the increasing distance, the current diminishes in intensity without ever altering its direction relatively to its proper course. It is only when it arrives at the parts most distant, at which the electricity excited is discharged, that a current is manifested in an *opposite* direction, or in one more or less oblique. I apprehend that it is wholly useless to speak of the partial alterations in the direction of the currents through the parts that are the nearest to the centre, or to the circumference; two or three curves that I have rudely traced will show in what directions these alterations take place.

The second consequence arising from the memoir of the Italian philosophers is, that "the direction of the currents upon the parts that enter is contrary to that of the producing currents; (that is, of those that are considered as existing in the magnet) while on the other side the direction in the two systems is identical." This assertion is exactly contrary to the reality (F. 117.). In figures 1. and 2. I have indicated, by means of arrows, the direction of the currents in the magnetic pole, which is the same as the direction given by Messrs. Nobili and Antenori in fig. 1. pl. iii. But my figure 2, as well as the indications of the galvanometer, shows evidently that the currents in the parts that enter *n*, *n*, *n*, when they approach the magnet, pass through in the same direction as the current in this side of the pole of the magnet; and that the currents in the parts that recede *s*, *s*, *s*, follow a direction contrary to those supposed to exist in the side of the magnetic pole from which they recede.

I may be mistaken, but it appears to me that Messrs. Nobili and Antenori suppose that circular currents are excited in the part of the metal adjacent to the pole, in absolutely the same manner as those formed in the helix, when it is made to approach the magnet, and that when this part of the disc recedes, the circular currents are somehow reversed, as occurs in the helix during its recession from the magnet. A passage in their first paper, and another at the end of page 284, appear to imply that such is their opinion. This idea occurred to me above a year ago, but I soon saw from numerous experiments, some of which I have just referred to, that it was by no means satisfactory; and when I had fully verified that the action of the helix in its approach to, and recession from the pole was wholly explained (F. 42.) by the law assigned (F. 114.), I was forced to abandon my previous ideas.

The memoir afterwards proceeds (p. 288.) to explain the phænomena of Arago's revolving disc; but as I have shown that the theory is in general based upon two conclusions contrary to truth, it is unnecessary to make a minute examination of it. It is impossible for it to exhibit the phænomena with exactitude. Those who are anxious for full satisfaction on the subject, may decide, by means of a few experiments, whether the opinions which I put forth in the paper which first announced the discovery of these currents be true, or whether the Italian philosophers were justified in declaring that I was in error, and that they had published more just ideas on the subject.

Everybody knows that when M. Arago published his remarkable discovery, he said the action of the disc upon the magnet was resolvable into three forces: the FIRST, perpendicular to the disc, which he found to be repulsive: the SECOND, horizontal and perpendicular to the vertical plane containing the radius beneath the magnetic pole; this is a tangential force, and occasions the rotation of the pole with the metal: the THIRD, horizontal and parallel to the same radius; it becomes null at a certain point towards the circumference; but when nearer the centre, it has a tendency to impel the pole towards the centre; and when nearer the circumference, to impel it in the contrary direction.

At page 289, Messrs. Nobili and Antenori give an explanation of the first of these forces. As has been already said, these gentlemen consider that the parts adjacent to the magnet have currents contrary to those which are found near the pole to which they approach, and consequently they are repulsive; and they consider that the parts that recede have currents identical in direction with those which are near the magnet from which they recede, and consequently these parts are attractive. The sum of each of these various forces is equal one to the other, but in what relates to the needle or magnet this distribution differs; for "the repulsive forces being the nearest, invade the disc as far as the parts under the needle, and thus obtain a preponderance over the action of the contrary forces, which are exerted more obliquely, and at a greater distance. In short, it is only a part of the repulsive forces which is balanced by the attractive forces; the remainder meets with no opposition, and it is this remainder that produces the effect."

But I have shown in this letter, that the currents in the parts adjacent or distant are exactly contrary to what is supposed by Messrs. Nobili and Antenori; and that consequently where they expect attraction they would find repulsion, and attraction where they expected repulsion; so that, following their opinion, corrected by experiment, the result should be *attraction* instead of *repulsion*. But Arago was right in saying that it is repulsion; and consequently the theory of the effect given cannot be the true one.

My views upon the subject in question may be found in my first paper. I examined whether it were possible or probable (F. 125.) that time could be a necessary element for the development of the maximum current in the metal. In this case the resultant of all the forces would be in advance of the magnet, when the plate was rotated, or in the rear of it, if it (the magnet) were rotated; and a line joining this resultant with the pole would be oblique to the plane of motion; then the force in the direction of this line might be resolved into two others, one parallel, the other perpendicular to the plane of movement or rotation; the latter would be a repulsive force, producing an effect analogous to that remarked by M. Arago.

The second force is that which occasions the magnet and the disc mutually to follow each other. Referring to page 290, fig. 1. or 2. (my figure 2. may also be made use of,) we read, "Forces of attraction exist in *s*, *s*, *s*, towards which it (the magnet) is attracted, and repulsive forces in *n*, *n*, *n*, which impel it in the same direction;" consequently the magnet moves either after, or with the metal; but the currents, and consequently the forces, are exactly contrary to what has been supposed, as I have just shown; the magnet and the disc should therefore move in opposite directions, if the forces act in the manner that has been supposed. But as they do not move, in fact, in opposite directions, it is evident that the theory which explains their movement by reversing the facts must be itself erroneous.

The third force is that which has a tendency to remove the magnetic pole either towards the centre or the circumference, on each side of a neutral point situated upon the radius above which the magnet is placed; this effect is described at page 281, and in fig. 4, which accompanies the memoir, and which I believe to be perfectly correct. The memoir proceeds to explain the effect by referring to the repulsive force admitted (p. 289.) to account for the first effect observed by Arago, viz. the vertical repulsion of the disc; and supposing that this repulsive force be distributed over a certain extent of the disc, beneath the magnet, it is established (p. 292. fig. 5.) that if the pole be situated very near the circumference, the portion of the body whence this force emanates will be lessened, being cut by the circumference itself; consequently the parts that are nearest to the centre are more powerful, and impel the pole in an outward direction; while if the pole be placed very near the centre, the extent whence the force emanates will pass it; and as this part in excess is considered, though erroneously, as inactive, the portion situated towards the circumference is more powerful, and impels the pole towards the centre.

Two or three slight objections present themselves to this opinion, but they are nothing, so to speak, in comparison with that which arises, when it is recollected, that in conformity with the author's own ideas upon the action of currents, the error with respect to the direction of those which are excited near the pole obliges us to substitute *attraction* for *repulsion*, as I have already shown when treating of the first of these forces: consequently all the movements which are connected with the third force would be in a direction contrary to those that are actually presented; and the theory which, when corrected by experiments made with the galvanometer, indicates such movements, must be abandoned.

Page 292 of the memoir refers to Mr. Faraday's "second law." As I have already said, I never stated those three assertions as laws. I really regret extremely that a letter that was never intended to convey minute details, but merely a few facts, selected in haste from a multitude described previously in the memoir read before the Royal Society,—I regret that this letter, which I never expected to see in print, should have led the Italian philosophers into error. However, after having examined anew all the facts, I do not see that I am in any degree responsible for the error they have committed, as having advanced fallacious results; nor, as far as the memoir is concerned, for not having given to the scientific world the most complete details at the earliest period possible.

I have not yet published my views as to the cause of the third force described by M. Arago; but as Messrs. Nobili and Antenori, when giving the hypotheses, which I justly regard as inexact, say (p. 293.), "In fact, what other hypothesis can reconcile the verticality that the needle preserves in the two positions n, s, n'', s'', (fig. 4) with the fact of the repulsion from below, above which raises the needle in the second position s'', n''?",—I am induced to offer another hypothesis, premising, however, that the directions and forms that I shall trace, as those of the excited magneto-electric currents, are to be considered only as general approximations.

If a piece of metal, large enough to contain without distortion all the currents which may be excited in its whole extent by a magnetic pole placed above it, be moved in a rectilinear direction beneath the pole, then an electric current will move across the direction of its motion, in the parts immediately adjacent to the pole, and will return in the opposite direction on each side in the parts which, being more distant from the pole, are subject to a feebler inductive force: the current will thus be completed or discharged (see fig. 3.). Let A B C D represent a piece of copper moving in the direction of the arrow E, and N the north end of the magnet placed above; currents of electricity will be produced in the piece of metal; and though they undoubtedly extend from the part below the pole to a great distance around (F. 92.), and at the same time diminish in intensity and alter in direction as they recede thence, yet the two circles may serve to represent the resultant of these currents; and it will be evident that the point of most intense action will be where they touch, and immediately under the magnetic pole, or, on account of the time required, a little in advance of it. Hence that portion of the forces which acts parallel to the plane of the metal will carry the pole forward in the direction of the arrow E, because the forces are equally powerful on the side of the pole A B, as on the side C D; and this portion, which on account of the time necessary for the production of the currents excited is perpendicular to the direction of the metal, will be, as we have said, repulsive, and have a tendency to impel the pole upwards and away.

But suppose that instead of the metal moving in a rectilinear direction, a circular disc revolving upon its axis be substituted; and then let us consider, in the first place, the case of the magnetic pole placed upon its centre (fig. 4.); there is then no production of electric currents, not because there is no tendency to their formation, for I have stated in this letter, and shown in my memoirs (F. 149. 156. 217.), that from the time the disc begins to move, currents are also ready to move; but because they have a tendency to be formed in the direction of radii from the circumference to the centre; and as all the parts are equally influenced, none of them having an excess of power over the others, and all equally distant from the centre, no discharge can take place, and consequently no current can be developed. As no current can exist, no effect dependent on the action of a current upon the pole can be produced, and consequently there is then neither *revolution* nor repulsion of the magnet. Hence the cause of the *verticality without repulsion* which occurs at this place.

Let us now consider the case in which the pole of the magnet, instead of being placed over the centre of the metal, is at one of its sides, as in N, figure 5. The tendency to form electric currents is due to the movement of the parts of the disc *through* the magnetic curves (F. 116. 217.), and when these curves are of equal intensity, the electric currents increase in force in proportion to the increase of rapidity in the motion of the parts of the disc that intersect the magnetic curves (F. 258.). Let us now trace a circle *a b* around a magnetic pole as a centre, and it will represent the projection of magnetic curves of *equal intensity* upon the disc; *a* and *b* are points situated at an equal distance from the pole, in the passing radius which is immediately under the pole; but as the part or point *a* passes by the pole with much greater velocity than the part *b*, the intensity of the electric current which is excited in this part *a* is proportionably greater. This is also true for the points in any other radius of the revolving plate cutting the circle *a b*, and true likewise for any other circle traced round N as a centre, and representing consequently magnetic curves of equal intensity; with the exception, that when the circle extends beyond the centre C of the revolving disc, as to *c d*, instead of the existence of a feebler current at the point *d* than at the point *c*, there is then a tendency to produce an opposite current.

The natural consequence of these actions of the different parts is, that as the sum of the forces tending to produce the electric current in the direction from *c* to *d* is greater on the side *c* of the magnetic pole than on the side *d*, the curvature or return of these currents by the right and left also commences on this side; and then the two circles, which as before may be considered as representing the resultants of these currents, do not come into contact exactly under the pole, but at a greater or less distance from it, towards the circumference, as in figure 6.

This circumstance of itself would not occasion any movement in a pole restrained in its motion to the direction of the radius only; but being combined with that which results from the *time* necessary for the development of the current, and to which reference has been already made, as explaining the *first* of the three forces by which M. Arago exhibits the action of the magnetic pole and disc, it will, I hope, fully elucidate all the effects that we are investigating, and will also prove the influence of time as an element. Let *c* (fig. 7.) be the centre of a revolving disc, and *r c* a part of the radius under the magnetic pole *p*; the contact of the two circles representing the currents is, as we have just seen, on the side of the pole beyond the centre *c*; but on account of the element of time and the direction of the rotation R of the plate of metal, it is also a little to the left of the radius *r c*; so that the pole is brought under the action of the two orders of currents, not symmetrically but obliquely. The necessary consequence is, that if it be free to move in the direction of the radius, and in that direction alone, it will move towards the centre *c*, for the currents produced by a marked pole (north) are exactly such as by their action on the pole to impel it in that direction.

This relation of the currents to the pole by which they are generated, may be proved by experiment as easily as by calculation. I have shown (F. 100.) that when a pole marked north is above a disc revolving in the direction of the arrows R, in the figures annexed either to Messrs. Nobili and Antenori's memoir or to mine, the currents (indicated by the circles) are as is represented in figures 3, 6, or 7. Upon arranging a metal wire which would conduct the currents in this double direction (fig. 8.), and placing over it a marked pole (north) capable of moving only in a parallel direction to *r c*, at any point in the line *r c*, I found it had not any tendency to move. There was also another line perpendicular to the first, and which crosses it at the point of contact of the circles, in which the pole had no tendency to move. If placed in any other situation than upon these two lines, it moved either in one direction or the other; and when placed in the positions marked 1, 2, 3, 4, it moved in the direction of the arrows represented at those points. Now the position of the pole, with regard to the currents in Arago's experiment, when the magnet and the disc are arranged as in figures 5 and 7, is exactly that of the point 1 in fig. 8, and hence that pole has a tendency towards the centre C.

We will now direct our attention to the result obtained if we gradually move the pole from the centre towards the circumference. Let figure 9. represent this new condition at a given time, as figure 5. represented the first state; it is evident that the velocity of the parts *a b* of the radius beneath the pole, will not differ from each other so much as they did previously, being only about 3 : 2 instead of 6 : 1; and the difference will also be less with all the curves of equal intensity comprised in this circle. This occasions the situation of the pole, and the place of contact of the circles representing the currents (fig. 7.) mutually to approach in the direction of the line *r c*, and necessarily carries the pole (fig. 8) nearer to the neutral line *l i*. If we examine the second circle *c d*, fig. 9, of magnetic curves of equal intensity, it will be seen, that as the disc does not extend to *c*, or even beyond *a*, there is nothing to add to the force of the current upon that side of the pole, while at *d* the radius, by moving through the magnetic curves, adds to the intensity of the current excited at *b*, and everywhere else on that side of the pole, and may easily, according to the position of the pole upon the metal plate (that is, according as it is nearer or further from the edge), render their sum equal or greater than the sum of the forces on the other side, or that towards the circumference. If the sum of the forces be equal, then the pole will be somewhere in the line *l i*, as at 5, fig. 8, and will have no tendency either towards the centre or the circumference, though its tendency to move with the disc or above it remains the same. Or if the sum of the forces be greater on the side *d*, fig. 9, than on the side *c*, then the pole will be in the position 2, fig. 8, and will be impelled outwards in the direction of the radius, in conformity with Arago's results.

Besides this cause of alteration in the motion of the pole parallel to the radius, and which is dependent on the position of the pole near the circumference, there is another cause that occurs, I apprehend, at the same time, and assists the action of the first. When the pole is placed towards the edge of the disc, the discharge of the currents excited behind is thrown against the side of the edge, from the absence of conducting matter; thus, in fig. 10, instead of having the regular form of the figures 7 and 8, the currents are deflected in their course towards the circumference, while they have all necessary latitude for their movement in the parts towards the centre; this of itself would cause the point of greatest force to fall a little nearer the centre than the projection of the axis of the magnetic pole, and assist in placing the pole in the position 2, fig. 8. I have such confidence in this opinion, that though I have not had opportunity to make the experiment myself, yet I venture to predict, that if instead of employing a revolving disc, a lamina or plate of metal, five or six inches broad, as A, B, C, D, fig. 9, were caused to move in a rectilinear direction conformably to the arrow, under a magnetic pole situated at *a*, the pole would have a tendency to move forward with the metal as well as above, but neither towards the right nor left; while if the pole were placed above the point *b*, it would be directed towards the edge A B; or if it were placed above *c*, it would have a tendency to move towards the edge C D.

Having thus replied to the question, "What other hypothesis"?, &c. proposed by the authors of the memoir at p. 293, I shall continue my examination of the memoir itself. At p. 294 the error relative to the nature of the currents, that is their supposed inversion, is repeated. The effect described is sure enough with a helix, and some particular forms of apparatus; but the simple and elementary current generated by the passage of a wire in front of a magnet is not reversed when the metal wire recedes. (F. 171. 111. 92.)

At p. 295 is the supposition that when the rotation is slow "the revolution of the currents is circumscribed within narrow limits, and there is *little to add* to the results that form the basis of the [our] whole theory;" but that when the motion is rapid the currents envelope the whole disc, "so as to become a species of labyrinth." For my part I believe the currents have the same general direction as has been assigned to them in the figures, whether the rotation be slow or rapid; the only difference is an increase of velocity.

A circumstance is then selected which is really simple, though it may at first appear complicated; namely, that in which the opposite poles are adjusted over a disc in one diameter, but towards the opposite edges on each side of the centre. This circumstance, with the direction of the movement and the current produced, is exhibited in fig. 7 of Messrs. Nobili and Antenori's memoir. It is unnecessary to quote pages 296 and 297, which contain the explanation of this figure, but I shall refer to fig. 12, which corresponds to it, and is in conformity with my views and experiments, so that the two may be compared together. It is very satisfactory to me to find, that in this part of the memoir, as well as in the first, there is no important result of experiment contrary to my published opinions, though I am very far from adopting the conclusions that have been drawn from them.

If figure 12 be examined, it will be instantly seen that it results in the most simple manner from the action of the two poles. Thus, as far as the upper or north pole only is concerned, the currents are as in figure 6. But as with the north pole, the current determined by it moves from the circumference towards the centre, so with the south pole, in the same or corresponding position, the currents move from the centre to the circumference (F. 100.); and consequently in fig. 12 they are continued along the diameter N, S, through the centre of the plate, to return in the direction of the arrows upon the sides E, O. The points upon which I find my views to disagree with the indications of the galvanometer obtained by Messrs Nobili and Antenori are, first, the direction of the currents at N and S, which is contrary to what I obtained; and, secondly, the existence of any oblique axis of power, as P, Q, in their figure 7.

The memoir concludes, at least as far as I am concerned, at page 298, by again mentioning the error (but not as an error) relative to the revolving disc, which becomes a *new electrical machine*. At the commencement, the authors being little conversant with the principles under the influence of which such a result is obtained, deny it; and though they say here, "What shall we say after the *new observations* that we have made during the continuation of our researches?"—I am not disposed to alter anything that I have published; I have even more confidence than before in my own views; for had their observations been in agreement with the results which I had obtained, I should have had great reason, after my examination of their papers, to fear that my own ideas were erroneous.

I cannot conclude this letter without again expressing my regret at having been obliged to write it; but if it be recollected that Messrs. Nobili and Antenori's memoirs were written and published *after my* original memoirs; that their last paper appeared even in the *Annales de Chimie et de Physique* after mine; and that it had consequently the appearance of advancing the science further than I had done; that both papers accuse me of error in experiment and theory, and also of dishonesty; that the last of these papers is dated in March, and though it is now December, has been followed by no correction or retractation on the part of the authors; and that I sent them several months ago (at the same time that I forwarded them to you and others,) copies of my original memoirs, and of my notes to a translation of their first memoir; and if it be considered that, after all, I have not to reproach myself with the errors of which I am accused, and that these gentlemen's memoirs are so framed as to compel me to reply to their objections;—I hope that no one will say that I have written too hastily what might have been avoided; or that I should have shown respect for the truth, and done justice to my own publications, or to this branch of science, if, being aware of such important errors, I had not called attention to them. I am, my dear Sir, yours very sincerely,

M. FARADAY.

LIV. *On the Law of Storms.* *By* H. W. DOVE.

To Richard Taylor, Esq.
Editor of the Philosophical Magazine and Journal.

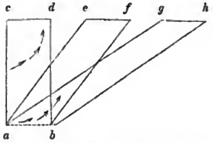
DEAR SIR,

IN the year 1828, I published in Poggendorff's *Annalen*, vol. xiii. p. 596, a memoir "On Barometric Minima," in which I established the fact, that the storm which accompanies a great depression of the barometric column is a vast whirlwind, which in the northern hemisphere proceeds from S.W. to N.E. The example there more especially investigated is the storm of the 24th of December, 1821, the centre of which travelled from Brest to Cap Lindenaes in Norway. The rotation in this whirlwind was in the direction S.E. N.W., consequently on the S.E. side of the storm, that is to say, in France, Holland, Germany, Italy, Denmark, and Prussia the weather-vane veered from S.E. to S.W. and W. through south; on the contrary, towards the Atlantic coast of North America the direction was N.E. At the same time I observed in this memoir (p. 599.), that the greater number of hurricanes in the southern hemisphere, which I had examined, are whirlwinds rotating in the opposite direction.

Three years later, Mr. Redfield of New York, arrived at the same result by independent observations, as appears from his memoir, entitled " Remarks on the prevailing Storms of the Atlantic Coast of the North American States." (Silliman's Amer. Journ. of Sc., 1831. Avril.) In a later memoir, however, on the gales and hurricanes of the western Atlantic (*ib.* vol. xxxi.), Mr. Redfield has added a new and weighty fact to those already accumulated by me. From the storms, the course of which the American philosopher has there discussed and delineated by a chart, it follows, that the hurricanes taking their rise within the tropics, so long as they are confined between these limits, retain unaltered their original direction from S.E. to N.W.; so soon, however, as they reach the temperate zones, they suddenly bend round almost at a right angle. and then travel from S.W. to N.E. Finally, Lieut.-Colonel Reid, in his valuable treatise " On the Law of Storms," published in 1838, confirms by new examples the results already obtained, and especially calls attention to the fact, that by this change in the direction of its course the whirlwind spreads itself out continually from the centre more and more.

So long ago as the year 1735, Hadley proposed to solve the problem of the trade-winds, upon the principle, that air moving from the equator to the poles gradually acquires a westerly, and on the contrary air moving from the poles to the equator an easterly direction. A simple modification, or rather extension of this theory, gives a key to all the complicated phænomena of the variable and apparently so irregular motions of the wind observed in our own and other extra-tropical latitudes. It is only necessary to take into consideration the two currents contending for and alternately obtaining the upper hand, in order to see that the origin of the current, which Hadley treated as fixed for a given place, is in fact variable; wherefrom it follows, that the direction of the vane ought to be not stationary, but changeable, according to a law which I have named the law of rotation. From the same fruitful principle I am now prepared to explain theoretically the phænomena of storms.

Let *a b* be a series of material points parallel to the equator, which are set in motion by a certain impulse in the direction *a t* from south to north. The rotation of the earth combined with this impulse will produce a motion of *a b* towards *g h*, if the space *d b h* is void of matter. But if this space is filled by quiescent matter, the particles at *b* will, as they move, come into contact with particles in the space *d b h*, which rotate with less velocity; their motion in the direction of east will therefore be retarded, and the point *b* will move not towards *h* but towards *f*. The particles at *a* are, on the contrary, in juxtaposition with particles, which at first have an equal velocity of rotation, and consequently move as they would in a vacuum, that is, towards *g*. If, then, *a b* represent a mass of air impelled from south to north, the storm will have a more southerly direction at the east side, a more westerly one on the west side, and will thus acquire a tendency to whirl in the direction S.E. N.W. This tendency to whirl would not take place, were there no resisting matter in the space *d b h*, and will therefore increase in proportion as this resistance prevents the course of the storm from deviating towards the west. Now within the north tropical regions the space *d b h* is filled with air, which flows from N.E. to S.W. Here, therefore, the resistance is at a maximum, and the air at *b* has its westerly tendency so far checked, that it retains its original direction towards *d* almost unaltered, whilst, on the other hand, the air at *a* has acquired a tendency to move towards *d*. The storm accordingly will whirl with the greatest intensity, but retains its initial direction and lateral magnitude. So soon, however, as it reaches the temperate zone, it finds itself in contact with air at *d b h*, which is in motion from S.W. to N.E. The resistance, which the particles at *b* experience, will therefore be considerably diminished, or even almost vanish, that is to say, the direction *b d* is transformed into the direction *b h*, and the storm bends round almost at a right



angle, and at the time grows wider and wider as it progresses.

The phænomena of storms south of the equator may easily be inferred from what has been said above of the opposite hemisphere. The rectilinear course within the tropics, the sudden curvature at the limits of the tropics and the temperate zones, the accompanying expansion of the whirls constituting the storm; in a word, all the essential phænomena of storms, must clearly be the same for one hemisphere as for the other, with the sole exception, that in the one (the northern) the rotation is after the order of the letters S. E. N. W., and in the other (the southern) after the order of the letters S. W. N. E.

I take this opportunity to make, in my own vindication, a few remarks upon the manner in which my labours in this field have been brought under the notice of the English public. In the Lond. and Edinb. Phil. Mag., vol. xi. p. 390, a paper by Mr. Dalton has appeared, in which I am directly charged with claiming for myself a theory which he had already many years before made known, and which had still earlier been promulgated by the celebrated Hadley. Upon a diligent perusal of Mr. Dalton's Meteorological Essays and Observations, and of Hadley's original Memoir (The Cause of the general Trade-wind, Phil. Trans. 1735, p. 58), I have not succeeded in finding a single trace of, or bare allusion to, the existence of the law of rotation, which it was the principal object of my paper to establish by observations, and explain upon theoretical principles. And if your readers or Mr. Dalton should do me the honour to look into my Meteorological Essays, Berlin, 1837–38, pp. 244–250, it will be seen, that so far from attempting to usurp the credit so justly due to Hadley for the fundamental idea, upon which my own theory is founded, and which he had himself so successfully applied to the particular problem of the trade-winds, I have been anxious to acknowledge the full extent of my obligations to him, and to bring his merits as a discoverer prominently forward.

In an article upon Lieut.-Colonel Reid's law of storms in the Edinburgh Review, I find my meteorological researches again alluded to, but upon a distinct ground. The anonymous Reviewer, in his patriotic anxiety to satisfy his readers of the purely British growth of this theory, allows that some remarkable passages upon the subject had previously appeared in the memoirs of the Berlin Professor, but that these are mere ingenious speculations, *for they are no more*. The term *passage*, for a memoir (on barometric minima) of seventeen closely printed pages, strikes me as a little extraordinary, but perhaps this arises from my imperfect acquaintance with the nice distinctions of your language. I leave it to my English readers to determine, with what degree of justice results deduced from a greater number of contemporary observations, than (as I believe) had ever previously or have even since been brought together, can be represented as *no more than ingenious speculations*.

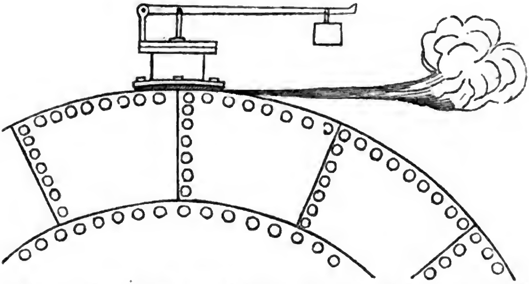
I am, dear Sir, yours with much esteem,

Berlin, Sept. 30, 1840. H. W. Dove.

LV. *On the Electricity of a Jet of Steam issuing from a Boiler. By* H. G. Armstrong, *Esq., in Letters to Professor* Faraday*.

Sir,

A FEW days ago, I was informed that a very extraordinary electrical phænomenon, connected with the efflux of steam from the safety-valve of a steam-engine boiler, had been observed at Seghill, about six miles from Newcastle. I therefore took an early opportunity of going over to that place, to investigate the truth of what I had heard, and by so doing I have ascertained the precise facts of the case, which appear to me to possess so much novelty and importance, that I deem it right to transmit the particulars to you, believing that in your hands they will prove most conducive to the advancement of science. Without further preface, I shall proceed to narrate what I saw and heard on the spot.



There is nothing remarkable in the construction of the boiler, which is supported upon brick-masonry in the usual way. The annexed sketch represents an end view of the boiler and safety-valve, by which it will be seen that the valve is placed on the top of a small cylinder, having a flange round the lower end, which is fastened by bolts to the summit of the boiler, between which and the flange, a cement, composed of chalk, oil and tow, is interposed for the purpose of making the joining steam-tight.

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* Communicated by Professor Faraday.

About three weeks ago the steam began to escape at this joining, through a fissure in the cement, and has ever since continued to issue from the aperture in a copious horizontal jet. Soon after this took place, the engine-man, having one of his hands accidentally immersed in the issuing steam, presented the other to the lever of the valve, with the view of adjusting the weight, when he was greatly surprised by the appearance of a brilliant spark, which passed between the lever and his hand, and was accompanied by a violent wrench in his arms, wholly unlike what he had ever experienced before. The same effect was repeated when he attempted to touch any part of the boiler, or any iron-work connected with it, provided his other hand was exposed to the steam. He next found that while he held one hand in the jet of steam, he communicated a shock to every person whom he touched with the other, whether such person were in contact with the boiler, or merely standing on the brick-work which supports it; but that a person touching the boiler, received a much stronger shock than one who merely stood on the bricks.

These singular effects were witnessed and experienced by a great many persons, and among others by two gentlemen with whom I am personally acquainted, and who fully corroborate the above account, which I obtained from the engine-man.

The boiler had been cleaned out the day before I saw it, and a thin incrustation of calcareous matter reaching as high as the water level had been removed, and the consequence was, that the indications of electricity, though still existing, were very much diminished. Still, however, what remained was very extraordinary; for when I placed one hand in the jet of steam and advanced the other within a small distance of the boiler, a distinct spark appeared, and was attended with a slight electrical shock.

From the effect produced by the cleaning of the boiler, it appears pretty obvious that the phænomenon is in a great measure, though not wholly, dependent upon the existence of an incrustation within; and the reason why such effects do not in any degree attend the effluxion of a jet of steam from a boiler in ordinary cases, must, I apprehend, be sought for in the fact, that in the present instance the steam escapes through an aperture in a non-conducting material, while in a vast majority of cases the escape must take place through a metallic orifice. Can the explosion of boilers, respecting the cause of which so much uncertainty at present exists, have any connexion with the rapid production of electricity which thus appears to accompany the generation of steam?

In the present case the incrustation in the boiler is very rapidly formed, and I therefore expect that in a few days the effects will have become as strong as they were at first. Whenever this takes place I shall again go over to witness them, and if you wish for any further information, I shall be glad to obtain it for you. In the mean time you are at liberty to make any use of this letter that you think fit.

I am, Sir, very respectfully yours,

Newcastle-upon-Tyne, Oct. 14, 1840. H. G. ARMSTRONG.

Newcastle-upon-Tyne, Oct. 22, 1840.

Dear Sir,—I yesterday revisited the boiler at Seghill, in company with some friends, and took with me such apparatus as I deemed necessary for experimenting on the electrical steam. The results of this second visit I now hasten to communicate, and you will find in the following account of my proceedings, answers to all the queries you were kind enough to send me, for the purpose of directing my attention to the proper points of inquiry.

I found the boiler, and everything connected with it, precisely in the state in which I have already described it, and on trying the steam in the same way as I did on the former occasion, the effect was very nearly the same; but when I placed myself on an insulating stool, the intensity of the sparks which passed between my hand and the boiler was greatly increased, as well as the twitching sensation in the knuckles and wrist, which accompanied the operation, and which in my former letter I designated a slight electrical shock. In pursuance of your instructions, I had provided myself with a brass plate, having a copper wire attached to it, which terminated in a round brass knob. When this plate was held in the steam by means of an insulated handle, and the brass knob brought within about a quarter of an inch from the boiler, the number of sparks which passed in a minute was from sixty to seventy, as nearly as we could count; and when the knob was advanced about one-sixteenth of an inch nearer to the boiler, the stream of electricity became quite continuous. The greatest distance between the knob and the boiler, at which a spark would pass from one to the other, was fully an inch. A Florence flask, coated with brass filings on both surfaces, was charged to such a degree with the sparks from the knob, as to cause a spontaneous discharge through the glass; and several robust men received a severe shock from a small Leyden jar charged by the same process. The strength of the sparks was quite as great when the knob was presented to any conductor communicating with the ground, as when it was held to the boiler. It appeared to make very little difference in what part of the jet the plate attached to the conducting wire was held; but when a thick iron wire was substituted for the plate, the effect was greatest when the wire was held very near to the orifice. The valve was loaded at the rate of thirty-five pounds per square inch; but the pressure of the steam fluctuated considerably, which gave me an opportunity of observing that the quantity of electricity derived from the jet increased and diminished with the pressure. The electricity of the steam was *positive*; for when the pith balls of the electrometer diverged upon an instrument connected with the steam, they were attracted by a piece of sealing-wax rubbed on woollen cloth; and when a pointed wire was held by the person on the stool, under the shade of a hat, *a pencil,* and not *a star,* of electrical light became visible.

Besides the principal jet of steam which I operated upon, there were several small streams issuing from different parts of the boiler, and in each of these the electrometer indicated the presence of electricity. From the peculiar manner in which the steam blew off from the safety-valve when the weight on the lever was lifted, it was quite impossible to try any satisfactory experiment upon the steam which was allowed to escape by that means. I applied the gold-leaf electrometer to various parts of the boiler, which, I ought to observe, is in direct communication with the ground by means of the steam-pipes, but could scarcely detect a trace of electricity in any part of it.

The engine has another boiler besides the one in question, and the two boilers lie immediately adjacent to each other. Having been informed that similar phænomena had been discovered in this second boiler, I proceeded to apply the electrometer to some small pencils of steam which were escaping in different parts, and found the same indications which I had observed under similar circumstances in the first boiler. I then raised the safety-valve, and the column of steam which escaped from it proved as highly charged with electricity as the horizontal jet which issued from the other boiler, and in which the phænomenon had first been observed.

Upon inquiry, I found that the water used in the boilers was obtained from a neighbouring colliery, where it was pumped out of the mine, and that the same water was used for the boiler of a small high-pressure engine adjoining the colliery from which the water was procured. In order, therefore, to form an opinion whether or not the phænomenon in question was dependent upon the quality of the water from which the steam was generated, I proceeded to examine the steam evolved from the boiler to which I had been referred, and which proved to be a very small one. The valve was loaded with only twenty pounds on the square inch, and I learned from the engine-man that no appearance of electricity had ever been noticed in the steam. Upon trial, however, I succeeded in obtaining very distinct sparks of electricity from the column of steam which issued from the safety-valve. The sparks were certainly weaker than those obtained at the other engine, but this may reasonably be ascribed to the inferior pressure of the steam, and smaller size of the boiler.

I then repaired to another high-pressure engine, which belonged to the same establishment, and the boiler of which was supplied with *rain* water instead of that drawn from the mine. In this case the pressure of the steam was forty pounds on the square inch. The valve was inaccessible, but a powerful jet of steam was obtained from the upper gauge-cock; I could not, however, obtain any trace of electricity in the steam from this boiler, not even sufficient sensibly to affect the gold-leaf electrometer. The presumption, then, is exceedingly strong, that the phænomenon is in some way occasioned by the peculiar nature of the water from which the steam is produced. I inclose you a specimen of the incrustation*, of a month's growth, deposited by the water from the mine in the boilers in which it is used.

I shall be glad to receive any further instructions from you as to the proper mode of pursuing the investigation, and should be much gratified to hear your opinion as to the cause of this most curious phænomenon †.

I am, dear sir,
Very respectfully yours,
H. G. Armstrong.

M. Faraday, Esq.

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* The incrustation is grey and hard; it contains traces of a soluble muriate and sulphate, but consists almost entirely of sulphate of lime, with a little oxide of iron and insoluble clayey matter, carried in probably by the water. There is hardly a trace of carbonate of lime in it.—M. F.

† The evolution of electricity by vaporization, described by Mr. Armstrong, is most likely the same as that already known to philosophers on

LVI. *Experiments on the Electricity of High-Pressure Steam. By* H. L. Pattinson, *Esq., F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A VERY singular phænomenon, viz. the production of electricity by two steam-boilers, has been observed in this neighbourhood within the last few weeks, the particulars of which I have the pleasure of transmitting to you for publication in your valuable Journal. The boilers in question are situated at Cramlington Colliery, eight miles north-east of Newcastle, where they supply steam to a high-pressure engine of 28-horse power, employed on the waggon-way to haul full and empty waggons to the top of two inclined planes, leading to the Colliery on the one hand, and to the river Tyne on the other. The boilers are cylindrical, with circular ends, each twenty-one feet long, and five feet diameter. They are supplied with water from an adjacent pond by iron feed-pipes, four inches diameter, and the steam they produce is conveyed to the working cylinder by other iron pipes, six inches diameter, which pipes form also a direct metallic communication between the tops of the boilers. By means of appropriate valves the steam is supplied to the cylinder from one or other boiler at pleasure. A pipe, two inches diameter, leads from the bottom of one boiler on the outside of the brick-work to the ash-pit, through which the sediment deposited by the water is occasionally blown from one of Scott's patent collecting cones, and a similar pipe is attached to the other boiler. The boilers are set in brick-work in the usual way, the fires below, with flues reaching all round, and passing into the chimney also in the usual manner. The flues are covered with large flat bricks, and in the space between the boilers the two flues are necessarily separated by a brick wall. The safety-valves are attached to the boilers by flange joints; and between the flanges, to render them steam-tight, is placed a ring of plaited hemp covered with a cement of litharge, sand and linseed oil, mixed up together, and when applied of the consistence of glaziers' putty. This cement, as it soon becomes hard, is used about the engine for steam joints which occasionally fail; but all the joints of the pipes are made of iron borings and

a much smaller scale, and about which there are as yet doubts whether it is to be referred to mere evaporation, as Harris says, or to chemical action, according to others. This point it neither settles nor illustrates; but it gives us the evolution of electricity during the conversion of water into vapour, upon an enormous scale, and therefore brings us much nearer to the electric phænomena of volcanos, water-spouts and thunder-storms, than before.—M. F.

sal-ammoniac, as ordinarily employed by engine-wrights. The steam is worked at a pressure of thirty-five pounds per inch.

The joint between the top of one of the boilers and the seat of its safety-valve had given way, and steam was issuing forcibly through this aperture, when on Tuesday, September 29th last, the engine-man, William Patterson, while standing with this current of steam blowing upon his legs, took hold of the weight attached to the lever of the safety-valve, to try the strength of the steam, when he felt a peculiar pricking sensation in the ends of his fingers, but as the steam prevented him from seeing distinctly, he thought he had merely struck his fingers rather suddenly against the weight. On Friday, October 2nd, on taking hold of the lever, he again felt a sensation in his fingers of the same kind as before; and on Saturday, the 3rd, on touching the weight, this sensation was stronger, and more distinct; so much so, as to arrest his attention and lead him to mention it to some other workmen employed about the engine, who all handled the weight, and convinced themselves that there was something about it very unusual. During the time they were thus employed, Patterson applied his finger gently to the lever, and perceived a spark. This was repeated by the whole party, and they soon found that sparks could be obtained from any part of the end of the boiler, as far as the valve upon the steam-pipe connecting the two boilers, and also from the pipe through which the sediment is blown, as already described. They observed further, that while standing in the volume of steam issuing from the joint, and touching the boiler, these sparks were always much stronger than when the boiler was touched by a person not in the current of steam. In one or two cases, according to their account, when the current of steam issuing from the joint was very strong, the person exposed to it being probably partially insulated by standing upon the dry and warm brick-work surrounding the boiler, gave strong sparks to others out of the current on bringing his hands to theirs; and once or twice they felt, under these circumstances, something like a slight electrical shock. It may be observed, that at this time the weather was exceedingly fine and dry. It was not long before the engineer of the colliery, Mr. Marshall, became acquainted with these circumstances, and his first feeling was to apprehend that the boiler was in danger of exploding, for, as he said, "when there was fire on the outside of the boiler, he did not know what there might be within." He accordingly sent to Messrs. Hawks's, of Gateshead, who built the boiler, for a person to examine it, and Mr. Golightly, their manager in that department, went out on Wednesday, the 7th inst., for that purpose. He gave his opinion as to the safety of the boiler, and returned much surprised at the phænomena it presented. The singular circumstance of a steam-boiler yielding electrical sparks, and giving shocks, now began to be noised abroad; and my friend, Mr. Henry Smith, of Newcastle, who had heard the account both from Mr. Golightly and Mr. Marshall, wrote me a note acquainting me with the matter, and desiring me to go with him to see it, which I did on the 11th inst., and again on the following day, having with us the second time proper electrical apparatus. On our first visit, the boilers being unplugged and empty, we merely satisfied ourselves as to all the particulars of their setting, etc., already detailed. Next day, on our arrival, we found the engine at work, the steam up to a pressure of thirty-five pounds an inch, and blowing off strongly at the joint in the boiler. The day was a little damp, but yet not unfavourable, and we were informed on alighting that the indications of electricity were very faint and weak; however, we proceeded to our examination, of which the following is the result.

1. On touching the boiler with the blunt point of a penknife anywhere about the circular end, the weight or the safety-valve itself, with the steam strongly blowing out of the joint, but with no part of the person exposed to the volume of steam, no spark could be perceived whatever.

2. On immersing one hand in the current of steam, and touching the parts of the boiler already named with the point of a penknife held in the other, a very minute but distinct spark was perceived, and this occurred equally on all parts of the boiler, or safety-valve, within reach.

3. By standing in the current of steam, so as to allow it to blow forcibly upon the person, the spark became larger; it was then one-eighth of an inch long.

4. On holding a large shovel in the current of steam with one hand, and touching the boiler with a penknife held in the other, a spark was obtained three-eighths of an inch long.

5. The cap of a gold-leaf electrometer, the bottom of which was held in the hand, was applied to the weight, the body of the operator being entirely out of the current of steam; and no divergence was produced whatever.

6. The electrometer held in the hand had its cap applied to the weight, the other hand of the operator being immersed in the current of steam: strong divergence was immediately produced.

From this it was evident that the electricity proceeded from the steam; but as the boiler-house was damp, so that insulation by glass could not well be preserved, a copper wire was attached to the shovel already mentioned, the end of which wire terminated in the engine-house, some yards distant from the boiler-house, where was placed a table. The shovel was held by Mr. Smith in the current of steam, with its edge about an inch and a half from the aperture through which the steam issued, and the wire leading away from the shovel was insulated by being attached to sticks of sealing-wax held by assistants. Mr. Smith stood on an insulating stool.

7. On touching a pith-ball electrometer, the threads of which were five inches long, with the insulated wire leading from the shovel held as mentioned, the balls diverged four inches with positive electricity.

8. The wire was attached to an insulated tin conductor, when it yielded sparks half an inch in length.

9. A pointed wire attached to this conductor exhibited the brush of light a quarter of an inch long, which always attends the escape of positive electricity from a point into the air.

10. A small jar was now charged so strongly as to give a rather disagreeable shock. By this time a large crowd of men, women and boys from the "Pit Raw," or pitmen's residences near the colliery, attracted by the novelty and singularity of the circumstances, had gathered about us, filling the engine-house and looking on with great curiosity and interest. A circle of sixteen of these men and women was formed, and they received together, much to their surprise and merriment, a powerful shock from the charged jar. This was several times repeated, the numbers receiving the shock varying each time from twelve to twenty.

11. A stout card was perforated by a discharge of the jar; and cotton wrapped round the end of a copper wire and dipped in pounded resin, readily set on fire.

12. When the edge of the shovel was made to approach the aperture through which the steam issued as near as three-quarters of an inch, very vivid and bright sparks of that length passed continually between it and the boiler.

13. The second boiler did not discharge steam through any fissure, but on lifting its valve by the hand it blew off in a strong current. When the shovel was held in one hand in this current of steam issuing from the safety-valve, and the boiler was touched with a penknife held in the other, a spark passed exactly, as under the same circumstances in the boiler subjected to the above experiments.

From this it would appear that the steam of both boilers was in the same electrical condition.

During the whole of these experiments the engine was doing its work as usual, occasionally going and occasionally standing; but no difference was observed in the electricity given off by the steam.

I have been most careful to supply an exact account of the facts of this extraordinary, and, as far as I know, unprecedented case, but I do not offer any theory to account for the phænomena. It is hardly possible to suppose that there is any local peculiarity about these boilers, or the place where they are situated, to occasion the highly electrical condition of the steam produced in them; and yet it is as difficult to suppose the fact of high-pressure-steam being electrical, a general one; for if it were so, it could hardly, up to this time, have escaped observation. The conditions, therefore, under which steam becomes electrical require to be investigated, and it is not unlikely that the investigation may lead to important results.

I am, Gentlemen,
Your obedient Servant,
H. L. PATTINSON.

Bentham-Grove, Gateshead,
October 19, 1840.

LVII. *Note on Elimination.* *By* J. J. SYLVESTER, *F.R.S., Professor of Natural Philosophy in University College, London**.

THE object of this brief note is to generalize Theorem 2. in my paper on Elimination which appeared in the last December Number of this Magazine. The Theorem so generalized presents a symmetry which before was wanting. Here, as in so many other instances, the whole occupies in the memory a *less* space than the part.

To avoid the ill-looking and slippery negative symbols, I warn my reader that I now use two rows of quantities written one over the other, to denote the product of the terms resulting from *taking away* each quantity in the under from each in the upper row.

Let $h_1 \quad h_2 \ldots\ldots h_n$ be the roots of one equation of co-existence.
$k_1 \quad k_2 \ldots\ldots k_m$ of the other.

And let the prime derivative of the degree r be required. Take *any* two integer numbers p and q, such that $p + q = r$. The derivative in question may be written,

$$\Sigma\left(\left(\frac{\overline{x-h_1}\,.\,\overline{x-h_2}\,..\,\overline{x-h_p}}{\overline{x-k_1}\,.\,\overline{x-k_2}\,..\,\overline{x-k_q}}\right)\frac{\begin{pmatrix}h_1\ h_2\,..\,h_p\\ k_1\ k_2\,..\,k_q\end{pmatrix}\times\begin{pmatrix}h_{p+1}\ h_{p+2}\ldots h_n\\ k_{q+1}\ k_{q+2}\ldots k_m\end{pmatrix}}{\begin{pmatrix}h_1\ h_2\,..\,h_p\\ h_{p+1}\ h_{p+2}\ldots h_n\end{pmatrix}\times\begin{pmatrix}k_1\ k_2\ldots k_q\\ k_{q+1}\ k_{q+2}\,..\,k_m\end{pmatrix}}\right)$$

N.B. Whatever p and q be taken, so long only as $p + q$

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* Communicated by the Author.

$= r$, the above expression changes nothing but its sign; which, therefore, upon transcendental grounds, it is easy to see is of one name or another, according as p is odd or even.

In the original paper, I asserted this theorem only for the case of $p = 0$ or $q = 0$.

University College, London, Oct. 29, 1840.

LVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 18, 1840. THE following letter was read from G. G. Anson, Esq., addressed to the President, enclosing a specimen of a deposit with which nine acres of land near Exeter, belonging to Lord Radnor, had been covered after the subsidence of a flood, and which was sent by H.R.H. Prince Albert, F.R.S.:—

"*Buckingham Palace, June* 8, 1840.

"MY DEAR LORD,

"His Royal Highness Prince Albert has commanded me to forward to you the enclosed specimen, which has been sent up to His Royal Highness from Lord Radnor's place near Exeter, where nine acres of land were covered with this curious substance after a flood had subsided. His Royal Highness thinks it very probable that the subject may already have been brought before the Royal Society, but in case it should not have been, he sends the accompanying packet. It is said that a good deal of it has been applied to the purpose of making waistcoats for poor people.

"Believe me,
"My dear Lord,
"Yours very faithfully,
"G. G. ANSON."

"*The Marquis of Northampton, President of the Royal Society.*"

The following description of the specimen referred to in the letter, drawn up by John Lindley, Ph. D. F.R.S., was also read:

"Description of the Specimen referred to in the preceding letter." By John Lindley, Ph. D., F.R.S.

The plant which overran Lord Radnor's land is the *Conferva crispa* of Dillwyn, which is said to be the *Conferva fluviatilis* of Linnæus. The species inhabits fresh water, and multiplies with great rapidity, forming entangled strata. The green portion is the Conferva in its young state, the white portion is the plant old and bleached. The whole mass consists of articulated filaments, among which are fragments of grass-leaves.

The following papers were then read, or their titles announced:—

1. An Account of Experiments on the Reflecting Telescope. By the Right Hon. Lord Oxmantown, F.R.S.

This paper enters minutely into the details of the experiments, of the precautions requisite to ensure success, and of the manipulations ultimately adopted in forming a speculum three feet in diameter, subsequently applied to a telescope, mounted in a manner very similar to that of Sir John Herschel. The author states, as the results he arrived at, that specula can be made to act effectively, when cast of the finest speculum metal, in separate portions, and retained in their positions by an alloy of zinc and copper, as easily wrought as common brass, and that they can be executed in this manner of any required size; that castings of the finest speculum metal can be executed of large dimensions, perfect, and not very liable to break; that machinery can be employed with the greatest advantage in grinding and polishing specula; that to obtain the finest polish, it is not necessary that the speculum should become warm, and that any temperature may be fixed upon, and preserved uniform during the whole process; and that large specula can be polished as accurately as small ones, and be supported so as to be secured from flexure.

2. On the theoretical explanation of an apparently new Polarity in Light. By G. B. Airy, Esq., M.A., F.R.S., Astronomer Royal.

The existence of a polarity in the rays of homogeneous light, having regard only to the sequence of colours in the spectrum, was inferred by Sir David Brewster from some experiments, of which he has given an account, contained in the Report of the Seventh Meeting of the British Association. The author states the results of his own observations of similar phænomena, and their theoretical explanation on the undulatory theory, together with the mathematical development of that explanation*.

3. On the Ferrosesquicyanuret of Potassium. By Alfred Smee, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author examines, in this paper, the action of chlorine upon the ferrocyanate of potassa, and the conversion of the latter into ferrosesquicyanuret; and proposes methods for obtaining this latter salt uncontaminated with impurities, and free from the difficulties and inconvenience attendant on the present mode of preparation †.

4. On the influence of Iodine in rendering several argentine compounds, spread on paper, sensitive to light; and on a new Method of producing, with greater distinctness, the Photogenic Image. By Mr. Robert Hunt. Communicated by Sir John Herschel, Bart., V.P.R.S.

This paper contains various details of the results of a great number of experiments made with a view of rendering paper capable of being employed instead of metallic plates, in Daguerre's photographic process. It is accompanied with 12 papers as specimens.

5. Hourly Observations of the Barometer and Thermometer at sea, on the 21st of March, 1840. By Major-General A. Lindsay, H.E.I.C.S. Communicated by Sir John F. W. Herschel, Bart., V.P.R.S.

These observations were made on board the ship Owen Glendower, on her voyage from Calcutta to London.

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* See our present volume, p. 81.—Edit.

† Mr. Smee's paper will be found in our present volume.

6. On the Constitution of Pigotite, and on the Mudesous and Mudesic Acids. By James F. W. Johnston, Esq., M.A., F.R.S.

In this paper the author describes a substance, found by himself and by the Rev. M. Pigot, forming an incrustation on the sides of certain caves, occurring in the granitic cliffs on the east and west coast of Cornwall. This incrustation is in mass of a brown, and in powder of a yellow colour; is insoluble in water and alcohol; when heated, it gives off much water, blackens, yields empyreumatic products, and leaves a black mass, having occasionally the lustre of graphite. In the air, at a bright red heat, this mass very slowly burns, leaving a grey or white ash, which consists of alumina, with some slight foreign admixtures.

The organic constituent of this substance (pigotite), the author considers to be derived from the decay of the various plants which grow on the moist moorlands above, and which, being carried by the waters into the fissures of the granite beneath, combines with the alumina of the decomposed felspar; and when it reaches the air, deposits itself on the roof and sides of the caverns, in the form of layers, varying from a line to two or three inches in thickness. With reference to its supposed origin, the author has given to the organic constituent the name of ***mudesous*** acid (from *μύδησις*, signifying decay through excess of moisture); and he mentions an observation, communicated to him by Dr. Boase, that the roots of the sea-pink (*Statice Armeria*) contain a colouring matter resembling, ***in appearance***, the solutions of the mudesous acid.

From numerous experiments and analyses detailed at length in his paper, the author derives the following general results:

1. That the native pigotite contains a dark-brown soluble, not deliquescent acid of vegetable origin, which, in the anhydrous state, is represented by $C_{12}\,H_5\,O_8$.

2. That this acid, the ***mudesous***, is tribasic, the salt of silver (mudesite), being represented by $(3\,A_9\,O + C_{12}\,H_5\,O_8)$, and precipitates the salts of the metallic oxides of a brown colour.

3. That the native mudesite of alumina (Pigotite) is represented as follows:

a. Dried in the air by $(4\,\underline{\dddot{Al}} + C_{12}\,H_5\,O_8 + 27\,H\,O)$.

b. Dried at 212° F. by $(4\,\underline{\dddot{Al}} + C_{12}\,H_5\,O_8 + 8\,H\,O)$, losing 27 per cent. of water.

c. Dried at 300° F. by $(4\,\underline{\dddot{Al}} + C_{12}\,H_5\,O_8 + 8\,H\,O)$, losing 32 per cent. of water.

4. That this native mudesite, however, is more probably a compound of the organic tribasic salt, with a hydrate of alumina, and may be rationally represented thus:

a. Dried in the air by $(\underline{\dddot{Al}} + C_{12}H_5O_8 + {}_9HO) + 3\,(\underline{\dddot{Al}} + 6HO)$.

b. Dried at 212° F. by $(\underline{\dddot{Al}} + C_{12}H_5O_8 + 4HO) + 3\,(\underline{\dddot{Al}} + 2HO)$.

c. Dried at 300° F. by $(\underline{\dddot{Al}} + C_{12}H_5O_8 + 2HO) + 3\,(\underline{\dddot{Al}} + 2HO)$.

5. That when treated with nitric acid, the native mudesite, as well as the mudesous acid itself, are oxidized and converted into a new brownish-yellow, soluble and deliquescent acid, containing more oxygen, and in the anhydrous state represented by $C_{12} H_5 O_{10}$.

6. That this new acid, the *mudesic*, combines readily with alumina and protoxide of mercury, giving salts of a yellow colour. Both the acids described in this paper are distinguished for their tendency to precipitate alumina and the protoxide of mercury. The mudesate of mercury dried at 300° F., is represented by $(2 H_g O + C_{12} H_5 O_{10})$.

7. That chlorine, when made to act on either of the acids, or their salts of alumina in contact with water, gradually deprives them of all colour, while, at the same time, muriatic acid is formed. Collected on the filter, boiled in water till the washings cease to precipitate nitrate of silver, and dried, the white gelatinous, apparently altered mudesite or mudesate, is found on analysis to contain no atomic proportion of chlorine, but to have sensibly the constitution of the mudesic acid, or mudesates prepared by the direct action of nitric acid. The author thinks it not unlikely that a chloro-mudesic acid exists, and may be formed during this process, represented probably by $C_{12} H_4 Cl O_{10}$, but which he has not succeeded in obtaining in a separate state.

The mudesous and mudesic acids are distinguished from each other by giving, the former brown, and the latter yellow precipitates with the neutral metallic salts—by being, the former unaltered, and the latter deliquescent in the air. Both form deliquescent salts with ammonia, and appear to undergo alteration by the long-continued action of hydrosulphuric, or of concentrated sulphuric and hydrofluoric acids.

7. On the Constitution of the Resins, Part V. By James F. W. Johnston, Esq., M.A., F.R.S.*

In this paper the author continues his examination of what are called the fetid resins, and from repeated analyses deduces for the resin of Sagapenum the formula $C_{40} H_{29} O_9$, and for that of Galbanum $C_{40} H_{27} O_7$. He then compares the formulæ for the four resins:

Opoponax $= C_{40} H_{25} O_{14}$, Assafœtida $= C_{40} H_{26} O_{10}$,
Galbanum $= C_{40} H_{27} O_{70}$, Sagapenum $= C_{40} H_{29} O_9$;

and considers it probable that, though no striking analogy among the *irrational* formulæ for these resins is perceptible, by which their analogy in physical properties can be accounted for, they may possess an analogous *rational* constitution which future researches may disclose.

Euphorbium consists of two resins, of which the more soluble, A, gave the formula $C_{40} H_{31} O_6$. Elemi also consists of two resins, of which the more soluble, A, is represented by $C_{40} H_{32} O_4$, and the less soluble, B, by $C_{40} H_{32} O_1$, as had previously been shown by Hess and Rose. The Bdellium of commerce contains much gum, and a resin $C_{40}H_{31}O_5$.

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* Abstracts of the preceding series will be found in Lond. and Edinb. Phil. Mag., vol. xv. p. 327, and present vol. p. 147.—Edit.

The resin of Benzoin presented peculiar difficulties when submitted to investigation, from the ease with which it undergoes decomposition, even at temperatures much below that at which it melts. With regard to this resin, the author gives the following as the result of his numerous analyses:—

1. That the colourless resin of benzoin is represented approximately by $C_{40}H_{22}O_9$.

2. That by heat and dilute carbonated alkalies it is decomposed into water, benzoic acid, a little volatile oil, and a resin $C_{40}H_{23}O_9$, or $C_{40}H_{24}O_9$.

3. That by boiling with quicklime, or concentrated carbonated alkalies, it gives two resins, one in large quantity $= C_{40}H_{24}O_8$; and another in small quantity $= C_{40}H_{30}O_7$.

4. That by caustic potash the crude resin is resolved into two resins represented respectively by $C_{40}H_{22}O_9$, and $C_{40}H_{30}O_7$, of which the former is precipitated, and the latter remains in solution, when a saturated aqueous solution of caustic potash is added to an alcoholic solution of the crude resin.

5. And that by oxide of lead two resins are separated, for which analysis gave respectively the formulæ $C_{40}H_{22}O_9$, and $C_{40}H_{26}O_{10}$.

The author concludes by stating that such metamorphoses are by no means confined to this resin, though the more accurate knowledge of their nature, obtained by the imperfect study he has made of the resin of benzoin, has explained many anomalies he had previously observed, with regard to the relations of the resins to the alkalies and metallic oxides. He considers the group of which dragon's blood is the type, and which he represents by the expression $C_{40}H_{24} \pm x\,O_9$ to be peculiarly susceptible of modification (or decomposition?) by the action of bases; and he specifies among other results, with regard to which it is his intention to address the Society in a future paper, that dragon's blood, of which the lump variety $= C_{40}H_{21}O_8$, and the drop variety (heated to 300° F.) $= C_{40}H_{20}O_8$, gives by the action of quicklime and oxide of lead, among other products, two resins represented approximately by $C_{40}H_{20}O_{10}$ and $C_{40}H_{20}O_{12}$?—that guiacum $= C_{40}H_{23}O_{10}$, with oxide of lead, gives a resin $= C_{40}H_{21}O_{11}$, the resin of jalap $= C_{40}H_{34}O_{18}$; by the action of the same oxide, a resin $= C_{40}H_{34}O_{20}$, and that of assafœtida $= C_{40}H_{26}O_{10}$, a new resin $= C_{40}H_{23}O_{15}$. These metamorphoses lead to the second great branch of inquiry respecting the nature and constitution of the resins. Certain results being established, at least approximately, with regard to the *irrational* constitution of the resins, and certain general irrational formulæ by which to express it, we are prepared for the study of their *rational* constitution. This part of the subject the author proposes to consider farther in subsequent communications.

8. Researches on the Tides. Twelfth Series. On the Laws of the Rise and Fall of the Sea's surface during each tide. By the

Rev. W. Whewell, B.D., F.R.S., Fellow of Trinity College, Cambridge*.

The materials of the present investigation are five months' tide observations made at Plymouth; three months' observations made at Liverpool, under the direction of Captain Denham, R.N.; and twelve months' observations made at Bristol, by Mr. Bunt, by means of his tide-gauge. According to the theory of the tides, the height of the surface of the water at a given place will increase as the sine, while the time increases as the arc. Hence if the time be made the abscissa, and the height the ordinate, the curve representing one tide would be the *figure of signs*. The author on making the comparison of the empirical curve of the rise and fall of the water, deduced from observation, with this theoretical curve, finds a general agreement between them; subject to certain deviations, consisting principally in the empirical curve indicating that both the rise and the fall are not symmetrical, like the theoretical curve, in consequence of the fall being generally more rapid than the rise, and thus occasioning a displacement of the summit of the curve towards that branch of it which corresponds to the fall.

9. Researches in Embryology. Third Series.—Additional Observations. By Martin Barry, M.D., F.R.S.†

Having in the paper to which the present is supplementary made known the fact that the germinal spot in the mammiferous ovum resolves itself into cells, with which the germinal vesicle becomes filled, the author has since directed his attention to the corresponding parts in the ova of birds, batrachian reptiles, and osseous fishes, which he finds to be the seat of precisely the same changes. The numerous spots in the germinal vesicle of batrachian reptiles and osseous fishes are no other than the nuclei of cells. The cells themselves, from their transparency, are at first not easily discerned, and appear to have hitherto escaped notice; but after the observer has become aware of their presence, they are, in many instances, seen to be arranged in the same manner, and to present the same interior themselves as the corresponding cells in the ovum of mammalia.

In the representations given by Professor Rudolph Wagner, the discoverer of the germinal spot, the author recognizes evidence of the same changes in ova throughout the animal kingdom. He confirms and explains the observations of R. Wagner, that in the ova of certain animals an originally single spot divides into many, and that in the ova of other animals the number of spots increases as the ovum ripens. But he expresses also the opinion that in all ova there is originally but a single spot, this being the nucleus of the germinal vesicle or cell.

The analogy between the ova of mammalia and the animal abovementioned, extends also to the substance surrounding the germinal vesicle, which consists of nucleated cells.

10. Description of a Calculating Machine invented by Mr. Thomas

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* See Lond. and Edinb. Phil. Mag., vol. xv. p. 316.—Edit.

† *Ibid.*, vol. xvi. p. 526.—Edit.

Fowler, of Torrington in Devonshire. By Augustus De Morgan, Esq. Communicated by F. Baily, Esq., V.P.R.S.

The arithmetical operations performed by the machine are those of multiplication and division; the factors and product in the former case, and the quotient, dividend and divisor in the latter, being expressed in digits of the ternary scale of notation, every digit being either $-1, 0$, or $+1$. In this system, unity being, in multiplication, only an index, the rules for multiplication and division must consist entirely in directions for the management of the signs of unity; and it is on this principle that Mr. Fowler's machine is made to act. A short account is given of the principal parts of the machine, and of the mode in which they bring out the final results. It is necessary, however, in applying it to use, to have recourse to tables, both for converting the factors and reconverting the result; operations which introduce both labour and risk of error.

11. On the Minute Structure and Movements of Voluntary Muscles, in a letter addressed to R. B. Todd, M.D., F.R.S., &c. By William Bowman, Esq., Demonstrator of Anatomy in King's College, London, and Assistant Surgeon to King's College Hospital. Communicated by Dr. Todd.

The objects of the author, in this paper, are the following.—1st. To confirm, under some modifications, the view taken of the primitive fasciculi of voluntary muscles being composed of a solid bundle of fibrillæ: 2dly. To describe new parts entering into their composition: and 3dly. To detail new observations on the mechanism of voluntary motion.

He first shows that the primitive fasciculi are not cylindrical, but polygonal threads; their sides being more or less flattened where they are in contact with one another; he next records, in a tabular form, the results of his examination of their size in the different divisions of the animal kingdom. It appears that the largest are met with in fish; they are smaller in reptiles, and their size continues to diminish in insects, in mammalia, and lastly, in birds, where they are the smallest of all. In all these instances, however, an extensive range of size is observable, not only in different species, but in the same animal, and even in the same muscle. He then shows that all the fibrillæ into which a primitive fasciculus may be split, are marked by alternate dark and light points, and that fibrillæ of this description exist throughout the whole thickness of the fasciculus; that the apposition of the segments of contiguous fibrillæ, so marked, must form transverse striæ, and that such transverse striæ do in fact exist throughout the whole interior of the fasciculus. He next inquires into the form of the segments composing the fibrillæ, and shows that their longitudinal adhesion constitutes *fibrillæ*, and their lateral adhesion *discs*, or plates, transverse to the length of the fasciculus; each disc being, therefore, composed of a single segment from every one of the fibrillæ. He shows that these discs always exist quite as unequivocally as the fibrillæ, and gives several examples and figures of a natural cleavage of the fasciculus into such discs. It follows that the transverse striæ are the edges, or focal sections of these discs. Several varieties in the striæ are then detailed, and the fact noticed that in all animals there is frequently more or less diversity in the number of striæ in a given space, not only on contiguous fasciculi, but also on the same fasciculus at different parts.

The author then proceeds to describe a tubular membranaceous sheath, of the most exquisite delicacy and transparency, investing each fasciculus from end to end, and isolating it from all other parts; this sheath he terms *Sarcolemma*. Its existence and properties are shown by several modes of demonstration; and among others, by a specimen in which it is seen filled with parasitic worms (Trichinæ), which have removed all the fibrillæ. The adhesion of this sarcolemma to the outermost fibrillæ is explained.

It is also shown that there exist in all voluntary muscles a number of minute *corpuscles* of definite form, which appear to be identical with, or at least analogous to the nuclei of the cells from which the development of the fasciculi has originally proceeded. These are shown to be analogous to similar bodies in the muscles of organic life, and in other organic structures.

The author next describes his observations on the mode of union between tendon and muscle; that is, on the extremities of the primitive fasciculi. He shows that in fish and insects the tendinous fibrillæ become sometimes directly continuous with the extremities of the fasciculi, which are not taper, but have a perfect terminal disc. In other cases the extremities are shown to be obliquely truncated, where the fasciculi are attached to surfaces not at right angles to their direction.

Lastly. He states his opinion, and gives new facts on which it is founded, that in muscular contraction the discs of the fasciculi become approximated, flattened, and expanded; the fasciculi, of course, at the same time becoming shorter and thicker. He considers that in all contractions these phænomena occur; and he adduces arguments to show the improbability of the existence of any rugæ or zigzags as a condition of contracting fasciculi in the living body. The paper is abundantly illustrated by drawings of microscopic appearances.

The Society then adjourned over the long vacation, to meet again on the 19th of November.

GEOLOGICAL SOCIETY.

[Continued from p. 309.]

MINING RECORDS OFFICE.

A third department, which it is proposed to add to this establishment, is an office, for the preservation of such records and documents relating to subterranean operations throughout the country as are important to be preserved for the information of future generations.

To the keeper of these records will be assigned the duty of arranging the documents which may be transmitted to him from all parts of the kingdom, by any engineers, mineral surveyors, and proprietors of mines and coal works, who may be willing to send them; particularly maps, sections, and under-ground plans, which will record the state of each mine, when it is abandoned, for the information of those who at a future period may be disposed to bring it again into operation. This office will be accessible to all persons interested in obtaining the information it will afford. To this collection several engineers of most extensive experience in the mines of Newcastle and Cornwall have promised large contributions.

The keeper will make copies of documents of this kind, which proprietors of mines, who cannot conveniently part with the originals, may lend, for the purpose of being preserved in this national collection.

The public importance of such a records office was submitted to the Lords of Her Majesty's Treasury by a Committee of the British Association for the Advancement of Science, assembled at Newcastle in August, 1838; it being notorious that great losses of life and destruction of property have resulted both at Newcastle and in other coal mines throughout the kingdom, from the imperfect preservation of records of the operations previously conducted in them, and that still greater losses will inevitably ensue hereafter, unless advantage be taken of the experience of living engineers and coal proprietors, who are willing to place in a public national repository copies of the documents they possess relating to their respective mines.

In 1834, the attention of the public was called to this subject by Mr. T. Sopwith*, an eminent civil engineer and mine surveyor at Newcastle; and this gentleman is preparing a practical book of instructions on the subject of drawing geological and mining plans, the conducting of subterranean surveys, and examining mineral districts, with a view to the preservation of such information respecting the state of each mine at the period when it may be abandoned, as may be useful when further proceedings are afterwards commenced therein, or in its vicinity.

A museum of œconomic geology, comprehending institutions of this kind, demonstrates, even to the unlearned, the advantages that result from science in its application to the extraction of the treasures which Providence has laid up in the rich storehouses of the interior of the earth; and by exhibiting the results obtained from the elaboration of these materials, by the industry of man, in the workshop and at the forge, will afford a full and satisfactory reply to the question so often raised by persons to whom the value of the truths of pure science and philosophy, pursued for their own sake, are unintelligible,—and by whom everything is appreciated merely according to its immediate subserviency to the acquisition of wealth, or its ministration to the daily necessities or conveniences of human life.

BUILDING-STONE COMMISSION.

Another event which marks increasing attention to the practical importance of geology, is the publication of a Report to the Commissioners of Her Majesty's Woods and Forests, from a Commission appointed by the Lords of the Treasury; containing the results of an inquiry into the qualities and durability of the various Building-stones of this country, with a view to the selection of the best material to be employed in erecting the New Houses of Parliament.

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* See Sopwith on Isometric Drawing, p. 50, *et seq.*

The results of this inquiry have been arranged in Tables, which represent the composition, colour, weight, size, cost, durability, &c., of all the most important kinds of stone that have been used in ancient edifices in England; the Commissioners having judiciously appealed to that which is the most severe test of the durability of any stone, viz. the existing condition of the decorated architecture in our most ancient buildings.

The Norman portions of the Church of Southwell, in Nottinghamshire, constructed of magnesian limestone, in the twelfth century, have been found to afford an example of stone which combines strength and durability with applicability to ornamental carved work, in a degree surpassing all other kinds of stone that have been employed in the most ancient fabrics of this country; the sharpest of the mouldings and carved enrichments of that church being throughout in as perfect a state as when first executed. The keep of Koningsburgh Castle, near Doncaster, built also of the magnesian limestone in that vicinity, offers another proof of the durability of certain beds of this formation, exceeding that of any other building-stone in Great Britain, which is equally fit for ornamental purposes. But there are also varieties of magnesian limestone, such as that of which York Cathedral is built, which are in far advanced stages of decay, where they have been used for mouldings and architectural decorations.

The general result of this elaborate inquiry into the durability of the different varieties of magnesian limestone is, that the stone resists decomposition in proportion as it is more perfectly crystalline; a result, the cause of which is further illustrated by the experiments of Professor Daniell, which show that the nearer the magnesian compounds approach to *equivalent* proportions of carbonate of lime and carbonate of magnesia, the more crystalline they are.

No investigation has been made by these Commissioners as to the capabilities of granite, porphyries, and other kinds of stone, which are inapplicable to the decoration of edifices without enormous expense.

The Report is followed by valuable tabular lists of the most remarkable ancient fabrics in England, specifying the materials of which they are constructed, and their various conditions of preservation or decay, as they are respectively built of sandstone, or of oolitic, shelly, or magnesian limestone.

To these are added tables of the chemical analysis, weight, cohesive power, specific gravity, and power of absorbing water, of many of the building stones most largely employed in England*.

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* [It may be added, as evincing the strong interest which this Report has excited, that its contents have been specially illustrated, by one of the Com-

I consider this Report as of the highest value, in showing the general advantages which may be derived from connecting scientific knowledge with practical arts; and I trust we shall hear no more of such discreditable and unfounded assertions as, not long ago, passed uncontradicted, at a meeting of an architectural society in London, that Stonehenge is made of statuary marble.

GEOLOGICAL COMMITTEE OF ENGLISH AGRICULTURAL SOCIETY.

The appointment of a Geological Committee, by the English Agricultural Society, at their meeting in Oxford, in July last, shows the sense entertained by that numerous body of landed proprietors, and cultivators of the soil of England, of the important services which may be rendered to them, by the application of geological knowledge to the improvement of the productive capabilities of the land.

It is well known to geologists that an almost unbounded supply of mineral manure may be found in the sub-strata, which in very many districts are composed of ingredients different from those of the surface. So constant are the characters of many of the beds of the geological groups which pass in long and narrow bands from one side of England to the other, that a single experiment, carefully conducted, on any one stratum of each formation, with a view to ameliorate its soil, by an admixture of the ingredients of some other adjacent stratum, will afford an example which may be followed with similar results in distant parts of the kingdom, through which this same stratum passes, in its course across the island.

Experiments, therefore, conducted by the owners and occupiers of land, under the advice of this Geological Committee, aided by the facilities for the analysis of soils now afforded by the laboratory of the Museum of Œconomic Geology, may shortly enable us to realize at least some share of the success that attended Lavoisier's application of chemistry to agriculture in France*.

SCHOOLS OF CIVIL AND MINING ENGINEERING IN THE UNIVERSITIES OF DURHAM AND LONDON.

The increasing demand for education in practical science has been recently provided for in the University of Durham, by the establishment of a course of instruction in Civil and Mining Engineering, with lectures in the Mathematical sciences, Chemistry, Metallurgy, Mineralogy, Geology, Surveying, Mapping, and Drawing, in addition to Ancient and Modern Languages. To theoretical instruction in

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missioners, Mr. Charles Smith, in lectures delivered before the Society for the Encouragement of Arts, &c. and the Institute of British Architects; and by Mr. Brayley, in a Course on the Mineralogy of the Arts, delivered before the Architectural Society, at one of the Friday-Evening Meetings of the members of the Royal Institution, and in two lectures expressly on the results arrived at by the Commissioners, given at the Russell Institution.—Edit.]

[*] It was said of Lavoisier, that in ten years he doubled the produce of his land in grain, while he quintupled the number of his flocks. No doubt this report is much exaggerated.

such parts of these branches of knowledge as bear more especially on Practical Engineering, are added at Durham occasional surveying excursions, both in the field and underground, conducted by a practical civil engineer. More than thirty young men have, during the last year, been actively engaged in this new department of academical study.*

The locality of Durham, upon the margin of the great Newcastle coal field, and in the vicinity of the lead mines of Alston Moor, and Weardale, is in a peculiar degree favourable for a school of mining and civil engineering; enjoying advantages of position similar to those of the great Saxon school at Freyberg, near the mining districts of the Ertzgebirge and the Hartz.

The University of London also is taking measures to institute examinations of Candidates for certificates of proficiency in Civil Engineering, and the arts and sciences connected with Mining.

In University College, London, courses of preparatory experimental lectures and exercises in Natural Philosophy have, during the last year, been provided for the students in that establishment, who are destined for the Profession of Civil Engineers.

And in King's College, London, a course of lectures in Civil Engineering, and Sciences applied to Arts and Manufactures, is at this time attended by more than fifty students, who have the opportunity of adding practical to theoretical knowledge in a workshop and laboratory established for their use.

SCHOOL OF MINES IN CORNWALL.

Another proof of the direction of public attention to the collateral branches of our science has, within the last twelve months, been afforded by the establishment in Cornwall, of a school for the instruction in Sciences and Arts connected with MINING, of young men who are to be engaged in conducting the important subterranean operations of that county. The want of such a school had been pointed out by Mr. John Taylor, in his Prospectus of a School of Mines in Cornwall, February 7, 1825,† and in his Records of Mining, published in 1829. It has at length been instituted chiefly through the exertions and at the expense of Sir Charles Lemon. This incipient school, and the University of Durham, form almost solitary examples in England, of such scientific establishments as are nearly universal in the mining districts of the Continent. The experiment has begun in Cornwall with Courses of Lectures in Mathematics, Mechanics, Chemistry, and Mineralogy, by three professors; and a course of instruction, by a practical surveyor, in Algebra, Drawing, and the Use of instruments: and during the next year, still further additions are contemplated.

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* See Durham University Calendar, 1839, p. 10. [See also a communication on this subject by the Rev. Prof. Chevallier and Prof. Johnston, in Lond. and Edinb. Phil. Mag., vol. xiii. p. 1.—EDIT.]

[† The Prospectus here alluded to will be found in Phil. Mag., First Series, vol. lxvi. p. 137.—EDIT.]

POLYTECHNIC SOCIETY OF CORNWALL.

To the zealous exertions of Sir Charles Lemon, and of many intelligent and active individuals at Falmouth, the county of Cornwall is also indebted for the establishment of a Polytechnic Society, which, during the few years of its existence, has been attended with extraordinary success. One of its chief objects is to encourage, by rewards, the invention and improvement of machinery, of which so large an amount is essential to the working of the mines. Another object is to collect materials for expressing the quantity and value of the mineral and other produce of the county; and to construct tables indicating the diminished longevity, and diseases, which, in a peculiar degree, affect the Cornish miners, and do not prevail amongst those employed in Collieries. It appears, from a paper published in the Sixth Annual Report of this Society (1839), that the average duration of a miner's life is less, by many years, than that of the agricultural labourer in the same district; the apparent causes of this frightful evil being the inevitably imperfect ventilation of many of the veins or lodes in which the miner works; and, partly, the extreme fatigue of ascending from great depths by ladders, instead of being lifted by machinery, as the workmen are from coal pits: these pits also are usually susceptible of more perfect ventilation, than the metalliferous lodes in Cornwall.

The attention of this Society is strenuously directed to the discovery of remedies for these tremendous evils, which affect no fewer than a population of 28,000 persons; that being the proportion of the inhabitants of Cornwall, who are occupied in working the mines.

LOCAL MUSEUMS.

Another circumstance which marks the progressive advancement of public feeling as to the value of geology, is the increasing disposition to form local museums in our provincial towns.

At the meeting of the British Association, at Birmingham, in August last, after a strong expression of opinion, in the Section of Geology, as to the benefit likely to accrue to science from the establishment of Provincial Museums, for the local productions of each neighbourhood, the justness of the suggestion was so fully recognised, that, in the adjacent town of Dudley, before five days had passed, a public museum had arisen from contributions, out of the cabinets of private collectors in that town; presenting to the Association a more perfect assemblage than was ever seen, of the exquisite organic remains found in the limestone of that district, which has long been the classic type of a formation widely and abundantly distributed over the globe.

About this time also a provincial museum was formed at BRADFORD, in a district abounding in splendid examples of the vegetable remains which pervade the Yorkshire coal field; where the extensive collieries now wrought will furnish abundant materials for a collection, destined to illustrate the history of the extinct forms of vegetable life, which have produced the coal.

The museum at LEEDS, also, possesses a valuable collection of fossil vegetables from the coal field in its neighbourhood; and the WEST RIDING GEOLOGICAL SOCIETY, formed under the auspices of Earl Fitzwilliam, on the plan of holding quarterly meetings at different towns of the Riding in succession, is diffusing a taste for Geology, and affording ground for appreciating its practical importance, to numbers of intelligent persons, whose local occupations, and property in the coal and iron mines, will enable them to enlarge the fossil Flora and Fauna of our country.

ROYAL INSTITUTION OF SOUTH WALES.

From the first Annual Report of the Royal Institution of South Wales, published during the last year, we learn that the Swansea Literary and Philosophical Institution, hitherto supported by the town and neighbourhood, has been expanded, under Royal patronage, to the whole southern division of the Principality; and is now establishing its Museum and Lecture Rooms in a large and commodious edifice in the town of Swansea, under the presidentship of Lewis Weston Dillwyn, Esq.

The position of this Institution, in the midst of a great mining and manufacturing district, is peculiarly favourable for collecting facts illustrative of geological phænomena, more especially those of the Coal formation; and much has already been done by Mr. Logan, to develope, with extreme accuracy and minuteness of detail, the stratigraphical succession of the rocks composing this formation; and to show the number and nature of the events which attended their original deposition, as well as the subsequent derangements that have affected them. Mr. L. W. Dillwyn, also, is attempting a classification of the coal plants of the South Wales Bason; with a view to ascertain, by means of a comparative collection in the Swansea Museum, whether there exists any specific difference between those of the upper and lower beds of the carboniferous series.

BRITISH MUSEUM.

The accessions lately made to the British Museum form another subject, of high importance in our Review of the Geological Proceedings for the past year. At the head of these is the purchase, from Mr. T. Hawkins, of an additional series of the remains of fossil *Saurians* from the Lias formation; which, added to his former collection, already placed in this national repository, present an unrivalled series of species in the extinct families of *Ichthyosaurus* and *Plesiosaurus*, once inhabitants of Britain. Equally important was the acquisition, in a former year, of the unique collection of still more gigantic and not less monstrous Reptiles, from the Wealden formation of Kent and Sussex, obtained by purchase from Dr. Mantell. The possession of these several collections places the Museum, where it ought to stand, at the head of all existing repositories of organic remains, almost exclusively the productions of England; and it is due to his late exertions, whilst Chancellor of the Exchequer, that I should bear this public testimony to the services which Lord Monteagle has rendered to science, by supplying the means of placing these unrivalled collections in our national repository; where their constant presentation to the view of its thousands of daily visitors cannot fail to attract increasing attention to the wonderful discoveries of Palæontology.

These important public events, occurring beyond our walls, and having a direct and immediate tendency to enlarge the field of our labours, form an epoch in the history of our science, and place Geology before the country in a new and more widely popular aspect than it had occupied before. The past year has been also distinguished beyond all precedent, by the number and value of the GEOLOGICAL MAPS it has produced.

GEOLOGICAL MAP OF CORNWALL AND DEVON.

The first map which I shall mention, affords another example of the recognition by Government of the importance of our subject, by their having attached a geological department to the Ordnance Survey of England and Wales. The first fruits of this appointment are the splendid Maps of Devon and Cornwall, and a part of Somerset, coloured after the surveys of Mr. De la Beche; and it may be truly said of them, that they are more beautiful in their execution, more accurate in their details, and more instructive in the œconomical and scientific information they give respecting mines, than any maps yet published by any government in the world; affording documents to which we can at length with pride appeal, in reply to the reproach that has so long, with too much truth, been cast upon us, that England alone, of all the civilized nations, has abandoned to gratuitous individual exertions, and the liberality of amateurs in science, the great work of exploring and delineating the mineral structure of the country; and ascertaining the nature and extent of the subterraneous produce, which lies at the foundation of the industry of its manufacturing population, and to which the nation owes no small portion of its wealth.

The statistical importance of this first portion of the Ordnance Geological Map of England will be duly appreciated only by those, who know the extent of the property embarked in the mining interests of the Western counties, and are aware that the annual value of the mineral produce of Cornwall and Devon alone has recently amounted to 1,340,000*l.*

In the chapter on Œconomic Geology, which forms part of the Memoir connected with his Map of Cornwall and Devon, Mr. De la Beche has placed, in a more prominent light than has ever yet appeared, the bearing of geological researches and mineral statistics upon political œconomy; and proves, by tabular documents, the important fact, that the average value of the annual produce of the mines of the British Islands amounts to the enormous sum of 20,000,000*l.**, of which about 8,000,000*l.* arise from iron, and 9,000,000*l* from coal.

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* See Geological Report on Devon and Cornwall, p. 624, and note, 1839. In this estimate the value of the copper is taken in the ore, before fusion;

Should this inquiry be extended through the endless departments of art, industry and commerce, which have their origin in the manufactories of metals, and in the power of steam, derived exclusively from the application of coal, the vast national importance of mineral statistics, and of models, maps and sections, on which alone their details can be effectually recorded, must be apparent to every one.

Still more extensive will be the statistical and political importance of the next portion of this great work, now in progress by the same highly accomplished geologist, which is to comprehend the coal and iron districts of Monmouthshire and South Wales.

GEOLOGICAL MAP OF ENGLAND.

You have this day the satisfaction to see suspended in your meeting-room a new edition of Mr. Greenough's Geological Map of England, which has for many years formed the glory of this Society. It is truly gratifying to observe how small a change this new edition exhibits, either in the general dispositions, which it represented nearly a quarter of a century ago, or in the complicated details of the boundaries of the different formations. Some alterations appear in the Greensand series, the Wealden, the Lias, and the New red Sandstone. The principal additions are the introduction of the Silurian divisions made in the slate rocks, by Mr. Murchison, in the border districts of England and Wales; and the new distribution very recently assigned to the slate rocks of Devonshire and Cornwall.

A great improvement also has been made by the substitution of an entirely new Map of Wales and Siluria, founded on the Ordnance surveys of those regions, of which no accurate physical map existed at the time of Mr. Greenough's first publication. Another improvement in the execution consists in the union of linear shadows with the colours representing the superficial extent of the strata. The combined effects of these *elements of expression*, judiciously employed, has been to exhibit, more distinctly, the subdivisions of formations, without destroying the unity of the general mass to which they belong. By the frequent introduction also of conventional signs, and figures of reference, Mr. Greenough has produced a more condensed assemblage of scientific information, of varied kinds, than has been put together in any map of equal extent yet published. Extreme attention has also been paid to the physical features of the country, and in the orographic details more than 500 heights are given. The hydrographic features also are delineated with scrupulous exactness.

GEOLOGICAL MAP OF IRELAND.

The last summer has witnessed the production of Mr. Griffith's large and splendid Geological Map of Ireland, containing the results

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that of the iron, lead, zinc, tin and silver, after fusion, in their first marketable condition — as pigs, blocks and ingots. The coal is valued at the pit's mouth.

of nearly thirty years' investigation, by that eminent geologist and civil engineer.

Mr. Griffith had supplied an outline of this map published in the Report of the Railway Commissioners for Ireland, 1838. It is obvious that the information thus conveyed, as to the nature of the materials of which the island is composed, affords the most solid basis for sound calculation as to the future improvement of Ireland by the application of its natural resources.

GEOLOGICAL MAP OF A LARGE PORTION OF EUROPE.

During the last year we have also witnessed the publication of a beautifully coloured general Geological Map of Germany, France, and England, and parts of the adjoining countries, compiled from the larger original maps of Von Buch, Elie de Beaumont, and Greenough, by Professor Von Dechen, in one large sheet, published at Berlin.* This map exhibits the geological details of a larger continuous portion of the surface of the earth than has ever before been put together with so much exactness, and set forth on such eminent authority. It also presents to the statesman and political œconomist the most important portions of central Europe, under the new aspect of the natural divisions of the mineral formations, of which each country is composed; showing that in every region the nature and disposition of the substrata lie at the foundation, not only of its agricultural productiveness, but also of its capability of supplying the materials, which form the basis of its industry and arts. As an historical document, this map demonstrates the rapid progress of our science, and the state of maturity which it has attained.

Thus far I have occupied your attention with external matters of extraordinary interest in the history of our science, which show that geological knowledge is spreading its salutary influence, more widely and rapidly than heretofore, over the practical business of the country. I now proceed to consider the communications made to the meetings of our Society during the past year.

[To be continued.]

LIX. *Intelligence and Miscellaneous Articles.*

NOTE REFERRED TO IN THE ABSTRACT OF PROFESSOR DANIELL'S PAPER, p. 354.

THE preceding considerations will furnish a satisfactory clue to the apparently anomalous origin of the currents in Becquerel's circuits; when, for example, nitric acid is placed on one side of a diaphragm, and solution of potassa on the other, platinum electrodes being placed in either cell and the circuit completed, oxygen is evolved on the potassa side, and hydrogen shows itself in the acid by its secondary action. "Nitrate of potassa is of course formed at the junction of the acid and alkali. Now let us recollect what nitrate of potassa is in its electrical relations: it is an *oxinitrion of potassium* $(N + 6\,O) + P$. Aqueo-nitric acid is also an *oxinitrion of*

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* Schropp and Company, 1839.

hydrogen $(N + 6\,O) + H$; and potassa is oxide of potassium $(P + O)$. In their local action upon each other, the acid and the alkali are both decomposed; the oxinitrion of the former combines with the metal of the latter, and water is formed by the union of the hydrogen and oxygen. This water there is no difficulty in regarding as a separate and secondary product, inasmuch as the salt is incapable of combining chemically with it.

"When a circuit, however, is formed of proper conductors, the compositions and recompositions take place through a series of connected particles, as in the manner of all other electrolytic conduction: and the oxygen and hydrogen, instead of combining together, as in the local action, are respectively evolved at the zincode and platinode. The following diagrams may perhaps assist in explaining my notion of the origin and connexion of the current:—

$$\overparen{O\,P}\;\overparen{O\,P}\;\overparen{O\,P}\;\underset{B}{\overset{A}{|}}\;(N + 6\overparen{O)\;H}\;(N + 6\overparen{O)\;H}\;(N + 6\overparen{O)\;H}$$

"Let $\overparen{O\,P}$ and $(N + 6\overparen{O)\;H}$ represent the two electrolytes on the opposite sides of the diaphragm A B before the action; after action has commenced they may be represented thus:—

$$O\,\overparen{P\,O}\,\overparen{P\,O}\,\overparen{P\;\underset{B}{\overset{A}{|}}\;(N}+6\,O)\,\overparen{H\,(N}+6\,O)\,\overparen{H\,(N}+6\,O)\,H."$$

NEW COMPOUND OF PLATINA.

Messrs. Rogers and Boyé have stated to the American Philosophical Society the existence of a new compound of platina. It is prepared by evaporating a solution of platina in aqua regia to dryness, and adding in small portions at a time a great excess of aqua regia. The compound may be thus readily obtained by filtration, and pressing the powder between folds of blotting-paper. If the concentration of the liquid be carried too far, it is necessary to add just as much water as is sufficient to keep the mass in a semifluid state, and to prevent the precipitation of the deutochloride of platina.

The salt is perfectly well characterized, and is composed of deutochloride of platina and nitric oxide. It is of a gamboge yellow colour, and crystallizes distinctly, though on account of the smallness of the crystals, their form has not been yet determined; it is very deliquescent, and absorbs atmospheric moisture at common temperatures with great avidity. It is rapidly decomposed by the mere addition of water, which causes a brisk effervescence of nitric oxide, and deutochloride of platina remains in solution.

When this compound is heated to 212°, it does not yield its combined water. In order to determine the quantity of platina and chlorine, the salt was fused with carbonate of potash, and the platina thus obtained was weighed, the chlorine was then precipitated by a solution of nitrate of silver. The quantity of nitric oxide was determined by introducing a portion of the salt into a graduated tube, inverted over mercury, and by decomposing it with water.

The mean of experiments performed in different modes gave as the composition of this substance,—

Chlorine	43·89
Nitric oxide	4·98
Platina	41·26
Water and loss	9·87
	100.

BLUE OXIDE OF TITANIUM IN SCORIÆ.

In analysing the blue scoriæ of different countries, M. Kersten found small quantities of titanic acid, and that these scoriæ possessed a blue colour similar to that of the blue oxide prepared in the dry way; he presumed, therefore, that this colour, instead of being derived from the protoxides of iron and manganese, which sometimes occur in them, might be owing to titanic acid [oxide ?]. According to the facts above stated, it is very probable that titanic acid, which occurs very often in the ores of iron, after having been dissolved and scorified during the operations of the blast-furnace, is reduced to the state of oxide by the fused iron, similarly to what occurs in the humid way with solutions, and as has happened in several preceding assays. If this assumption be well founded, it ought to be possible, with the substances usually contained in the scoriæ and with titanic acid, to reproduce blue glass on the small scale. M. Kersten succeeded, not only in fusing together silica, lime, alumina, titanic acid and iron, all of them pure, and in producing earthy glasses of a blue colour, resembling the blue scoriæ of iron, but also succeeded in obtaining them with the same earths, titanic acid and zinc free from iron, or with pure tin.

M. Kersten, therefore, concludes from these researches, that the blue colouring matter of many iron scoriæ is the blue oxide of titanium. This blue oxide deserves some attention from its application to the arts, and the author endeavoured to procure blue enamels upon porcelain by using it; and though they were not so fine as those of cobalt, they most nearly approach them.—*L'Institut*, No. 353.

ON THE PROTEIN OF THE CRYSTALLINE HUMOUR.

This substance was discovered by Berzelius, and according to him it constitutes 35·9 per cent. of the crystalline humour, and does not become, like albumen, a coherent mass by coagulation, but a granular one. In other respects its properties are similar.

This new product may be obtained as follows:—

The crystalline humour is to be carefully separated from fifty ox-eyes, they are to be washed, the cells torn, water is to be added and filtered. The liquor heated in a water-bath soon coagulates and deposits clots. After drying, the pounded mass was subjected to the action of alcohol and water, and boiled; after this it was dried at 266° Fahrenheit.

Thus obtained this substance is white, and possesses all the properties of albumen, but is not easy to pulverise it. When it is put into a solution of potash in a silver vessel, the silver is blackened, which shows that it contains free sulphur: it contains no free phosphorus.

The composition of the crystalline humour [protein ?] is stated to be

Hydrogen....	6·94	or	62	atoms.
Carbon......	55·89	—	40	—
Oxygen	21·16	—	12	—
Azote	16·54	—	10	—

The quantity of sulphur is in the proportion of one atom to fifteen atoms of protein.

The entire crystalline humour dissolved in nitric acid, mixed with a solution of nitrate of iron and precipitated by ammonia, furnishes the same quantity of phosphate of lime as a hydrochloric solution of the same substance. The phosphoric acid is derived from the phosphate of lime; the substance therefore contains no free phosphorus.

If the crystalline humour in a very dry state be put into sulphuric acid, it swells, and is converted into a transparent gelatinous mass, like the protein of caseum, fibrin, &c. By the addition of water it contracts and yields a hard powder.

The author concludes that the principle of the crystalline humour is protein, for it possesses the same composition and atomic weight as the pure protein of albumen, fibrin, caseum, &c.—*Journal de Chim. Médicale,* Août, 1840.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1840.

Chiswick.—Sept. 1, 2. Fine. 3. Rain. 4. Cloudy: rain. 5, 6. Fine. 7, 8. Very fine. 9. Hazy. 10—13. Very fine. 14. Hazy: heavy rain. 15. Cloudy: rain at night. 16. Rain, with brisk S.W. wind: barometer exceedingly low. 17. Very fine: frosty at night. 18. Frosty haze: very fine. 19. Cloudy and cool. 20. Fine. 21. Fine: rain. 22. Heavy rain. 23. Rain: clear and fine at night. 24. Heavy showers. 25. Cold and wet. 26. Overcast: rain. 27. Cloudy and fine. 28. Heavy rain. 29, 30. Clear and fine.

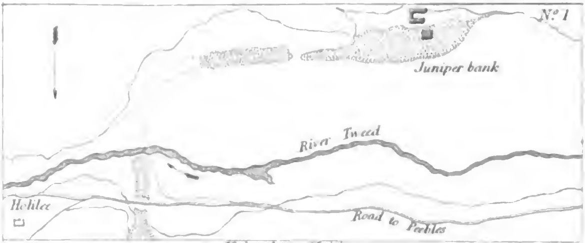
Boston.—Sept. 1. Cloudy. 2. Fine. 3. Rain: rain early A.M. 4—6. Fine. 7. Cloudy. 8. Fine. 9. Cloudy: rain early A.M. 10. Fine: rain early A.M. 11, 12. Fine. 13. Fine: rain P.M. 14. Cloudy. 15. Fine. 16. Fine: rain early A.M.: rain P.M. 17. Cloudy: rain early A.M. 18. Fine: rain P.M. 19, 20. Cloudy. 21. Cloudy: rain P.M. 22. Stormy and rain: rain A.M. 23. Rain: rain early A.M. 24. Fine: rain early A.M. 25. Rain: rain early A.M.: rain A.M. 26. Fine: rain P.M. 27. Fine. 28. Cloudy. 29. Fine: rain P.M. 30. Fine.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fine harvest day: air electric. 2. Rain from midday. 3, 4. Showery. 5. Fine and clear. 6. Fine but cloudy. 7. Fine: a few drops of rain. 8. Cloudy A.M.: rain P.M. 9. Wet: cleared up: wet again. 10, 11. Occasional heavy showers. 12. Moist, but moderate. 13. The same: one shower. 14. Fine and clear. 15. Cold and showery. 16. Rain A.M. 17, 18. Very fine. 19. Fine A.M.: moist P.M. 20. Fine A.M. 21. Fine A.M.: showery. 22. Fine and dry: thunder A.M. 23. Rain. 24. Fine and fair. 25—27. Very wet. 28, 29. Moist. 30. Showery.

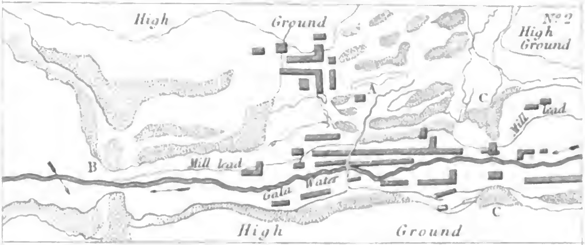
Sun shone out 28 days. Rain fell 21 days. Thunder 1 day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

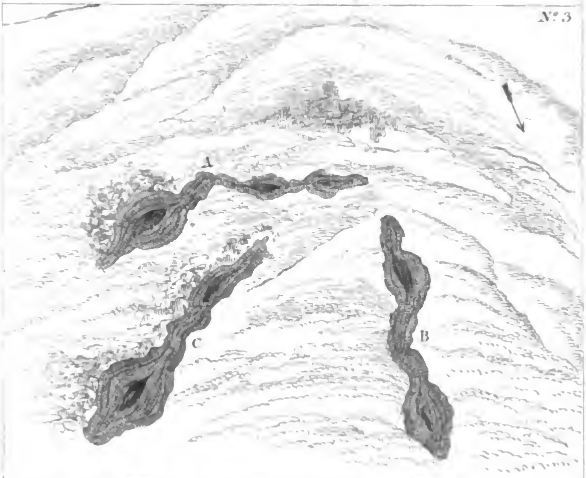
Days of Month. 1840. Sept.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·034	29·965	29·713	29·48	30·04	29·77	65·9	72·9	54·3	80	58	62	64	47½	N.	E.	E.	E.	...	...	...	...	63
2.	29·756	29·689	29·559	29·07	29·48	29·35	69·8	76·2	63·9	78	53	69	65½	51	S.	S.	calm	SE. SW	...	·22	...	...	66
☽ 3.	29·664	29·727	29·569	29·00	29·42	29·49	60·3	75·7	56·3	66	43	56	59	45½	W.	W.	calm	SW.	·236	·02	·21	...	61
4.	29·826	29·774	29·735	29·26	29·40	29·58	61·8	64·2	52·6	62	46	56	58	47	S.	SW.	W.	S.	...	·13	...	...	58
5.	30·012	30·063	29·927	29·44	29·75	29·95	60·0	64·8	51·2	68	40	57·5	58½	43½	W.	W.	calm	S.	·122	...	...	...	57
6.	30·276	30·196	30·112	29·50	30·03	29·90	59·8	67·2	50·8	74	53	62	59	44	S.	W.	calm	S.	...	...	...	...	55
7.	30·132	30·066	30·005	29·50	29·80	29·91	62·6	69·8	57·3	66	44	62	60	54	SW.	SW.	calm	SW.	...	·02	...	1·56	58
8.	30·180	30·105	30·073	29·58	29·88	29·78	58·4	68·0	52·3	71	51	58	57½	44	NW.	W.	calm	WSW.	...	·01	...	...	56
9.	30·044	29·986	29·880	29·37	29·58	29·48	62·3	66·7	56·7	68	59	60	64	53	S.	S.	W.	SSW.	...	·02	·02	...	57
10.	30·014	29·962	29·917	29·35	29·69	29·70	61·3	68·0	60·7	72	45	59	59	48	WNW.	W.	W.	SW.	·041	...	·04	...	59
○ 11.	29·994	29·933	29·901	29·30	29·66	29·63	60·0	66·8	51·9	69	40	57·5	54½	45	SW.	W.	W.	WSW.	...	...	...	...	55
12.	30·024	29·968	29·894	29·38	29·64	29·74	56·8	65·3	47·2	66	35	54	56	45½	W.	W.	W.	NW.	...	...	...	...	52
13.	29·900	29·856	29·643	29·27	29·67	29·52	53·8	62·2	45·2	65	34	54	58	43½	SW.	W.	W.	W.	...	...	...	...	48
14.	29·474	29·455	29·197	28·95	29·31	29·31	54·3	60·7	46·0	54	43	48·5	53½	40½	S.	E.	calm	E by S.	...	·40	·01	0·50	49
15.	29·314	29·290	29·100	28·75	29·27	29·04	51·2	59·4	45·9	59	41	50	55	42½	W.	W.	NW.	N by E.	·283	·05	...	...	50
16.	28·892	29·079	28·744	28·31	28·76	29·02	55·7	60·8	46·9	57	43	50	51½	44	S.	SW.	W.	NE.	·130	·19	·31	...	49
17.	29·550	29·718	29·470	28·88	29·32	29·68	55·3	60·0	45·7	62	29	50·5	53½	44½	SW var.	W.	W.	NE.	·111	...	·06	...	48
☾ 18.	29·896	29·875	29·856	29·36	29·90	30·01	51·3	59·3	41·7	62	48	53	55½	40	W.	NE.	N.	NE.	...	...	·05	...	45
19.	29·880	29·930	29·837	29·37	30·00	29·93	53·3	59·7	49·3	57	38	48·5	56	37½	NNW.	N.	N.	ENE.	...	...	·02	...	49
20.	30·036	30·002	29·995	29·50	29·87	29·80	51·3	56·6	42·4	62	32	48	54½	38	WNW.	W.	calm	SW.	...	...	...	...	44
21.	30·026	29·988	29·735	29·40	29·63	29·48	54·5	57·8	45·0	63	52	51·5	56	48	S.	S.	W.	SW.	...	·10	...	0·52	45
22.	29·500	29·497	29·482	28·87	29·20	29·15	55·2	62·5	52·4	53	40	51	56	42	S.	SW.	S.	E.	·083	·40	·22	...	50
23.	29·402	29·470	29·301	28·85	29·18	29·42	51·4	56·7	44·6	60	39	48	53	42	WSW.	S.	calm	ENE.	·333	·02	·07	...	45
24.	29·530	29·571	29·455	29·06	29·53	29·67	53·2	58·8	48·0	60	48	54	55	42	E.	W.	calm	S.	...	·36	·02	...	48
● 25.	29·796	29·984	29·702	29·30	29·83	29·94	50·2	59·3	50·0	57	35	52	57	35	NW.	N.	calm	S.	·097	·04	·47	...	49
26.	30·062	30·026	29·874	29·54	29·74	29·75	50·7	56·5	45·4	63	42	47	54½	41½	SW.	SW.	calm	SW.	·019	·16	·18	...	46
27.	30·014	29·967	29·899	29·36	29·62	29·60	55·4	60·6	49·0	65	49	54	53½	45	S.	W.	calm	SW.	·130	...	·13	...	48
28.	29·790	29·757	29·514	29·10	29·25	29·09	56·3	63·8	51·3	59	48	53	57	50	SE.	SW.	NW.	SW.	...	·30	...	...	52
29.	29·624	29·677	29·583	28·94	29·15	29·40	55·5	60·3	50·2	60	42	52	50½	46	S.	SW.	W.	SW.	·269	·01	·06	·096	53
30.	29·998	29·947	29·934	29·30	29·65	29·58	52·8	59·0	46·3	60	51	51	53	43	W.	W.	calm	SW.	...	...	·03	...	49
Mean.	29·821	29·817	29·686	29·21	29·575	29·589	67·3	63·3	50·0	53·93	44·03	54·3	56·6	44·4					Sum. 1·854	2·45	1·90	3·54	Mean. 52



Vale above Holilee



Town and Vale of Galashiels



Creux and Lapiaz, called Meigle pots

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

DECEMBER 1840.

LX. *On the Minerals found in the Neighbourhood of Glasgow. By* THOMAS THOMSON, *M.D., F.R.S. &c., Regius Professor of Chemistry in the University of Glasgow*[*].

PERHAPS there is no part of Great Britain, not even excepting Cornwall, or the mining counties of the north of England, so rich in mineral species as the neighbourhood of Glasgow, if we include under that appellation Lead Hills and Wanlock Head, and the ranges of mountains on both sides of the Clyde, constituting the Kilpatrick hills, and the ranges of hills behind Greenock and Port Glasgow, extending as far as Kilmacolm.

The mines at Lead Hills began to be wrought during the reign of James IV., under the name of gold-mines, and it is said by Boethius that he extracted from them a considerable treasure. The jaspers, rubies and diamonds which Boethius describes in glowing terms, were doubtless the different species of lead ore which still exist in these mines, namely, the sulphate, carbonate and phosphate of lead, remarkable at once for the beauty of their colours, their figure and their lustre.

In the time of James V. Lead Hills was a lead mine, as it is at present; and it is particularly described by Agricola in his celebrated work, *De re metallica*. Now Agricola died in the year 1555.

Becher, who first attempted to establish a theory in chemistry, visited the Lead Hills mines in the year 1682, and ever since that period they have been wrought to a considerable extent.

The principal ore at Lead Hills is galena or sulphuret of lead, which occurs in the mine in all the different varieties which that prolific mineral is known to assume. The gangue of the vein which accompanies the ore is most commonly sulphate of barytes, which occurs in abundance, and in a state of great purity. Calcareous spar also is common, and arragonite is met with in very beautiful crystals.

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[*] Read at the British Association at Glasgow, and now communicated by the Author.

Besides galena, no fewer than nine species of lead ore occur in Lead Hills, some of which have not hitherto been observed anywhere else.

Of these nine species I may take a short review.

1. *Sulphate of lead.*—Beautiful specimens of this species of lead ore occur at Wanlock, crystallized, and perfectly white. The crystal is a right rhombic prism, and the lustre is adamantine, but I think less so than that of carbonate of lead. Magellan, in his English edition of Cronstedt's Mineralogy, published in 1788, says, that this species was discovered by Monnet, but I do not find any allusion to sulphate of lead in Monnet's *Mineralogie*, published in 1779; so that the discovery of its composition must have been later than that period. Klaproth first subjected this mineral to analysis in 1802. The specimens which he examined were from Anglesey and from Wanlock Head. I analysed a specimen from Wanlock Head, and found it contained no other foreign matter than a trace of water. It constitutes one of the rarest of the ores of lead.

2. *Carbonate of lead.*—This species is much more abundant than the last. It occurs pretty frequently in crystals, constituting right rhombic prisms, considerably more oblique than those of the sulphates: the composition of carbonate of lead ore was known to Monnet in 1779. It has the diamond lustre in great perfection. This species, when made artificially, is a well-known paint called *white-lead*. It was first analysed by Klaproth in 1802. The specimen which he examined was from Lead Hills; I analysed a very fine crystal from the same locality, and found it a pure carbonate of lead, with the exception of a mere trace of water.

3. *Cupreo-sulphate of lead.*—This species of lead ore was first noticed by Mr. Sowerby, and so far as I know, it has been hitherto met with only at Lead Hills. It has the colour of the finest specimens of blue carbonate of copper. Its shape approaches very nearly to that of sulphate of lead.

Mr. Brooke analysed a few grains of it, and found it composed of one atom sulphate of lead, one atom oxide of copper, and one atom of water. By the kindness of Mr. Brown, I got a specimen sufficiently large to enable me to subject it to analysis; the specific gravity was 5·2137. I obtained

Sulphate of lead ...	74·8,	or 1 atom.
Oxide of copper ...	19·7,	or 1 atom.
Water..............	5·5,	or $1\frac{1}{4}$ atom.
	100·0	

It is therefore a compound of one atom of sulphate of lead and one atom of hydrate of copper. The blue colour shows that the oxide of copper which it contains is combined with water.

4. *Sulphato-carbonate of lead.* — This species has been hitherto observed only at Lead Hills, at least so far as I know; the colour is yellowish or greenish white; it is usually crystallized in oblique four-sided prisms. Mr. Brooke analysed it and described it in 1820. By the kindness of Mr. Brown, I got a sufficient quantity of it to subject it to analysis. I found the specific gravity to be 6·3197, which is rather less than that given by Mr. Brooke. My analysis agrees very nearly indeed with that of Brooke. I obtained

Carbonate of lead	46·04
Sulphate of lead ...	53·96
	100·00

showing that it is a compound of one atom of carbonate and one atom of sulphate of lead combined together.

5. *Sulphato-tricarbonate of lead.*—This species is also peculiar to Lead Hills. It was first noticed by Bournon. Mr. Brooke described and analysed it in 1820. By the kindness of Mr. Brown, I was enabled to subject it to analysis a few months ago. The lustre is splendent, and the colour yellowish-white. It occurs crystallized both in rhomboids and prisms. I found its specific gravity exactly 6·000. Its constituents were

Carbonate of lead	72·57,	or 3 atoms.
Sulphate of lead	27·43,	or 1 atom.
	100·00	

This almost coincides with the previous analyses of Brooke and of Stromeyer, who also subjected this mineral to analysis.

6. *Phosphate of lead.*—This species occurs in Lead Hills and Wanlock Head in considerable quantity, and exhibits a variety of colours, chiefly green, yellow and brown. It is frequently crystallized in six-sided prisms, but more commonly in these mines in cauliflower-like vegetations.

It is curious that phosphate of lead has never yet been found native except mixed with chloride of lead. The proportions vary somewhat; but the average may be stated one atom of chloride of lead and eight atoms subsesquiphosphate of lead. There is usually a small quantity of oxide of iron present, to which in all probability the colour of the mineral is owing. I examined five specimens of this mineral from Lead Hills; the average quantity of chloride of lead in them was 10¼ per cent.; the average quantity of protoxide of iron was 1⅓ per cent.; the rest was a compound of 1½ atom of oxide of lead and one atom of phosphoric acid.

The colour of these specimens was grass-green, olive-green, yellow and brown. The specific gravity varied from 6·70016 to 6·5781. None of them contained any phosphate of lime. This was the case also with the specimens analysed by Wöhler and Berthier, and with those that Klaproth had analysed. But some years ago Mr. Charles Kersten of Freyberg published an analysis of seven specimens of phosphate of lead from various localities, five of which contained phosphate of lime, varying in the different specimens from 12 per cent. to 0·59 per cent. The specific gravity of these specimens, which contain much phosphate of lime, is lower than of the others. That which contains 12 per cent. has a specific gravity of only 5·092.

Some months ago I got two specimens of phosphate of lead from Mr. Brown, locality Lead Hills, still lighter than any I had before seen. The first specimen was of a light greenish-yellow colour, and a slaty texture, and had a specific gravity of only 5·366. It contained no less than 15 per cent. of phosphate of lime.

The other specimen was dark-green, and constituted a botryoidal concretion upon the surface of the first kind. Its specific gravity was 5·970, and it contained rather more than nine per cent. of phosphate of lime.

From the great difference of the quantity of phosphate of lime in different specimens, it would seem rather to be mechanically mixed with than chemically combined with the phosphate of lead.

7. *Cupreo-sulphato-carbonate of lead.*—This is the scarcest of all the ores of lead which occur at Lead Hills. It has a fine green colour, precisely like that of malachite, and occurs most frequently in mineral crystals, having for their primary form, according to Brooke, a right rhombic prism with angles of 95° and 85°. It was first noticed by Mr. Sowerby under the name of green carbonate of copper. It was first described and analysed by Mr. Brooke in 1820.

I could only employ for analysis 3·29 grains of this mineral; but as the analysis is simple, and I employed every precaution I could think of to avoid error, I have reason to believe that the result approaches pretty near the truth. The specific gravity by my trials was 5·000, but the specimen was not absolutely pure.

From 3·29 grains, I obtained

Sulphate of lead	1·74	
Carbonate of lead ...	1·05	
Oxide of copper ...	0·44	—— 3·23.

The 0·06 gr. wanting to make up the original weight was foreign matter and water.

According to this analysis 100 parts of the ore would have given

		Atoms.	
Sulphate of lead...	52·88	1·46	1½
Carbonate of lead.	31·91	1	1
Oxide of copper..	13·37	1·4	1½
Impurity and water	1·84		
	100		

Mr. Brooke supposes the copper to exist in this mineral in the state of carbonate, but the result of the analysis just stated, is quite inconsistent with that opinion. It would, however, be desirable, in order to be quite certain, to repeat the analysis on a larger quantity of the mineral than I could procure.

8. *Chromo-phosphate of lead.*—This beautiful mineral exists in the Wanlock Head mine in considerable quantity. It has a beautiful orange colour, without any of the red tinge which characterizes chromate of lead. It is often in cauliflower concretions; but it occurs also crystallized in six-sided prisms, constituting the common form which characterizes phosphate of lead. I ascertained many years ago that this beautiful orange colour which distinguishes this ore was owing to the presence of about two per cent. of chromate of lead; the rest was a mixture of subsesquiphosphate of lead and chloride of lead in nearly the same proportions as in the green varieties.

9. *Vanadiate of lead.*—This very rare ore has hitherto been found only in Mexico and at Lead Hills. It was analysed in 1804 by Del Rio, who announced the existence in it of a new metal. This was contradicted by Collet Descotils, who affirmed that what Del Rio took for a new metal was chromium; and certainly the resemblance between chromium and vanadium is considerable.

The specimens in my possession I got several years ago from Mr. Doran, an Irish mineral dealer, who told me that he picked them up in an old neglected lead mine in the county of Wicklow, in Ireland. But there is reason to suspect that he was mistaken.

The colour is light brownish yellow, and, like phosphate of lead, it is crytallized in six-sided prisms.

My specimen was analysed a good many years ago by my nephew, Dr. R. D. Thomson, who found it a compound of 1 atom of chloride of lead, and nearly 9 atoms of sesquivanadiate of lead *.

Besides these ten species of lead ore which occur at Lead Hills or Wanlock Head, there are found also native copper and malachite, or hydrous dicarbonate of copper; a beautiful green-coloured mineral often crystallized, and when large specimens occur, as the well known ores from Siberia, they are often cut into ornamental tables, &c.

The Lead Hills malachite is rather a dark green, but the specimen of it subjected to analysis was very nearly pure.

Lead Hills also contains specimens of blende or sulphuret of zinc. By far the finest specimens of hydrous silicate of zinc which I have seen, are from Lead Hills; they constitute beautiful white crusts or plates. Its specific gravity is 3·1645, and its constituents, as determined by analysis, are

Silica..................	23·2	1
Oxide of zinc	66·8	1·12
Water	10·8	0·82
	100·8	

or it is a simple silicate of zinc with rather less than an atom of water. It is curious that in all the analyses of this mineral hitherto made the water never amounts to an atom. In that made by Mr. Smithson the water was only 4½ per cent. By Berzelius's analysis in 1819, it amounted to 7½ per cent., and in one analysis of a specimen from Lead Hills it was 10·8 per cent., or almost 11 per cent. The probability is that it undergoes a kind of efflorescence by keeping, and that originally the water in the ore was exactly an atom. If that supposition be correct, the water originally must be 13½ per cent.

The Kilpatrick hills, which bound the valley of the Clyde from the Stockey muir to Dumbarton, and also the corresponding but lower range on the south side of the valley are composed of various trap rocks, among which amygdaloid is pretty common. Now amygdaloid is a rock full of almond-shaped cavities of various sizes, usually filled up by crystallized minerals, many of which, though not the whole, belong to the beautiful tribe of zeolites. We may, therefore, divide the minerals found in these mountains into two sets: 1. The *zeolites*, so called because they froth before the blow-pipe, and they owe this frothing property to the great quantity of water which they contain, and which is easily driven off by heat. 2. Minerals nearly destitute of water, which in general, though not in all cases, exist in greater quantities than the zeolites, and may be often considered as constituting an integrant portion of the substance of the mountain in which they occur.

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* Records of General Science, vol. i. p. 34.

The zeolites, including under the term one or two minerals, which agree with them in containing water and in the way in which they occur, amount to about fifteen species, and there are several which are peculiar to these mountains, or at least have not hitherto been met with anywhere else. I shall take a short view of them.

1. *Stellite.*—This species has hitherto been observed only in the rift of a greenstone rock situated on the banks of the Great Canal, a little to the east of Kilsyth. The rock had been blasted, and a good deal of it brought to the neighbourhood to mend the horse-path of the canal. My son accidentally observed the *stellite* as we were walking on the banks of the canal, and drew my attention to it.

It is snow-white, and always in crystals, which issue like rays from several centres. Each circle of crystals is firmly attached to the rock. The shape of the crystal cannot easily be determined, from its small size and irregularity of form. It is rather harder than calcareous spar, and has a specific gravity of 2·612. This exceeds that of zeolite in general, showing that it contains less water than most others. But it agrees with the zeolites in melting with effervescence before the blow-pipe into an enamel. It is composed of $4\ Cal\ S^2 + Mg\ S^2 + Al\ S + 2\frac{1}{2}\ Aq$, according to the analysis of myself and Dr. R. D. Thomson.

2. *Thomsonite.*—This mineral, together with the three that are to follow it, were confounded together by Cronstedt under the name of *zeolite*, in his paper on the subject published in the Memoirs of the Stockholm Academy for 1756. In his Mineralogy he attempted to subdivide the zeolite into species, but his descriptions are too imperfect to enable us to recognize the minerals to which he alludes. Werner afterwards divided the zeolites into needlestone, radiated zeolite and foliated zeolite. Haüy afterwards attempted a more exact arrangement, and divided the zeolites into two species, which he distinguished by the names of *mesotype* and *stilbite*. In 1817, Fuchs and Gehlen made an accurate chemical analysis of a number of zeolites, and showed that the mesotype of Haüy contains three distinct species, which they distinguished by the names of *natrolite*, *mesolite* and *scolezite*. In 1820, Mr. Brooke, without being aware of what had been done by Fuchs and

Gehlen, showed that the mesotype of Haüy ought to be divided into three species, which he distinguished by the names of mesotype, needlestone, and Thomsonite. The first two of these constitute the natrolite and scolezite of Fuchs and Gehlen; but the third is a new species, which Mr. Brooke first described. He showed that these minerals differ in their crystalline shape and in their specific gravity; Thomsonite being the heaviest, and natrolite or mesotype the lightest.

Thomsonite is a beautiful mineral, and is rather abundant both in the Kilpatrick hills, and those on the south side of the Clyde. When pure it is snow-white; the crystal is a right rectangular prism. The lustre is vitreous, and the specific gravity is about 2·37, according to Brooke; though I have found it as low as 2·29. Before the blow-pipe it swells. It contains about 13 per cent. of water, and is composed of $3\,\mathrm{Al\,S}+\mathrm{Cal\,S}+2\tfrac{1}{2}\,\mathrm{Aq}$.

There is a curious variety which occurs at Ballimony, in the north of Ireland; its colour is brown, and it is not crystallized. Its specific gravity is only 2·29; but its chemical analysis shows the same constitution as the Thomsonite of Kilpatrick*.

There is another variety which occurs at Port Rush, which deserves attention. It constitutes small spheres about the size of a pea in an amygdaloidal rock; these spherules are composed of needles radiating from the centre to the circumference. Colour white, with a slight tinge of yellow; translucent. Lustre vitreous. Specific gravity 2·366, and it is rather softer than common Thomsonite. It froths before the blowpipe, like the other zeolites. Its composition is the same as that of Thomsonite; but its proportion of alumina is rather less, and it contains 6⅓ per cent. of soda, and the proportion of water is less. If we reckon

Thomsonite $3\,\mathrm{Cal\,S}+13\,\mathrm{Al\,S}+10\,\mathrm{Aq}$, the Port Rush mineral will be $3\,\mathrm{Cal\,S}+12\,\mathrm{Al\,S}+\mathrm{N\,S}+8\,\mathrm{Aq}$.

My son, to whom I was indebted for the chemical analysis and description of this mineral, distinguished it by the name of Scoulerite, in honour of Dr. Scouler, to whose zeal and abilities the mineralogy of Ireland lies under so many obligations.

3. *Natrolite.*—This mineral constitutes the mesotype of Brooke. The name natrolite was first applied to a yellow mammillary variety from Högan, near Lake Constance, in which he discovered no less than 6½ per cent. of soda. When pure it is quite white, and the crystal is a right rhombic prism, deviating by only 1° 10′ from a rectangular prism. The

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* Analysed by Dr. R. D. Thomson.

prism is commonly terminated by eight faces, constituting two four-sided prisms of different inclinations.

It contains about 16½ per cent. of soda, and 9½ per cent. of water. The other constituents are silica and alumina.

Its constitution is $3\ \mathrm{Al\,S} + \mathrm{N\,S}^3 + 2\ \mathrm{Aq}$.

Mesolite.—Natrolite in a pure state, that is perfectly destitute of lime, is uncommon in this neighbourhood. Almost always we find a mixture of soda and lime constituting the *mesolite* of Fuchs and Gehlen; they gave it that name because they considered it as intermediate between natrolite and scolezite, or composed of an integrant particle of natrolite united to an integrant particle of scolezite.

Mesolite is abundant both in this locality and in various other parts of Scotland, where trap rocks occur; and we find it in two different states.

(1.) In four-sided prisms generally united so firmly to each other, that we cannot easily separate them. Fine specimens of this variety are found at present at Bishoptown in Renfrewshire, where a hill is perforating for a tunnel, constituting part of the Greenock and Glasgow rail-road, at present forming. This variety approaches very nearly to natrolite; since it contains only 1½ per cent. of lime, while there is no less than 13⅓ per cent. of soda, and a small portion of potash. If natrolite be

$$3\ \mathrm{Al\,S} + \mathrm{N\,S}^3 + 2\ \mathrm{Aq},$$

this variety will be

$$3\ \mathrm{Al\,S} + (\tfrac{30}{36}\ \mathrm{N} + \tfrac{1}{36}\ \mathrm{K} + \tfrac{5}{36}\ \mathrm{Cal})\ \mathrm{S}^3 + 2\ \mathrm{Aq}.$$

Its specific gravity is 2·1365.

The other variety constitutes veins in the rock. It is much softer, has a foliated texture, and a specific gravity of 2·212.

It contains 6½ per cent. of soda and 3 per cent. of lime; so that the atoms of soda are very nearly twice as numerous as those of lime. In other respects its constitution agrees nearly with the other variety.

4. *Scolezite.*—This is the mineral which Werner distinguished by the name of needle zeolite. It occurs often in long needles, as fine as human hair, and quite flexible; sometimes in needles of a longer and larger size, and irregularly interlaced; sometimes the needles are firmly attached to each other, so as to give it a splintery appearance. In this state it is harder than calcareous spar; whereas the hairy variety is quite soft. In some specimens the central portion is hard and splintery, while the outer portion is quite soft and friable. We would naturally suppose that this friability was owing to the loss of a portion of water; but on examining some re-

markable specimens of this kind in my possession, I found that the friable portion contained more water than the hard and splintery.

The crystalline shape of scolezite is the same as that of natrolite, a right rhombic prism. The obtuse angle is 10′ greater than in natrolite, being 91° 20′, while that of natrolite is 91° 10′.

Scolezite differs from natrolite in lime replacing the soda. It contains also more water, being

$$3\ \mathrm{Al\ S} + \mathrm{Cal\ S}^3 + 3\ \mathrm{Aq}.$$

It deserves attention, that though in some specimens the soft hairy portion of this mineral appears to be a continuation of the fibres of the splintery central portion, yet the constitution of the two portions is not the same.

I analysed a remarkable specimen of this kind from the Giant's Causeway. The result was as follows:—

	Central splinters.	
Silica......................	48·88	46·0
Alumina	26·36	27·6
Lime with trace of ♂	7·64	15·2
Magnesia	2·46	—
Soda...	4·20	—
Water	12·32	14·35
	101·86	103·15

The difference will be best seen by stating the following formulas :—

Central splintery part

$$12\ \mathrm{Al\ S}^{1\frac{1}{4}} + 2\ \mathrm{Cal\ S}^{1\frac{1}{4}} + \mathrm{Mg\ S}^{1\frac{1}{4}} + \mathrm{N\ S}^{1\frac{1}{4}} + 11\ \mathrm{Aq}.$$

Soft outer part

$$12\ \mathrm{Al\ S}^{1\frac{1}{4}} + 4\ \mathrm{Cal\ S}^{1\frac{1}{4}} + 13\ \mathrm{Aq}.$$

5. *Glottalite.*—The specimen of this mineral in my possession is, I believe, from the hills behind Port Glasgow: at least I so understood Mr. Clachers, from whom I got it. The specimen is unique.

It is white, crystallized apparently in octahedrons, has a vitreous lustre, is translucent, is harder than calcareous spar, and has a specific gravity of 2·181. It contains 21¼ per cent. of water. Its constituents are $\mathrm{Cal\ S} + \mathrm{Al\ S}^{1\frac{1}{2}} + 3\ \mathrm{Aq}$.

6. *Laumonite.*—This mineral was found occasionally in the Kilpatrick hills, but beautiful specimens have been recently obtained at Bishoptown, while digging the tunnel for the rail-road.

Its colour is white, usually with a shade of red. It contains 16½ per cent. of water, which it loses spontaneously when exposed to the atmosphere, and falls to powder unless protected by a coat of gum.

It is usually crystallized, and its primary form is an oblique rhombic prism. The acute angles of the prism are of 84° 30′, and the base of the prism makes with the lateral faces angles of 114° 54′, and 65° 6′. The specific gravity is 2·361, and it is harder than calcareous spar.

Its constitution is $3\ Al\ S^2 + Cal\ S^2 + 5\ Aq$. The only difference which I have found in the Bishoptown laumonite is the presence of 2 per cent. of magnesia in it. Neither Leopold Gmelin, nor Vogel, nor Dufresnoy, who analysed this mineral, take any notice of magnesia as a constituent. I was led to look for it in consequence of the considerable deficiency which occurred in my analysis. Vogel gives 2½ per cent. of carbonic acid. This must be given merely to make up the deficiency in his analysis. It would not be surprising if his specimen, like mine, had contained magnesia.

7. *Chabazite.*—This name was applied by Box d'Antic to our mineral. The Greek term *χαβαζιος* occurs in a poem ascribed to Orpheus, in which twenty kinds of stones are celebrated for their medicinal virtues, but without any description. The last of the twenty is *χαβαζιος*. Chabazite occurs pretty frequently in this neighbourhood, particularly about Kilmacolm. It is always in rhomboids, approaching nearly to the cube, the obtuse angles being 94° 56′. It is usually transparent, and has a vitreous lustre. The specific gravity of the rhomboidal crystals is from 2·076 to 2·088. Its constituents are

Silica…………	49·20	24·6
Alumina………	17·91	7·96
Lime…………	9·64	2·75
Potash………	1·92	0·32
Water………	20·41	$18{\cdot}14^{11{\cdot}03}$
	99·08	

$$3\ Al\ S^2 + Cal\ S^3 + 6\ Aq.$$

There is another variety of chabazite which never occurs here, but is pretty common in the north of Ireland. The crystal is never rhomboid, but a very peculiar figure, of which the only way of forming an idea is to examine the model. The primary faces are nearly all concealed; yet it cleaves easily in the direction of the primary faces and yields a rhomboid similar to the primary crystal.

Now this variety differs from the common chabazite of this neighbourhood, both in its specific gravity and its composition. Its specific gravity is 2·472.

And its constituents (the Port Rush variety) are

Silica............	48·988	24·49	
Alumina	19·774	8·8 ...	3
Soda	6·066	1·51	1
Lime............	4·068	1·16	
Protoxide of ♂	0·404		
Water	20·700	.. ...	6
	100·000		

Here the constitution is the same as in the rhomboidal variety, excepting that a considerable portion of the lime is replaced by soda. In a specimen from Oberstein, analysed by Berzelius, there was no lime at all, but only soda. From this it is obvious that chabazite, like mesotype, is divisible into three species or varieties. The first or rhomboidal chabazite is a hydrous compound of bisilicate of alumina and tersilicate of lime. In the trihedral variety the tersilicate of lime is replaced by tersilicate of soda; and the specimens from Port Rush contain both tersilicate of lime and tersilicate of soda, being analogous to mesolite.

I believe it will be ultimately found that every modification of a crystalline form is occasioned either by a difference in the constitution, or by the action of some peculiar foreign matter during the act of crystallizing.

It may be worth while to mention, that a red-coloured chabazite occurs at Kilmacolm; its characters correspond with those of common chabazite; and its constituents are the same, excepting that it contains 2·75 per cent. of oxide of iron, to which, doubtless, its red colour is owing.

But about two months ago I received from Messrs. Jackson and Alger of New York (well known as the authors of an interesting and valuable paper on the geology of Nova Scotia), a specimen of a mineral which they have distinguished by the name of Acadiolite or yellow chabazite. It is in crystals, having the common form of chabazite, and its other properties (colour excepted) are the same as those of common chabazite. Its specific gravity is 2·0202. But it contains rather more silica and less alumina than common chabazite; and there was found in it 2·4 per cent. of red oxide of iron, to which its colour was doubtless owing. It contains neither potash nor soda, but simply 11·6 per cent. of lime. The formula for its constitution is

$2\ Al\,S^3 + Cal\,S^2 + 6\ Aq$; that of common chabazite
$2\frac{1}{2}\ Al\,S^2 + Cal\,S^3 + 6\ Aq$.

The deficiency of alumina in the acadiolite, which amounts to 5·5 per cent., prevents us from reducing it under the common formula of chabazite. But the formula which it gives is simpler, and may ultimately be found to belong to pure chabazite.

8. *Analcime.*—This mineral, which Werner distinguished by the name of *cubizite*, was first noticed by Dolomieu. It occurs rather abundantly in the Kilpatrick hills. In these hills it is white and translucent; but in the Giant's Causeway in Ireland, transparent crystals of it are found. It is crystallized in cubes, whose angles are usually replaced by three planes. In most crystals these planes have increased so much in size as to obliterate the primary faces. The crystal is then an imperfect sphere bounded by twenty-four equal trapezoidal planes. This figure is well known to mineralogists by the name of the *leucite crystal.*

It is but an imperfect zeolite; yet it melts before the blowpipe, though without ebullition; and like the other zeolites, is soluble in muriatic acid. Its constituents are silica, alumina, soda and water.

		Atoms.	
Silica............	55·36	27·68	
Alumina	23·00	10·22	2·8
Soda	14·19	3·55	1
Water	8·08	7·18$^{13·77}$	
	100·63		

$$3\,Al\,S^3 + N\,S^2 + 2\,Aq.$$

The translucent and transparent analcimes have the same composition.

9. *Cluthalite.*—I have given this name to a mineral which occurs in the Kilpatrick hills near Dumbarton, forming large nodules in the amygdaloid of that district.

Its colour is flesh-red; it forms a congeries of imperfect crystals, seemingly rectangular prisms. It is nearly opake, has a vitreous lustre, is harder than calcareous spar, and has a specific gravity of 2·166.

Its constitution bears a certain analogy to analcime, the alumina being partly replaced by magnesia, and the soda by peroxide of iron.

Silica..................	51·266	... 9·35
Alumina	23·560	4
Magnesia	1·233	
Soda	5·130	1
Peroxide of iron ...	7·306	
Water	10·558	... 3·43
	99·048	

$$4\,(Al + Mg)\,S^2 + (f\,N)\,S^2 + 3\,Aq.$$

But it contains an additional atom of bisilicate of alumina and of water.

10. *Stilbite* and *Heulandite*.—These two species are found in great abundance in the Kilpatrick hills and at Carberry in the valley which divides the Campsie hills from the Kilpatrick hills. They are always, or almost always, of a deep flesh-red colour, and generally crystallized, though seldom in very regular shapes. These two species were first distinguished from each other by Werner, who gave to stilbite the name of *radiated zeolite*, and to Heulandite that of *foliated zeolite*. Haüy confounded the two under the name of stilbite. Mr. Brooke pointed out the difference between them in 1822; and left Haüy's name *stilbite* to one of the species, while the other was named Heulandite, in honour of Mr. Heuland of London. It is unlucky that the names had not been reversed. The word *stilbite* (from the Greek *στιλβω*, to *shine*) was applied by Haüy, in consequence of the great lustre of the mineral. Now the base of the prism in Heulandite has a much greater lustre than in stilbite, being pearly and splendent. In the specimens from this neighbourhood, the lustre is not so great as in those from the Feroe islands, which are white, and have the pearly lustre in perfection.

The constituents of both these minerals are silica, alumina, lime and water.

Heulandite contains most silica and least water.

Stilbite is $2\,\mathrm{Al\,S^3} + \mathrm{Cal\,S^3} + 5\,\mathrm{Aq}$.
Heulandite $3\frac{1}{2}\,\mathrm{Al\,S^3} + \mathrm{Cal\,S^3} + 6\,\mathrm{Aq}$.

11. The only other zeolite that occurs in any quantity in the Kilpatrick hills is *harmotome*. It was noticed early by mineralogists under the name of *cross stone*, in consequence of the remarkable way in which the crystals frequently cross each other; that shape is not common in the harmotome of the Kilpatrick hills. It is usually crystallized in right rectangular prisms. But the common modification is a terminal four-sided pyramid, consisting of faces replacing the solid angles of the base of the prism.

The mineral called harmotome ought to be divided into three distinct species, which might be distinguished by the names harmotome, Philipsite, and Morvenite; the first of these only occurs in our neighbourhood; but the first and third are common at Strontian. The second, or Philipsite, has been observed in Sicily, and in other places. The first is a compound of silica, alumina, barytes and water; the second of silica, alumina, lime and water; and in the third the proportions of the constituents differ very much from those of the second species.

Harmotome is $2\frac{1}{2}\ \mathrm{Al\ S^3} + \mathrm{Br\ S^{14}} + 4\ \mathrm{Aq.}$
Philipsite is $3\ \mathrm{Al\ S^2} + (\frac{2}{3}\ \mathrm{Cal} + \frac{1}{3}\ \mathrm{K})\ \mathrm{S^2} + 5\ \mathrm{Aq.}$
Morvenite is $5\ \mathrm{Al\ S^4} + \mathrm{Cal\ S^4} + 11\ \mathrm{Aq.}$

Morvenite is transparent, while the other two are only translucent.

12. These are all the minerals in the neighbourhood of Glasgow which belong to the tribe of zeolites; they all contain water as an essential constituent; they all froth before the blow-pipe, and they are all soluble in muriatic acid. As they fill cavities in amygdaloid, there is some reason for suspecting that they were deposited in these cavities after the trap rocks had assumed their present form. There is another mineral which occurs in the same situation, which contains water as a constituent, and which is soluble in muriatic acid, I mean the dihydrous peroxide of iron found in a trap rock near Gourock.

It has a reddish brown, and is composed of very fine needles. But at St. Just in Cornwall, it occurs in crystals, the primary form of which is a right rhombic prism. It has an imperfect metallic lustre, is harder than apatite, and has a specific gravity of 4·375. Its constituents are,

2 atoms peroxide of iron	10
1 atom of water	1·125.

Carbonate of magnesia has also been found recently at Bishoptown; I am not aware of its having been observed in any other part of Great Britain. It abounds in Hindostan, and Hoboken in New Jersey; in both places in serpentine. About two years ago I got a great many very pure specimens from Madras, for which I was indebted to Dr. Cole.

There are twenty other species of minerals which occur in the trap rocks in this neighbourhood; some of them as veins, others making one of the constituents of the rocks. The veins often contain water as a constituent, though not always. But those minerals that enter as constituents into the rocks are destitute of water.

As veins we have sulphate of barytes, calcareous spar, fibrous sulphate of lime, arragonite, Wollastonite and prasolite.

1. Fine specimens of arragonite are found at Lead Hills.

2. The name Wollastonite has been applied by Haüy to bisilicate of lime, the table spar of Werner. But as this new name has not been generally adopted, and as mineralogy lies under the deepest obligations to Dr. Wollaston, I have given his name to a mineral found in considerable quantity in veins in a greenstone rock near Kilsyth, not far from the great canal, and which Lord Greenock has also found near Edinburgh.

It is white, has a fibrous texture, and the fibres are in tufts diverging from a centre. Lustre silky, translucent on the edges, softer than calcareous spar. Specific gravity 2·850.

It is composed of silica, lime and soda, with a little magnesia. If we include the magnesia along with the soda it will be composed of

3 atoms bisilicate of lime.
1 atom tersilicate of soda.

$$3\,\mathrm{Cal}\,S^2 + \mathrm{N}\,S^3$$ *

3. *Prasolite* is a name by which I have distinguished a *mineral* which occurs in the Kilpatrick hills, constituting a vein about an inch in thickness. It was brought me by one of my students about two years ago, who wanted to know what mineral it was. From its appearance I pronounced it to be common *sulphate of lime*. But happening to examine it a little more closely, I found that I had been mistaken in my opinion, and that it, in fact, constituted a new mineral species, allied to the zeolites; since it contained no less than 18 per cent. of water.

Its colour is leek-green, and it is as soft as Venetian talc, not being capable of scratching selenite. The specific gravity is 2·311. It is composed of fibres very loosely cohering together, since it may be crumbled to powder between the fingers.

Its constituents, besides water, are silica, magnesia, peroxide of iron and alumina, and probably soda. Its constitution may be represented thus:—

$$9\,\mathrm{Mg}\,S^{1\frac{1}{2}} + 4\,\mathrm{f}\,S^{1\frac{1}{2}} + 3\,\mathrm{Al}\,S^{1\frac{1}{2}} + 18\,\mathrm{Aq}, \text{ or}$$
$$3\,(\tfrac{3}{4}\,\mathrm{Mg} + \tfrac{1}{4}\,\mathrm{Al})\,S^{1\frac{1}{2}} + \mathrm{f}\,S^{1\frac{1}{2}} + 4\tfrac{1}{2}\,\mathrm{Aq};$$

Fluor spar is found sparingly in cubic crystals in the trap rocks near Gourock.

Prehnite is so exceedingly abundant, that at one time a whole cartful of it was brought to Glasgow.

I need not notice augite and amphibole, felspar and albite, which constitute the common constituents of the greenstone rocks so abundant in this part of Scotland. But it is right to notice *labradorite*, which constitutes one of the constituents of a peculiar variety of greenstone which may be seen in a state of perfection in Campsie glen, and which constitutes the principal rock in the hills south of Paisley. In these rocks it is very conspicuous, because they have been altered by the action of the weather, and the labradorite has become white, while the hornblende, the other constituent, retains its dark colour.

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* Analysed by myself and Dr. R. D. Thomson. Records of General Science, i. 220.

Labradorite, as the name implies, was first noticed on the coast of Labrador. It is of a dark smoke-gray colour, and in particular positions reflects a variety of colours, blue, green, yellow, red and brown. The crystals approach those of felspar, but differ a little in the measurement of the angles. The constituents are silica, alumina and lime, with a trace of soda. The presence of lime distinguishes it from albite; and the proportion of silica is much smaller.

Felspar is $4\,\mathrm{Al\,S^3} + \mathrm{K\,S^3}$
Albite...... $3\,\mathrm{Al\,S^3} + \mathrm{N\,S^3}$
Labradorite $3\,\mathrm{Al\,S^2} + (\tfrac{2}{3}\,\mathrm{Cal} + \tfrac{1}{3}\,\mathrm{N})\,\mathrm{S}$.

I think it unnecessary to mention mica, epidote, steatite, iron pyrites or carbonate of iron, which constitutes so abundant a mineral in this neighbourhood. Nor is it necessary to give an account of the gray ore of manganese which has been met with, though sparingly, in the hills in our neighbourhood. But there are two species of minerals which I believe to be peculiar to the range of hills in our district, and which, therefore, deserve to be described. These are Kilpatrick quartz, and sulphuret of cadmium, or Greenockite, as it has been called.

1. *Kilpatrick quartz.*—This variety of quartz occurs in the amygdaloid of the Kilpatrick hills.

It is white and translucent, and constitutes spheres about the size of a hazel-nut, mixed abundantly with stilbite and calcareous spar. The spheres constitute an aggregation of crystals; the exterior termination of each, examined by the microscope, is a four-sided prism. The hardness is the same as that of common quartz; but the specific gravity is 2·525, while that of common quartz is 2·690. It contains 3 per cent. of water, according to the analysis of Dr. R. D. Thomson, who found also a trace of sulphuric acid; while common quartz is anhydrous.

These differences seem to warrant the propriety of considering it as a peculiar species of quartz. The same mineral exists in the mountains of Nova Scotia, which bear a striking analogy with those in the neighbourhood of Glasgow, both in their constitution and in the minerals which they contain.

2. *Sulphuret of cadmium.*—This mineral occurs along with prehnite in an amygdaloidal rock at Bishoptown in Renfrewshire. It has also been seen on the Cochno burn on the north side of the Clyde. It was mistaken for blende by the mineral dealers in the neighbourhood. Lord Greenock examined it in the summer of 1840, and showed to Professor Jameson that it could not be blende. Mr. Connel subjected

it to analysis, and found it to be pure *sulphuret of cadmium.* His analysis was performed on 3·71 grains.

It is always crystallized in six-sided pyramids. The summit is sometimes replaced by a more acute pyramid: some of the crystals show a six-sided prism.

Translucent to transparent.

Lustre vitreous, or sometimes almost adamantine.

Hardness about 2·75.

Specific gravity 4·534; but the specimen was not quite pure. Mr. Connel states the specific gravity to be 4·842; but the determination was made on 3·68 grains. If the impurity in my specimen was prehnite, the specific gravity of pure sulphuret of cadmium will be 4·546.

On subjecting it to analysis I obtained

Sulphur......	22·4	or 1·009 atom.
Cadmium ...	77·6	or 1 atom.
	100·0	

This very nearly agrees with Mr. Connel's analysis, who obtained from 3·71 grains of the mineral,

Sulphur	22·56	
Cadmium	77·30	——99·86

Professor Jameson has distinguished this mineral by the name of Greenockite, in honour of Lord Greenock, who first discovered it.

LXI. *Deductions from the first Year's Observations at the Magnetic Observatory at Prague: in a Letter from* Professor KREIL, *Director of the Prague Observatory, to* Major EDWARD SABINE, *R.A., V.P.R.S.*[*]

Prague, July 21, 1840.

I HAVE seen with great pleasure, by your last letter and its inclosure, that you have made known in English[†], my letter to M. Kupffer, containing the results of the Milan Magnetic Observations, and that you have sent it to the newly-established Observatories. I am indeed far from regarding all the deductions from those observations as undoubted facts; I consider them rather as indications afforded by the first made continuous series of observations, and awaiting confirmation or correction from subsequent researches.

Since my arrival at Prague, I have had the advantage of several zealous assistants, and have been able to make the observations with the magnetic instruments at much shorter intervals than was done at Milan, namely, at every hour from 5 a.m. to 10 p.m.*. This we have done uninterruptedly for a twelvemonth, viz. from July 1839 to June 1840, and I have now the pleasure of laying before you a summary view of the principal results.

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[*] A translation communicated by Major Sabine.

[†] See Lond. and Edinb. Phil. Mag., vol. xvi. p. 241.

1. The variations of the magnetic elements exhibit a dependence on the seasons, for there appear in certain months maxima and minima of which scarcely a trace is discernible in the opposite months. It is therefore desirable to separate the observations of the winter from those of the summer, and to consider each apart. By uniting them we should risk obtaining a daily march which would be wholly imaginary, and would not correspond with the phænomena in *any* month of the year. Thus, for example, the declination in the winter months shows regularly a minimum in the later hours of the evening; in summer the minimum occurs in the morning; the mean of the whole year, therefore, would show two minima, one in the evening, and the other in the morning, whereas the occurrence of two minima on the same day is in every month of the year an exception to the ordinary course. I do not consider the difference in the time of the minima in summer and winter a mere displacement arising from the season; at least it is not so in the case of the inclination, in which a similar difference is observable: but this point must be decided by future observations continued through the hours of the night.

In the winter months, October to March, we observed,

			Diff.
At 8 a.m. Göttingen mean time,	the decl.	= 410·77†	
			16·34
At 1 p.m. ——————	the max.	= 427·11	
			19·27
At 10 p.m. ——— ———	the min.	= 407·84	

In the summer months, April to September,

At 8 a.m. ——————	the min.	= 398·01	
			29·49
At 1 p.m. ——————	the max.	= 427·50	
			21·74
At 10 p.m. ——————	——	= 405·74	

The time of the maximum does not seem to be dependent on the season; the exact time obtained from the hourly observations by interpolation, taking into account the second differences to quarters of an hour, is $0^h\ 45^m$ in May, September and December; this is the earliest: in February it is at $1^h\ 45^m$; and the latest occurs in July, viz. at $2^h\ 0^m$. The monthly means gave the daily difference greatest in August,

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* We have now extended our observations so as to include some of the hours of the night.

† As the observations of the absolute declination have not yet been made with the requisite exactness, the variations are here given in divisions of the scale, each division being 27·2261 seconds of arc.

= 36·01; in April 33·01; and least in December, = 8·41, if the observations at 8 a.m. and 1 p.m. are compared, or 13·06 if those at 1 p.m. and 10 p.m. are compared. The secular decrease appears to have been unusually great this year; if the apparatus showed correctly, it amounted from July 1839 to June 1840, to no less than 17·70 divisions of the scale, = 8′ 01″·9; but as in October, 1839, the thread by which the needle was suspended broke, this result may be inexact. The observations of the following month, however, show a still greater decrease. If we take the mean of all the observations from 5 a.m. to 10 p.m., we obtain the following declinations:

1839.	November.	Declination	= 417·80
	December.	———	= 416·88
1840.	January.	———	= 415·80
	February.	———	= 414·74
	March.	———	= 410·35
	April.	———	= 407·50
	May.	———	= 402·87
	June.	———	= 399·30

The declination, therefore, decreased in these eight months 18·50 scale divisions, or 8′ 23″·7.

2. The horizontal intensity observed with the bifilar had in the winter months its

Minimum ......=544·42* at 11 a.m.
Maximum ......=564·83 at 10 p.m.

Difference ......= 20·41

In summer, its

Minimum ......=443·63 at 10 a.m.
Maximum ......=488·90 at 8 p.m.

Difference...... = 45·27

The minimum was earliest in August, when it took place at 9 a.m., and in June, when it occurred at 9ʰ 30ᵐ a.m.; it was latest in December, at 1 p.m.; and in January was at 11ʰ 45ᵐ a.m. The maximum appears to fall in winter, i. e. in December, January and February, during the hours of the night; it was earliest, at 8 p.m. in July, November, April and June. The following are the means of all the observations between 5 a.m. and 10 p.m.

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* A scale-division in this apparatus corresponds to 18·5757 seconds of arc, or $\frac{1}{15853}$ of the whole horizontal intensity; but it should be remarked, that since the instrument was set up in May, 1839, the magnetism of the bar has not been examined, in order that the series might not be broken.

1839. July. Horizontal intensity = 532·41
August. ——— = 465·54
September. ——— = 488·85
October. ——— = 489·12
November. ——— = 559·27
December. ——— = 598·58
1840. January. ——— = 586·91
February. ——— = 552·68
March. ——— = 560·01
April. ——— = 498·00
May. ——— = 437·17
June. ——— = 396·01

The numbers here given are not corrected for the influence of temperature, because it appears to affect not only the intensity, but also the direction of the magnetic force, and we must wait for more certain data. The continued and simultaneous observations of the bifilar magnetometer and the inclinatorium have shown, indeed, that changes of temperature are always accompanied by changes in the horizontal intensity; but they are also accompanied by corresponding alterations in the dip, which increases as the horizontal intensity decreases, and *vice versâ*.

In some months of the year a decrease takes place in the horizontal intensity between 2 and 5 p.m., which may be ascribed to the maximum of inclination which occurs in those hours; this was previously recognized in the Milan observations.

3. In winter the inclination attains its minimum at 6 a.m., and its maximum at 3 p.m.

Minimum	270·69*
Maximum	272·02
Difference	1·33

In summer the minimum is at 5 a.m., and the maximum at 8 a.m.

Minimum	289·70
Maximum	291·23
Difference......	1·53

The several months show at all seasons a small minimum of the inclination about noon, whence we must conclude that the two maxima, one in the forenoon and the other in the afternoon, always occur, though in the summer months the former is the most conspicuous, and in the winter the latter; the observations which I had previously made at Milan were not sufficiently numerous to manifest the occurrence of this noon-minimum.

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* The value of a scale-division = 28·185 seconds of arc. The apparatus was set up in June, 1839, but was not sufficiently steady to give value to the observations with it until August; it continued, however, subject to a tremulous motion occasioned by passing carriages until February, 1840, when this disadvantage was remedied.

Observations made in May and June, 1840, at 2 and 4 a.m., show a maximum and minimum in the hours of the night, which sometimes exceed those above-mentioned.

In May, at 11 p.m. the maximum	= 246·43
at 2 a.m. the minimum...... ...	= 244·49
Difference	1·94
In June, at 11 p.m. the maximum	= 267·73
at 4 a.m. the minimum	= 266·53
Difference......	1·20

The circumstance already noticed, that these maxima are in some months almost insensible, makes it difficult to recognize the dependence of the hour of their occurrence on the season of the year.

The following numbers, which are the monthly means of all the observations of the inclination made between 5 a.m. and 10 p.m., will show to what considerable alterations that element is subject in the course of a longer period.

1839.	August.	Inclination	= 368·06
	September.	———	= 374·42
	October.	———	= 366·42
	November.	———	= 336·40
	December.	———	= 294·18
1840.	January.	———	= 252·14
	February.	———	= 196·59
	March.	———	= 182·91
	April.	———	= 197·47
	May.	———	= 245·59
	June.	———	= 266·70

If we compare these numbers with the means of the horizontal intensity, we shall not, it is true, see any perfectly parallel march, which indeed we ought not to expect, as the horizontal component depends on the total intensity as well as on the dip; but it is sufficiently clear that there is a general accordance, the horizontal intensity increasing with the decrease of dip, and *vice versâ*: therefore the changes of inclination indicated by the instrument are not to be ascribed to mere alterations of the centre of gravity in reference to the point of suspension, but are, in part at least, due to the altered direction of the force itself. It would seem then that the dip is subject to much greater alterations than has been hitherto recognized; and this is quite conceivable, if we suppose temperature to be one of the chief causes of the variation of the magnetic elements; for if the *daily* progress of the temperature from east to west produces the large *diurnal* variation in the *declination*, its *annual* march from south to north, and *vice versâ*, ought in like manner to occasion an *annual* variation in the *inclination*.

4. The times of oscillation of the dipping needle showed during the winter,—

		s.	Diff.
At 8 a.m. a maximum	=	12·86978	
At 10 a.m. a minimum	=	12·85321	0·01657
At 2 p.m. a second maximum	=	12·86923	0·01602
At 8 p.m. a second minimum	=	12·84823	0·02100
In summer,			
At midnight, a maximum.			
At 6 p.m. a minimum	=	12·55078	
At 2 p.m. a second maximum	=	12·57437	0·02359
At 9 p.m. a second minimum	=	12·56605	0·00832

The midnight maximum was shown by the night observations of May and June.

The dependence of the hours of maximum and minimum on the season could not be recognized with certainty from the observations of each month separately considered; it appeared, however, as if those of the forenoon observed in winter approached progressively nearer to noon.

The following are the monthly means of all the observations made from 5 a.m. to 10 p.m.

				s.
1839.	August.	Time of vibr.	=	12·02318
	September.	———	=	11·86037
	October.	———	=	11·78188
	November.	———	=	11·90803
	December.	———	=	12·69148
1840.	January.	———	=	13·36133
	February.	———	=	13·40867
	March.	———	=	14·00672
	April.	———	=	13·76645
	May.	———	=	12·77688
	June.	———	=	12·39025

From these numbers, which are not corrected for the influence of temperature, or for any decrease in the magnetism of the bar, we cannot trace a connexion with the temperature, such as has been usually supposed to exist, viz. a decreased force in increased temperature, and *vice versâ*; but there is a correspondence between these numbers and the monthly means of the inclination; the inclination having decreased and the times of oscillation increased from October to March, whilst subsequently to March the inclination increased, and the times of oscillation decreased. Such observations, continued with instruments somewhat differently constructed, but having the same purpose in view, will soon show what part of this apparent connexion in the variations of the intensity and inclination is due to the instrument, and what to the phænomena themselves.

5. The influence of the moon on the magnetic condition of the earth was examined in the same manner as was done in the case of the Milan observations. Having corrected approximately the observed horizontal intensities for temperature and the loss of magnetism sustained by the bar, there appeared a confirmation of the results previously obtained, viz. that the magnetism of the earth is stronger at the time of the new moon than when the moon is full. The mean of the whole body of the observations showed an intensity

During the last quarter = 549·99
At the new moon = 548·79
During the first quarter = 542·62
At the full moon = 541·11

6. The difficulty referred to in par. 2. of freeing the observations of the horizontal intensity from the influence of temperature, and the uncertainty of the correction for the decrease of magnetism in the bar, induced me to adopt a mode of arranging the observations in reference to the inquiry relatively to the moon, by which the errors arising from both these causes might be avoided. If the moon has an influence on the magnetic condition of the earth, it must produce a daily variation, masked by the greater effect produced by the sun, but recognisable when the latter is eliminated. I constructed, therefore, tables, having for their argument the day of the month, and for the titles of the several columns the different distances of the moon from the magnetic meridian, i. e. her magnetic horary angles. To simplify the calculation, I assumed that the moon passed the magnetic meridian an hour earlier than the true meridian. I deducted from each observation the monthly mean corresponding to the solar time of the observation; thus eliminating the influence of the sun, considered as the cause of the regular diurnal variation. This deduction being made, the remainders represent the sum of all other influences; but as these numbers were sometimes + and sometimes — I augmented each by a

constant quantity. The numbers thus obtained were entered in the tables, each in the column corresponding to the distance of the moon from the magnetic meridian at the time of the observation. From the great number of observations that were in this combination, it might be expected that the effects of other influences would disappear, and that of the moon alone would be visible. The two horizontal elements, the declination and intensity, were thus treated, and the means obtained for the separate months were combined in a yearly mean, which is represented in the following table:—

I. Declination in Scale-divisions, each = 27″·2261.

Eastern Horary Angles.	Declin.	Declin.	Western Horary Angles.
12	11·44	11·21	11
13	10·96	10·79	10
14	11·14	11·01	9
15	10·78	10·64	8
16	10·76	10·24	7
17	10·55	10·69	6
18	10·51	10·42	5
19	10·52	10·15	4
20	10·66	10·16	3
21	10·48	10·32	2
22	10·54	10·46	1
23	10·97	10·62	0

From this table we may draw the following conclusions:—

I. If we take the sums of the declinations during the eastern and during the western horary angles, we find the first sum to exceed the second by 2·60 scale-divisions = 70″·79; therefore the declination is greater when the moon is east of the meridian, as already shown by the Milan observations.

II. If we compare the sum of the declinations corresponding to the horary angles from 6^h to 17^h with those from 18^h to 5^h, the first sum exceeds the second by 4·40 scale-divisions = 119″·79; the declination is therefore greater when the moon is in the neighbourhood of the inferior meridian: and the table shows that it is greatest at the hour when the moon is *on* the inferior meridian.

III. From the comparison of the sums of the declinations corresponding to the horary angles from 21^h to 2^h with the sums of those from 18^h to 20^h, and 3^h to 5^h, it results that the first sum exceeds the second by 0·97 scale-divisions = 26″·41; therefore it appears that at the time of the moon's passing the superior meridian, a second maximum of the declination does take place, though considerably less than the one indicated above.

The results as to the horizontal intensity are contained in the following table:—

II. Horizontal Intensity.

Eastern Hor. Angles.	Intensity.	Intensity.	Western Hor. Angles.
12	32·92	34·15	11
13	32·62	33·33	10
14	32·78	32·63	9
15	33·64	32·52	8
16	32·62	33·12	7
17	32·03	31·53	6
18	31·11	31·90	5
19	30·96	32·16	4
20	30·16	31·77	3
21	29·45	29·96	2
22	29·07	30·31	1
23	29·92	30·04	0

From this table it appears:—

I. That the intensity is stronger when the moon is west of the magnetic meridian, for the sum of the intensities corresponding to western horary angles exceeds by 5·14 scale-divisions the sum of the intensities corresponding to eastern horary angles.

II. The intensity is decidedly greater when the moon is in the neighbourhood of the inferior meridian than when she is near the superior meridian, for the horary angles from 6^h to 17^h give a sum greater by 28·08 scale-divisions than do the horary angles from 18^h to 5^h.

7. The increased frequency and the greater number of the Prague observations enabled us to pursue the interesting phænomena of the magnetic perturbations with more exactness than we had been able to do at Milan. We proceeded in the following manner with both the horizontal elements. The changes were noted which had taken place between each observation and the next, and the sum of these (ζ) was taken for each day without reference to their signs. The monthly mean of these sums was taken and called Σ. Now if on any day the quotient $\frac{\zeta}{\Sigma}$ was found greater than 2, that day was reckoned one of disturbance.

It hence appeared that the magnetic elements frequently sustain a very considerable change of brief duration; though during the remaining hours of the day, the perturbations may so little exceed the average, that on the whole it may not be a day of disturbance according to the above definition.

This phænomenon, which may be called a *magnetic shock*, ought to be brought into notice, being, in fact, a perturbation of short duration, greater perturbations consisting only of several such shocks. As a definition of a magnetic shock, let ϱ be the change in either of the magnetic elements between two successive observations, and σ the average change in the same month between every successive pair of observations; then every change for which $\frac{\rho}{\sigma} > 2$, is to be regarded as a magnetic shock.

The following table shows for each month the number of days of disturbance according to the above definition; and also the number of separate shocks which took place in addition, namely, on days which were not those of disturbance.

Table of Disturbances.

Month.	Declination.		Horizontal Intensity.	
	Days of Perturbation.	Shock.	Days of Perturbation.	Shocks.
1839. July.	1	1	5	2
Aug.	3	1	7	2
Sept.	5	4	4	10
Oct.	7	15	10	3
Nov.	8	7	16	...
Dec.	10	15	25	2
1840. Jan.	14	12	15	14
Feb.	7	8	9	...
March.	5	1	16	2
April.	2	5	11	2
May.	2	1	13	1
June.	1	1	9	6

From this table we may infer:—

I. That the perturbations are much more frequent in the winter than in the summer months; which may be caused, in part, by the force which produces the regular diurnal changes being much weaker in winter: but the very great perturbations which take place chiefly in the winter months, indicate that the disturbing forces have actually more intensity at that season of the year. The greatest disturbances observed in the twelvemonth occurred on the following days:—

4th and 15th of September.
18th, 22nd, and 23rd of October.
23rd ....of November.
4th and 18th of January.
6th, 7th, and 9th of February.
29th and 30th of May.

II. The days of disturbance are more numerous in the horizontal intensity than in the declination.

III. In this year also several great perturbations were observed to occur on the same days on which the same phænomenon had taken place in preceding years. The days which particularly deserve notice in this respect, are about the 18th of January, from the 18th to the 22nd of February, and the 18th of October.

Great disturbances were observed in

1837.	January	16th.	1837.	February	18th.
1838.	———	17th.	1838.	———	16th and 21st.
1839.	———	19th.	1839.	———	18th and 21st.
1840.	———	18th.	1840.	———	18th and 21st.
1836.	February	17th.			

In February, 1840, the disturbance of the horizontal intensity was but feebly marked, probably in consequence of the very great perturbations which took place in the same month on the 6th, 7th, and 9th.

The recurrence in October is a particularly marked one;

1836.	October	18th.
1837.	———	18th.
1838.	———	17th.
1839.	———	18th.

The two periods, February and October, are about equidistant from the winter solstice.

8. If we put together, without respect to signs, the shocks which occur at the different hours of the day, both on the perturbation days and on others, we obtain as a final result the following table, exhibiting the number of scale-divisions by which the needles were displaced by disturbances.

Sum of the Displacements.

Hours.	Declination.	Horizontal Intensity.
17 to 19	536·44	1373·36
20 to 22	463·90	1746·14
23 to 1	497·95	1425·33
2 to 4	697·76	1472·05
5 to 7	1475·16	1911·15
8 to 10	1481·17	2964·23

This table shows that the least disturbance takes place in the declination from 8 to 10 a.m., and the greatest from 8 to 10 p.m., a result which had already appeared from the Milan observations. In the horizontal intensity also the disturbances are more frequent in the evening than in the morning.

9. If we now take the signs into account, and call an augmentation of either element +, and a diminution —, the following table exhibits the remainders when the one sum is taken from the other.

Direction of the Changes.

Hours.	Declination.	Horizontal Intensity.
17 to 19	— 36·92	—1219·69
20 to 22	+224·56	—1193·38
23 to 1	+203·34	— 120·13
2 to 4	—405·36	— 104·75
5 to 7	—654·88	+ 94·37
8 to 10	—556·19	— 295·81

These numbers confirm for the declination the deduction already derived from the Milan observations, viz. that the declination is increased by the disturbances in the forenoon and middle of the day, and diminished by those occurring in the evening hours. In respect to the horizontal intensity, the negative signs are the prevailing ones, so that, in general, disturbances diminish this element, which is also in correspondence with the previous deductions; but it also appears from the numbers, that this occurs in a much higher degree during the hours of the night and morning than in the forenoon and afternoon.

10. During the greater disturbances we did not fail to observe for several hours, from 5 minutes to 5 minutes, and to study the march of the phænomena in all the elements with as much exactness as possible. Ten perturbations were thus observed, and the following circumstances were noticed as common to them all. These were before partly indicated by former observations, and appear to give a character of regularity to phænomena which at first sight might be regarded as wholly irregular.

I. Although the general effect of a disturbance is to weaken, on the whole, the horizontal intensity, considerable augmentations of that element do take place, but are of brief duration, and always preceding the diminution.

II. The horizontal intensity remains weaker for some time after the great oscillations have ceased, and only gradually resumes its ordinary force.

III. All the alterations of this element are accompanied by changes of the dip, and may chiefly be ascribed to them; an increase of dip, and a diminution of the horizontal intensity always taking place together, and *vice versâ*.

IV. The vibration of the dipping-needle is more rapid during disturbances, consequently the total force is increased*; and as we have seen that the horizontal intensity is weakened at such times, the influence of the increase of dip preponderates over the augmentation of the total intensity. In countries where the dip is much less than at Prague this may not always be the case, and the horizontal intensity may increase during a disturbance.

V. The variations of the total intensity frequently occur simultaneously with those of the dip and horizontal force. In half the number of perturbations which were observed continuously, the strongest total intensity coincided with the highest dip and least horizontal intensity, or the weakest total intensity with the smallest dip and greatest horizontal intensity; affording a further evidence that the variations of the horizontal component are chiefly due to the changes of dip.

VI. In a strong disturbance all the three elements were usually affected, but in a variable degree, which may probably

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* This is in contradiction to the 23rd paragraph in my letter to M. Kupffer, (Phil. Mag., April 1840, p. 249), and I hope that it may prove a rectification. I consider the method applied to the Milan observations an unsafe one, as by it the daily means of the times of vibration on days of disturbance were compared with the average time of the whole month. Such a comparison cannot show with exactness the small effect which the perturbations produce in this element, because in the course of a month it is subject to too great changes, whether real or instrumental. The greater frequency of the observations in each day at Prague, enabled me to proceed in a different manner, namely by comparing the mean of a disturbed day with those of the preceding and following days. Occasionally, indeed, the observations of the same day evidenced the nature of the change produced by the disturbance; the times observed before the commencement of the perturbation being sensibly longer than those which were determined during its continuance: thus the following times of vibration were observed on the 23rd of November.

Before the disturbance.				After it had commenced.			
h.	m.		s.	h.	m.		s.
3	30	Time of vibr. =	12·1569	9	0	Time of vibr. =	12·0724
4	30	——— =	12·1670	10	0	——— =	12·0894
6	0	——— =	12·1531	11	0	——— =	12·0752
8	0	——— =	12·1424	12	0	——— =	12·0624
				13	0	——— =	12·0901

The following day the time of vibration returned to its previous duration.

depend on the angle which the direction of the disturbing force makes with that of the regular force. By the hourly observations, the 23rd of March appeared to be a day of greater disturbance than any other in the month in respect to the horizontal intensity, although not a single shock was shown by the declination magnetometer. Had, however, the observations been continuous instead of hourly, the declination might also have been seen to have been disturbed.

VII. The more these phænomena are studied the more strong becomes the impression of the importance of observing them at short intervals. Those of 5 minutes are too long, for the bar may alter its position many hundred scale-divisions in that interval, as we found by the Prague bifilar on the 22nd of October. On days of great disturbances, or when the aurora borealis is seen, we observe the two horizontal elements, here and at Göttingen, uninterruptedly for several hours at intervals of 15 or 20 seconds: it is much to be desired that this practice should become more general.

In my letter to M. Kupffer, I noticed certain vertical vibrations supposed to be produced by earthquakes. By a mistake of the pen these were said to have taken place in the dipping-needle, whereas it was in the declination magnetometer that they occurred. In Prague this instrument is suspended to a beam, which is supported by the principal walls of the house, and such vibrations only take place during very violent winds. In such cases there can be no doubt that they are due to this mechanical cause.

In addition to the magnetic observations, we now note the temperature of the earth from one to five feet deep, and the height and temperature of the Moldau. I wish to add observations on the temperature of the dew-point and the intensity of the sun's rays, and shall be much obliged to you to inform me the price of the actinometer and the dew-point hygrometer, that I may request our government to purchase these instruments.

I remain, with the highest esteem, &c. &c.

Major Sabine, R.A. CARL KREIL.

LXII. *On* Mr. Potter's *Application of* Huyghens's *Principle in Physical Optics. By* JOHN TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN your October Number, p. 243, Mr. Potter gives some examples of the application of the principle above-named; from which he contends that the result of this principle is

" that light ought to bend into the shadows of bodies to an indefinite extent, as sound is known to pass through all apertures and to bend round all obstacles." Consequently, " that the result Mr. Airy (Tracts, p. 270.) has obtained by an approximate method is not to be depended upon, and that the objection to the undulatory theory, which was believed to have been removed, remains in full force." p. 246.

I thought it possible that Mr. Airy might reply to this, but as he has not yet done so, I beg to be allowed to offer a few words, in order to point out the mistake which I conceive Mr. Potter has made.

The expressions, which Mr. Potter obtains for the intensity of the light, have reference only to a certain line C B. He has not shown that there ought to be any light in the shadow except on this line. But a luminous line is merely a geometrical conception, from which no inference can be drawn as to the sensible intensity of the light in the shadow where this line is found.

It appears then that Mr. Potter has mistaken a luminous *line* for a luminous *space*; and, consequently, that his conclusions have, in reality, no foundation.

The meaning of what I have here stated may be illustrated by a fact which Mr. Potter has mentioned. The intensity of the light at certain points on a line in the centre of the shadow of a circular disc is, according to the theory, the same as if the light passed uninterruptedly. Now, when the disc is very small, and the light properly managed, a luminous spot may be observed in the centre of the shadow: when the disc is larger the spot vanishes; not from the diminution of its brightness, but from the diminution of its magnitude.

I am, Gentlemen, yours, &c.

Littlemore, Clitheroe, Nov. 5, 1840. JOHN TOVEY.

LXIII. *On the Focal Properties of Surfaces of the Second Order. By* JAMES BOOTH, *A.M., of Trinity College, Dublin; Principal of and Professor of Mathematics in Bristol College.*

WHETHER properties of surfaces of the second degree exist, analogous to those of the foci of the conic sections, has long been a subject of inquiry with the most distinguished geometers; among whom may be mentioned as preeminent in researches of this nature M. Chasles and Professor MacCullagh, who have arrived independently at a series of discoveries, of which may be stated as the most important and fundamental, the property, " that in the principal planes of a surface of the second order, there exist conic sections, termed by him *eccentric conics*, confocal to the sections of the surface in the principal planes; possessing properties analogous to the foci of curves of the second order; and cylinders perpendicular to those planes, bearing an analogy to the directrices of the same curves;" but this and other theorems of the same class, may be considered rather as the limiting relations of confocal surfaces, than as analogous to the well-known properties of curves of the second degree; and this view of the subject is further confirmed by the consideration, that the theorems above alluded to fail in the very case most analogous to that of the conic sections, when the surface is one of revolution round the transverse axe.

M. Chasles, indeed, in a very elegant memoir published now more than ten years ago *, has given several of the analogous theorems in the case of surfaces of revolution round the transverse axe, but has not hitherto extended his researches to the case of oblate spheroids, or to that of surfaces having three unequal axes, to do which is the object of the following pages.

By a simple application of a new method, which has now been for some time published †, I have been led to the discovery of a very extensive class of properties of surfaces of the second degree, hitherto, I believe, entirely unknown; from which may be easily deduced a series of theorems, relative to curves of the second order, none of which, so far as I am aware, have been yet given to the public.

The restricted limits of the present communication preclude the possibility of giving more than an outline of the theory, and the enunciations of several new theorems, the demonstrations of several of which being somewhat tedious, have been suppressed; but this can cause no difficulty to any moderately expert analyst.

(1.) Let a, b, c, denote the three semiaxes in the order of magnitude of a surface of the second order, which for brevity may be represented by the symbol (Σ), and let the eccentricities of the three principal sections of the surface (Σ) be e, ε, η, so that

$$e^2 = \frac{a^2 - b^2}{a^2}; \quad \varepsilon^2 = \frac{a^2 - c^2}{a^2}; \quad \eta^2 = \frac{b^2 - c^2}{b^2}.$$

(2.) Let u denote the semidiameter of the surface passing through one of its *umbilici*, and let a point be assumed on this diameter, at the distance $u \epsilon$ from the centre; this point may be termed a *focus* of the surface. Hence in general a surface of the second order has *four* FOCI, situated on the umbilical diameters.

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* See the *Nouveaux Mémoires de l'Académie Royale de Bruxelles*, vol. v.

† See a short treatise by the author, "On the application of a new Analytic Method to the theory of Curves and Curved Surfaces." Dublin, Hodges and Smith, 1840.

(3.) Let a plane be drawn parallel to one of the circular sections of the surface, meeting the umbilical diameter conjugate to this circular section, at the distance $\frac{u}{\epsilon}$ from the centre; this plane may be termed a *directrix plane* of the surface.

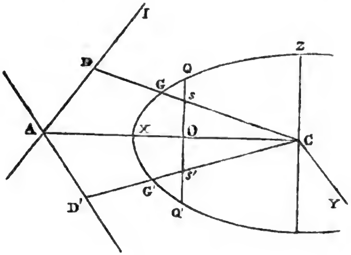
This plane, and the focus of the surface, on the diameter of the surface, conjugate to this plane, are *polar plane* and *pole*, relative to the given surface. Hence in general a surface (Σ) has *four* DIRECTRIX planes, parallel two by two, to its circular sections.

Each pair of those planes intersect in the directrices of the principal section, whose semiaxes are a and b; and these pairs may be termed *conjugate directrix planes.*

The foci of the surface, which are the poles of the conjugate directrix planes, may be called *conjugate foci.*

The line joining a pair of conjugate foci is perpendicular to the plane of the principal section in the plane of $x y$, whose semiaxes are a and b.

(4.) Let the middle point of this line be called the *focal centre* of the surface. Hence in general a surface of the second order has *four* FOCI and *two* FOCAL CENTRES.



Thus C is the centre of (Σ).

G and G′ are the umbilici, s and s' the conjugate foci of the surface; A D, A D′, the conjugate directrix planes; O the focal centre of (Σ); or the focus of the principal section of

(Σ) in the plane of $x y$, $CG = u$, $CX = a$, $CZ = c$, $CY = b$, $CD = \frac{u}{\varepsilon}$, $Cs = u\,\varepsilon$, $CO = a\,e$, $CA = \frac{a}{e}$.

(5.) Let $\theta = DAC$, be the angle which a directrix plane (or which a circular section of the surface as being parallel to it) makes with the plane of $x y$, or with the plane of the principal section, whose semiaxes are a and b; then

$$\cos\theta = \frac{\eta}{\varepsilon}.$$

(6.) Let ω be the angle which the umbilical diameter u makes with the corresponding directrix plane, then

$$\tan^2\omega = \tan^2 CDI = \frac{a^2 c^2}{(a^2-b^2)(b^2-c^2)}; \text{ or } \sin\omega = \frac{a\,c}{b\,u}.$$

Hence in two cases, the directrix plane is perpendicular to the diameter conjugate to it; either when the surface is one of revolution round the transverse axe, when $b = c$, or when (Σ) is an oblate spheroid, in which case $a = b$.

When the surface is an elliptic paraboloid, let l and l' be the semiparameters of the parabolas in the planes of $x y$ and $x z$; then $\theta = \omega$, and $\sin^2\omega = \frac{l'}{l}$.

(7.) When (Σ) is a surface of revolution round the transverse axe, or the axis of X, $\eta = 0$; and in this case the conjugate directrix planes A D, A D′ coincide and become perpendicular to the transverse axe C X, at the distance $\frac{a}{e}$ from the centre; and the foci of the surface coincide with the focal centre; but when (Σ) is an oblate spheroid $e = 0$, and $\varepsilon = \eta$; in this case then $\theta = 0$, or the conjugate directrix planes become parallel to the plane of $x y$, distant from it by $\frac{c}{\varepsilon}$; and the focal centre coincides with the centre of the surface.

(8.) When the surface is an elliptic paraboloid, one pair of conjugate directrix planes is infinitely distant, so that this surface has but *two* foci, and *two* directrix planes.

(9.) When the surface is a cone, the conjugate directrix planes pass through the vertex, and are parallel to the circular sections of the cone; the foci of the surface, and the focal centres all coincide with the vertex.

(10.) The distance of a focus of the surface (Σ) from the focal centre, or from the plane of $x y$, is $= \frac{c\,b\,\eta}{a}$.

(11.) The line $O\,Q = \frac{c\,b}{a}$, or the cord of (Σ) passing through a pair of conjugate foci $= \frac{2\,c\,b}{a}$.

(12.) The length of the perpendicular from one of the foci of the surface on the corresponding directrix plane, is $= \frac{c^3}{a\,b\,\varepsilon}$.

(13.) The length of the perpendicular from the centre on one of the directrix planes $= \frac{a\,c}{b\,\varepsilon}$.

(14.) The segment of the cord joining a pair of conjugate foci, intercepted between the plane of $x\,y$ and one of the directrix planes, is $= \frac{c\,b}{a\,\eta}$.

(15.) The length of the perpendicular from the focal centre on one of the conjugate directrix planes $= \frac{b\,c}{a\,\varepsilon}$.

We now proceed to give the enunciations of a very few theorems, merely as specimens of the results which flow from the preceding definitions, and the application of the method alluded to above; premising that neither the preceding definitions, nor the following theorems, are applicable either to the hyperboloid of one sheet, or to the hyperbolic paraboloid; and this may suggest a natural division of surfaces of the second order into two classes, the one containing the umbilical surfaces, the other those surfaces whose generatrices are right lines.

Prop. I.—From any point τ of a surface of the second order, let perpendiculars p, p', be let fall on two conjugate directrix planes; the rectangle under those perpendiculars is to the square of r,—the distance of the point τ from the focal centre O, relative to those conjugate directrix planes in a constant ratio, as the square of the perpendicular P from the centre on one of the directrix planes is to the square of the transverse axe a, or

$$\frac{p\,p'}{r^2} = \frac{P^2}{a^2};$$

this constant ratio is one of equality, when the least semiaxis c of the surface is equal to the perpendicular let fall from the centre of the surface on the line joining the extremities of the semiaxes a and b: this ratio of equality can never exist then when the surface is one of revolution round the transverse axes, except when (Σ) is an elliptic paraboloid.

Generally when the ratio is one of equality, $\varepsilon^2 + \eta^2 = 1$.

When the surface is one of revolution round the transverse axe the conjugate directrix planes coalesce, p and p' therefore coincide, and are equal, and $P = \frac{a}{e}$: hence $\frac{p}{r} = \frac{1}{e}$; which is a fundamental property of surfaces of revolution and of the conic sections.

When the surface is an oblate spheroid the conjugate directrix planes become parallel to the plane of $x\,y$, and the focal centre coincides with the centre of the surface, the perpendiculars $p\,p'$ are in the same right line but on opposite sides of τ, hence in an oblate spheroid, if a right line is drawn perpendicular to the directrix planes meeting the surface in τ, and the directrix planes in m and m'; the rectangle $m\,\tau \times \tau\,m'$ is to the square of the semidameter O τ, as the square of the distance between the directrix planes is to the square of the diameter of the central circular section, or

$$\frac{m\,\tau \times \tau\,m'}{r^2} = \frac{\overline{m\,m'}^2}{4\,a^2}.$$

When the surface is an elliptic paraboloid, let l and l' be the semiparameters of the parabolas in the planes of $x\,y$ and $x\,z$, then

$$\frac{p\,p'}{r^2} = \frac{l'}{l}.$$

When the surface is a cone, let α and β denote its semiangles; $\alpha > \beta$; then

$$\frac{p\,p'}{r^2} = \frac{\sin^2\beta}{\tan^2\alpha},$$

or the square of the distance of any point on the surface of a cone from the vertex is to the rectangle under the perpendiculars from the same point on two planes passing through the vertex parallel to the circular sections of the cone in a constant ratio.

Prop. II.—The cone whose vertex is a focal centre of (Σ), and base any plane section of this surface, has its circular sections parallel to the planes passing through the vertex of the cone, and the right lines in which the base of the cone intersects the conjugate directrix planes.

When the base of the cone passes through the right line, in which the conjugate directrix planes intersect, the planes parallel to the circular sections coincide, and the cone is therefore a surface of revolution.

Hence the cone whose vertex is a focal centre of (Σ), and base any plane section of this surface, passing through the intersection of the conjugate directrix planes, is a surface of revolution, its axis being the line joining the conjugate foci of the surface.

When (Σ) is a surface of revolution round the transverse axe, the conjugate directrix planes coincide, therefore the planes through the vertex of the cone and the intersections of the directrix planes by its base, coincide, hence the cone is a right cone, and we obtain the known theorem, that "the cone whose vertex is the focus, and base any plane section of a surface of revolution, round the transverse axe, is a right cone."

Hence also the cone whose vertex is the centre of an oblate spheroid and base any plane section of this surface, has its circular sections parallel to the diametral planes passing through the right lines in which the base of the cone intersects the parallel directrix planes.

Prop. III.—The preceding theorem may be generalized thus: let a cone enveloping (Σ), having its vertex anywhere on the line QQ' joining the conjugate foci, be cut by any plane passing through the intersection of the conjugate directrix planes in a conic section, the cone whose base is this section and vertex the focal centre of the surface, is a surface of revolution.

Prop. IV.—Let a right line be drawn meeting a surface of the second order, in the points τ and τ', and the conjugate directrix planes in the points m and m', the segments of this right line, $m\tau$ and $m'\tau'$, subtend equal angles at the focal centre.

When the line is parallel to one of the directrix planes, it may be easily shown that the rectangle $m\tau \times m\tau' = \overline{mO}^2$, O being the focal centre, and m the point in which the right line meets the directrix plane to which it is *not* parallel.

Hence, if a tangent plane is drawn to a surface of the second order at the umbilicus, meeting the other conjugate directrix plane in a right line, the distances of any point in this right line from the umbilicus and focal centre are equal.

When the surface is one of revolution round the transverse axe the points m and m' coincide, or the line Om bisects the supplement of the angle $\tau O \tau'$, which is a known property of such surfaces.

Let a series of surfaces of the second order, having the same focal centre, and the same pair of conjugate directrix planes, be cut by any transversal; the segments of this right line, between each pair of surfaces, subtend equal angles at the common focal centre.

When (Σ) is a cone, if from m any point in one of the directrix planes a right line is drawn parallel to the other, meeting the cone in the points $\tau\tau'$, the rectangle $m\tau \times m'\tau$ = square of the distance of m from the vertex of the cone. Hence if a sphere is described through the circular base and vertex of a cone, the tangent plane to the sphere at the vertex of the cone is parallel to the second circular section of the cone.

Prop. V.—From the four foci of a surface of the second order, let fall perpendiculars on a tangent plane; multiplying together the perpendiculars from those foci which are situated on the same diameter and taking the sum, we have

$$\mathrm{P}p + \mathrm{P}'p' = 2\left\{b^2 \sin^2 \nu + \frac{c^2 u^2}{a^2} \cos^2 \nu\right\},$$ ν denoting the angle the perpendiculars make with the axis of Z.

When (Σ) is a surface of revolution round the transverse axe $\mathrm{P} = \mathrm{P}'$, $p = p'$, $b = c$ and $u = a$; hence $\mathrm{P}p = b^2$.

When the surface is an elliptic paraboloid, P and P′ are infinite, and we obtain

$$(p + p') \cos \lambda = l \sin^2 \nu + l' \cos^2 \nu.$$

λ being the angle the perpendicular makes with the axis of X.

Prop. VI.—Through any point of a surface of the second order, let two cords be drawn, passing through the extremities of the cord of the surface Q Q′, which joins a pair of conjugate foci, and meeting one of the conjugate directrix planes in the points m, m', these points m, m', subtend at the focal centre a right angle.

Prop. VII.—Let a plane quadrilateral be inscribed in a surface of the second order, whose sides, $\alpha, \beta, \gamma, \delta$, are produced to meet one of the directrix planes in the points A, B, C, D. The sum of the angles which the points A, B and C, D subtend at the focal centre is equal to two right angles.

Let two of the sides α, β of the quadrilateral be fixed, and let γ, δ be variable; then A and B are fixed, and therefore the angle A O B is constant; hence also the angle C O D is constant.

Def.—The right line in which two tangent planes intersect, and the line joining the points of contact, are called *conjugate polars* relative to the given surface.

Prop. VIII.—Two conjugate polars to a surface (Σ), meet one of the directrix planes in two points, which subtend a right angle at the focal centre.

When the conjugate polars become conjugate tangents, the proposition still holds, and when the conjugate tangents are tangents to the lines of curvature they are at right angles. Hence if a tangent plane be drawn to any point of a surface, meeting one of the directrix planes in a right line, and the tangents to the lines of curvature be produced to meet this line in the points m, m', the sphere described on $m\,m'$ as diameter will pass through the point of contact and the focal centre; hence may be given a new method of determining the lines of curvature.

Prop. IX.—Through the focal centre of a surface (Σ), let a right line and a plane be drawn perpendicular to each other; the line meeting the surface in a point τ, and the plane meeting one of the directrix planes in a right line $m\,m'$; the plane $m\,m'\,\tau$ envelopes a surface of revolution, whose focus is the focal centre of (Σ), and whose directrix plane passes through the intersection of the conjugate directrix planes of the given surface.

Let the equation of the surface, the origin being placed at the focal centre, be

$$\frac{x'^2}{a^2} + \frac{y'^2}{b^2} + \frac{z'^2}{c^2} - \frac{2\,e\,x'}{a} = \frac{b^2}{a^2} \text{ (a)};$$ $(x'\,y'\,z')$ being the coordinates of the point τ; let $\xi = \mu\,\zeta + \alpha$, (b) $\upsilon = \nu\,\zeta + \beta$, (b′) be the* tangential equations of the line $m\,m'$ in the directrix plane; and $x'\,\xi + y'\,\upsilon + z'\,\zeta = 1$ (c) the equation of the plane passing through the point $(x'\,y'\,z')$ and the right line $m\,m'$.

In the first place, as the line $m\,m'$ is in a plane passing through the origin, $\mu = \frac{x'}{z'}$, $\nu = \frac{y'}{z'}$; and equations (b) (b′) are changed into $\xi = \frac{x'}{z'}\,\zeta + \alpha$, $\upsilon = \frac{y'}{z'}\,\zeta + \beta$; and as the line $m\,m'$ is in the directrix place of which the tangential coordinates are

$$\xi = -\frac{a\,e}{b^2}\,,\quad \upsilon = 0,\quad \zeta = \frac{a\,\eta}{b\,c}\,;$$ these values of ξ, υ, ζ must satisfy the tangential equation of the right line $m\,m$; by these substitutions the equations (b) (b′) are changed into

$$\left.\begin{aligned} z'\,c\,(b^2\,\xi + ae) &= x'\,(b^2\,c\,\zeta - a\,b\,\eta) \\ z'\,c\,b^2\,\upsilon &= y'\,(b^2\,c\,\zeta - a\,b\,\eta) \end{aligned}\right\}\text{(d)}$$

and we have now to eliminate $x'\,y'\,z'$ between the four equations (a) (c) (d); from the three latter we get

$$x' = \frac{b^2\,c^2\,\xi - a\,c^2\,e}{b^2\,c^2\,(\xi^2 + \upsilon^2 + \zeta^2) + a\,c^2\,e\,\xi - a\,b\,c\,\eta\,\zeta}$$

$$y' = \frac{b^2\,c^2\,\upsilon}{b^2\,c^2\,(\xi^2 + \upsilon^2 + \zeta^2) + a\,c^2\,e\,\xi - a\,b\,c\,\eta\,\zeta}$$

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* See the treatise quoted above, page 11.

$$z' = \frac{b^2 c^2 \zeta - a b c \eta}{b^2 c^2 (\xi^2 + \upsilon^2 + \zeta^2) + a c^2 e \xi - a b c \eta \zeta}.$$

Putting those values of x' y' z' in (a), and making

$$V = b^2 c^2 (\xi^2 + \upsilon^2 + \zeta^2) + 2 a c^2 e \xi - a^2 \eta^2 - c^2,$$

we shall find for the resulting equation
$b^4 c^2 a \{ 2 b c \eta \zeta - c^2 e \xi - a e^2 \} V = 0$, which is satisfied by putting $V = 0$, or $b^2 c^2 (\xi^2 + \upsilon^2 + \zeta^2) + 2 a c^2 e \xi = a^2 \eta^2 + c^2$, the tangential equation of an ellipsoid of revolution whose focus coincides with the origin. It is easily shown that the tangential equation of the given surface referred to the same origin and axes is $b^2 (\xi^2 + \upsilon^2) + c^2 \zeta^2 + 2 a e \xi = 1$.

Hence if the given surface is a surface of revolution $\eta = 0$, and the locus found becomes identical with the given surface, as is otherwise known.

When the given surface is an oblate spheroid $e = 0$, $a = b$, and the locus becomes $c^2 (\xi^2 + \upsilon^2 + \zeta^2) = 1$, the tangential equation of a sphere described on the axis of revolution of the oblate spheroid as diameter.

Similarly may it be shown, that if through any fixed point in space, a right line and a plane are always drawn at right angles to each other; the former meeting a fixed plane in a point τ, and the second intersecting another fixed plane in a right line $m m'$; the plane $m m' \tau$ envelopes a surface of revolution of the second order, one of whose foci is at the fixed point.

In a future Number, after treating of the general and numerous kindred properties of the two surfaces of the second order, whose generatrices are right lines, the author proposes resuming this subject, and developing briefly a general method, by the theory of *reciprocal polars*, of demonstrating these and other similar theorems, many of which want of space has compelled him to omit in the present communication.

LXIV. *Address of the General Secretaries of the British Association*, Roderick Impey Murchison, *F.R.S.*, *F.G.S.*, *and Major* Edward Sabine, *V.P.R.S.*: *read at the Meeting at Glasgow, September* 1840.

In entering upon the duty assigned to us, we heartily congratulate our associates on this our second assembly in Scotland. As on our first visit we were sustained by the intellectual force of the metropolis of this kingdom, so now, by visiting the chief mart of Scottish commerce, and an ancient seat of learning, we hope to double the numbers of our northern auxiliaries.

Supported by a fresh accession of the property and intelligence of this land, we are now led on by a noble Marquis, who, disdaining not the fields we try to win, may be cited as the first Highland chieftain who, proclaiming that knowledge is power, is proud to place himself at the head of the clans of science.

If such be our chief, what is our chosen ground?—raised through the industry and genius of her sons, to a pinnacle of commercial grandeur, well can this city estimate her obligations to science! Happily as she is placed, and surrounded as she is by earth's fairest gifts, she feels how much her progress depends upon an acquaintance with the true structure of the rich deposits which form her subsoil; and great as they are, she clearly sees that her manufactures may at a moment take a new flight by new mechanical discoveries. For she it is, you all know, who nurtured the man whose genius has changed the tide of human interests, by calling into active energy a power which (as wielded by him), in abridging time and space, has doubled the value of human life, and has established for his memory a lasting claim on the gratitude of the civilized world. The names of Watt and Glasgow are united in imperishable records!

In such a city, then, surrounded by such recollections, encouraged by an illustrious and time-honoured university, and fostered by the ancient leaders of the people, may we not augur that this Meeting of the British Association shall rival the most useful of our previous assemblies, and exhibit undoubted proofs of the increasing prosperity of the British Association?

Not attempting an analysis of the general advance of science in the year that has passed since our meeting at Birmingham, we shall restrict ourselves, on the present occasion, to a brief review of what the British Association has directly effected in that interval of time, as recorded in the last published volume of our Transactions. From this straight path of our duty we shall only deviate in offering a few general remarks on subjects intimately connected with the well-being and dignity of our institution.

One of the most important—perhaps the most important service to science—which it is the peculiar duty of the Association to confer, is that which arises from its relation to the Government,—the right which it claims to make known the wants of science, and to demand for them that aid which it is beyond the power of any scientific body to bestow. In the fulfilment of this important and responsible duty, the Association has continued to act upon the principle already laid down in the Address of the General Secretaries at the meeting at Newcastle in 1838, namely, to seek the aid of Government in no case of doubtful or minor importance; and to seek it only when the resources of individuals, or of individual bodies, shall have proved unequal to the demand. The caution which it has observed in this respect has been eminently displayed in the part which it has taken with reference to the Antarctic expedition, and to the fixed magnetical observatories. It abstained from recommending the former to the Government until it had called for, and obtained from Major Sabine, by whom the importance of such an expedition was first urged, a report in which that importance was placed beyond all doubt; and it withheld from urging the latter, although its necessity was fully felt by some of its own members, until the letter of Baron Humboldt to the Duke of Sussex gave authority and force to its recommendation.

The delay which has in consequence occurred, has been productive of signal benefit to each branch of this great two-fold undertaking. Since the time alluded to, our views of the objects of investigation in terrestrial magnetism have been greatly enlarged, at the same time that they have become more distinct. Major Sabine's memoir on the Intensity of Terrestrial Magnetism has served to point out the most interesting portion of the surface of the globe, as respects the distribution of the magnetic force, and has indicated, in the clearest manner, what still remained for observation to perform; and the beautiful theory of M. Gauss, which has been partly built upon the data afforded by the same memoir,—while it has assigned the most probable configuration of the magnetic lines of declination, inclination, and intensity,—has done the same service with respect to all the three elements.

In another point of view, also, delay has proved of great value to both branches of the undertaking, but more especially to the fixed observatories. Our means of instrumental research have, since the time of their first projection, received great improvements, as well in their adequacy to the objects of inquiry, as in their precision; and finally, the two great lines of inquiry,—the research of the distribution of Terrestrial Magnetism on the earth's surface,—and the investigation of its variations, secular, periodic, and irregular,—have been permitted to proceed *pari passu*.

Last of all, the prudent caution, and vigilant care, which the two great scientific bodies have exhibited, both in the origin and progress of the undertaking, have naturally inspired the Government with confidence; and while on the one hand science has not hesitated to demand of the country all that was requisite to give completeness to a great design, so on the other, the Government of the country has not hesitated to yield, with a liberal and unsparing hand, every request the importance of which was so well guaranteed.

But while we thus enumerate the benefits which have resulted to magnetical science from the delay, it must be also acknowledged that something has been lost also, not to science, but to British glory. Although terrrestrial magnetism stood forward as the prominent object of the Antarctic expedition, yet it was also destined to advance our knowledge of the "*physique du globe*," in all its branches, and especially in that of geography. Had the project of an Antarctic expedition been acceded to when it was first proposed, viz. at the meeting of the British Association, in Dublin, in 1835, there can be no reasonable doubt, that a discovery, which by its extent may almost be designated a Southern Continent, situated in the very region to which its efforts were to have been chiefly directed, must have fallen to its lot; and the flag of England been once more the first to wave over an unknown land. But while, as Britons, we mourn over the loss of a prize which it well became Britain and British seamen to have made their own, it is our part too as Britons, as well as men of science, to hail the great discovery—one of the very few great geographical discoveries which remained unmade;—and to congratulate those by whom it has been achieved, those whom we are proud to acknowledge as fellow-labourers, and who have proved themselves in this instance our successful rivals in an honourable and generous emulation.

The caution which has characterized the British Association in the origination of this great undertaking, has been followed up by the Royal Society in the manner in which it has planned the details, and in the vigilant care with which it has watched over the execution. Of the success which has attended this portion of the work, the strongest proof has been already given in the unhesitating adoption of the same scheme of observation by many of the continental observers, and in the wide extension which it has already received in other quarters of the globe. All that yet remains is to provide for the speedy publication of the results. The enormous mass of observations which will be gathered in, in the course of three years, by the observatories established under British auspices, and by the Antarctic expedition, will render this part of the task of great expense and labour. To meet the former, we ain look to the Government, and to the East India

Company, who will certainly not fail to present the result of their munificence to the world in an accessible form. The latter can only be overcome by a well-organized system. The planning of this system, will, of course, be one of the first duties of the Royal Society; and it is important that it should be so arranged, that while every facility in the way of reduction may be given to those who shall hereafter engage in the theoretical discussion of the observations, care is taken at the same time that the data are presented entire, without mutilation or abridgement. The Council of the Royal Society, will, doubtless, be greatly assisted in this duty by the eminent individual who has had in every way so large a share in the formation of these widely scattered magnetic establishments, and whose own observatory, founded by the munificence of the Dublin University, has nearly completed a twelve months' magnetic observations on that enlarged and complete system of which it set the first example.

In referring, as we have done, to those most valuable services which the Royal Society have rendered, and are continuing to render, in directing and superintending the details of this great undertaking, in both its branches, it is right that, on the part of the British Association, we should express the cordial satisfaction and delight with which we have witnessed their exertions, united with our own in this common cause; nor should we omit to recognize how much this desirable concurrence has been promoted by the influence of the noble president of the Royal Society, the Marquis of Northampton, whom, as on so many former occasions, we have the pleasure of seeing amongst us, as one of our warmest supporters and most active members.

In the volume of our Transactions now under notice, is contained the memorial presented to Lord Melbourne by the Committee of the British Association, appointed to represent to Her Majesty's Government the recommendations of the Association on the subject of terrestrial magnetism. This memorial is one of many services which have been rendered to our cause by Sir John Herschel, whose name, whose influence, and whose exertions, since our meeting two years since at Newcastle, have largely contributed to place the subject where it now stands. The devoted labour of other of our members has long been given to an object which they have had deeply at heart, viz. the advancement of the science of terrestrial magnetism; but the sacrifice which Sir John Herschel has made of time, diverted from the great work, in which his ardent love of astronomy, his own personal fame, and his father's memory are all deeply concerned, the more urgently demands from our justice a grateful mention, because the science of magnetism had no claim on him, beyond the interest felt in every branch of science, by one to whom no part of its wide field is strange, and the regard which a national undertaking such as this deserved, from the person who occupies his distinguished station amongst the leaders of British science.

The advancement of human knowledge, which may be reckoned upon as the certain consequence of the Antarctic expedition (should Providence crown it with success), and of the arrangements connected with it, is of so extensive a nature, and of such incalculable importance, that no juster title to real and lasting glory than it may be expected to confer, has been earned by any country at any period of time; nothing has ever been attempted by England more worthy of the place which she occupies in the scale of nations. When much which now appears of magnitude in the eyes of politicians has passed into insignificance, the fruits of this undertaking will distinguish the age which gave it birth, and, engraved on the durable records of science, will for ever reflect honour on the scientific bodies which planned and promoted it, and on the Government which, with so much liberality, has carried it into effect.

Were the value of this Association, Gentlemen, to be measured only by the part which it has taken in suggesting and urging this one object, there might here be enough to satisfy the doubts of those who question its utility: to overlook such acts as these, and the power of public usefulness which they indicate, to scrutinize with microscopic view the minute defects incidental to every numerous assemblage of men, to watch with critical fastidiousness the taste of every word which might be uttered by individuals amongst us, instead of casting a master's eye over the work which has been done, and is doing, at our meetings, is no mark of superior discernment and comprehensive wisdom, but is evidence rather of a confinement to narrow views, and an indulgence of vain and ignoble passions.

But to proceed with our useful efforts,—one of the principal objects of our Annual Volumes, is the publication in the most authentic form of the results of special researches, undertaken by the request, and prosecuted in many instances at the cost, of the Association. It is a trite remark, that if a man of talent has but fair play, he will soon secure to himself his due place in public estimation. We fully admit the truth of this in many instances, and above all where the points of research are connected with commerce and the useful arts; but many also are the subtile threads of knowledge, which, destined at some future day to be woven into the great web in which all the sciences are knit together, are yet not appreciable to the vulgar eye, and if simply submitted to public judgement, would too often meet with silent neglect. Numberless, we say, are the subjects (and if your Association exceeds a centenary, still more numerous will they be) with which the retired and skilful man may wish to grapple, and still be deterred by his want of opportunity or of means. Then is it that, adopting the well-balanced recommendations of the men in whose capacity and rectitude you confide, you step forward with your aids, and bring about these recondite researches, the result of which in the volume under our notice, we now proceed to consider.

The first of these inquiries to which we advert, you called for at the hands of Professor Owen, upon "British Fossil Reptiles," one of the branches of Natural History, on a correct knowledge of which the development of geology is intimately dependent.

The merits of the author selected for this inquiry are now widely recognized, and he has, with justice, been approved as the worthy successor of John Hunter, that illustrious Scotchman who laid the foundation of comparative anatomy in the British isles. That this science is now taking a fresh spring, would, we are persuaded, be the opinion of Cuvier himself, could that eminent man view the progress which our young countryman is making towards the completion of the temple of which the French naturalist was the great architect. It is therefore a pleasing reflection, that when we solicited Professor Owen to work out this subject, we did not follow in the wake of Europe's praise, but led the way (as this Association ought always to do), in drawing forth the man of genius and of worth; and the value of our choice has been since stamped by the approval of the French Institute.

If Englishmen* first perceived something of the natural affinities of Palæosaurians, it was reserved for Cuvier to complete all such preliminary labour. The publication of his splendid chapters on the Osteology of the Crocodile and other Reptiles, drew new attention and more intelligent scrutiny to these remains; and it ought to be a subject of honest pride to us to reflect that the most interesting fruits of the researches of that great anatomist were early gathered by the English Palæontologists, Clift and Hume. One of our leaders, whose report on Geology ornaments the volumes of this Association, formed the genus *Plesiosaurus*, on an enlarged view of the relation subsisting between the ancient and modern forms of reptile life; while shortly after Buckland established the genus *Megalosaurus*, and Mantell, *Iguanodon* and *Hylæosaurus*, worthy rivals of the *Geo-Sauri* and *Moso-Sauri* of Cuvier. The other Englishmen who have best toiled in this field, are De la Beche, Hawkins, and Sir Philip Egerton.

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* Stukeley.

Yet although this report is on *British* reptiles, we are fully alive to the great progress which this department has made, and is making, on the Continent, through the labours of Count Münster, Jäger, and Hermann Von Meyer. The last-mentioned naturalist has been for some time preparing a series of exquisite drawings of very many forms unknown to us in England, most of which have been detected in the Muschelkalk, a formation not hitherto discovered in the British isles. Yet despite of all that had been accomplished in our own country or elsewhere, Professor Owen has thrown a new light of classification on this subject, founded on many newly discovered peculiarities of osseous structure, and has vastly augmented our acquaintance with new forms, by describing sixteen species of *Plesiosauri*, three of which only had been recognisably described by other writers; and ten species of Ichthyosauri, five of which are new to science. Such results were not to be obtained without much labour; and previous to drawing up his report, Professor Owen had visited the principal depositories of *Enaliosauri* described by foreign writers, as well as most of the public and private collections of Britain. This, the first part of Mr. Owen's report, concludes with a general review of the geological relations and extent of the strata through which he has traced the remains of British Enaliosauri. The materials which he has collected for the second and concluding portion of his report on the terrestrial and crocodilean Sauria, the Chelonia, Ophidian, and Batrachian reptiles, are equally numerous, and the results of these researches will be laid before the Association at our next meeting. Deeply impressed as we are with the value of this report, we cannot conclude a notice of it, without again alluding to its origin, in the words of Professor Owen himself. "I could not," says he, "have ventured to have proposed to myself the British Fossil Reptilia as a subject of continuous and systematic research, without the aid and encouragement which the British Association has liberally granted to me for that purpose."

Mr. Edward Forbes, whose labours in detecting the difference of species and varieties among the existing marine testacea of our shores, have been most praiseworthy, has on this occasion given us a report on the pulmoniferous mollusca of the British isles. The variations in the distribution of the species in this class of animals, are shown by him to depend both upon climate and upon soil, the structure of the country (or geological conditions) having quite as much share in such varied distribution, as the greatest diversity of temperature. The Association has to thank the author for valuable tables, which show both the distribution of the pulmoniferous mollusca in our islands, and their relations to those of Europe generally.

[To be continued.]

LXV. *Remarks on the Electricity of Steam. By Dr.* CHARLES SCHAFHAEUTL[*].

THE discovery of a large quantity of free electricity in a jet of steam[†] is decidedly of great interest, but the circumstances under which this electricity is developed, are still involved in such great mystery, that I cannot omit to call the early attention of the experimenter to some points which appear to me of primary importance.

Is the electricity in the jet of steam developed by the simple evaporation of water in the boiler, by the expansion of high-pressure steam in the air, or by the condensation of the steam, that is in its transition from the gaseous state to that in which it begins to become visible?

It would of course be easy to decide the first question by cementing a glass tube containing a metallic wire into the boiler, the inner portion of which being of course in contact with the steam in the chamber, and precautions being taken to prevent the escape of steam either into the cylinder or the open air.

If I recollect right, during the process of evaporation, the evaporated part has been generally found to be negative electric in respect to the remaining liquid; during condensation the reverse takes place. According to Mr. Armstrong's account, the electricity of the jet was positive, and seems therefore to correspond with the electricity developed by the process of the condensation of steam. If we ascribe the electricity contained in the jet of steam simply to the evaporation of the water in the boiler, the opposite electric state of the boiler seems difficult to be explained by the laws of common electricity, because there appears to me to be no reason why the steam in contact with the inside of the iron sheets of the boiler should not discharge its electricity the same as when it comes in contact with the outside, except the inside of the sheet iron, by a process of oxidation, becomes a non conductor of electricity in respect to the outside. An incrustation, according to Mr. Armstrong's account, was found in the boiler only as high as the water reached; but in boilers in which the water becomes very muddy, and which, therefore, are apt to prime, a sort of thin incrustation is often spread over the whole interior of the boiler as well as the safety-valve, and therefore the state of the interior of the steam-chamber ought to be very closely examined.

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[*] Communicated by the Author.

[†] See our last Number, pp. 370, 375.

The number of sparks obtained by Mr. Armstrong from the boiler at a distance of a quarter of an inch, amounted to between 60 and 70 per minute. If we assume the quantity of water necessary for a 28-horse power high-pressure engine to be 2·47 cubic feet per minute, supplied by two boilers; one boiler evaporated, therefore, 1·23 cubic feet per minute, and the evaporation of 35·5 cubic inches of water with two ounces of Newcastle coal would be necessary to produce one spark of a quarter of an inch length per second, a quantity of electricity which seems to bear no proportion with the small quantity of electricity produced during simple evaporation on a small scale.

But Mr. Armstrong's experiments seem distinctly to indicate that the electricity of the steam depends chiefly on its density, and the great quantity of free electricity may, therefore, perhaps, be made sensible by the rapid expansion of high-pressure steam, and may perhaps have some relation to the quantity of free caloric becoming latent during the expansion of high-pressure steam. I scarcely need here mention the observation of Mr. Hare, that the operation of his deflagrator was entirely suspended by the operation of the common galvanic trough apparatus; besides, all conductors of electricity during mutual friction develope caloric, whilst non-conductors of electricity, on the contrary, during mutual friction, develope, instead of caloric, electricity.

It seems to me a great question whether the electricity of the steam was not in close connexion with the deposit, or the induration of the deposit upon the plates of the boiler.

I have already shown in an article on steam-boiler explosions, published in the Mechanics' Magazine, that those incrustations were composed of a series of distinct layers, sometimes very easily separable, and which proves that the induration or crystallization of these layers, notwithstanding the continuous evaporation and feeding, must have been occasioned at certain intervals, and that one layer must have already been in an indurated state before the other was deposited. The layers assume a crystalline form only when they are in close contact with the iron plates.

I have also shown in the above-mentioned treatise, that during the deposition of certain salts held in solution by the boiling water the ebullition became interrupted, taking place only at intervals, and always with a sort of explosion or sudden development of steam, which often caused the glass flask to burst. During these sudden explosions the electricity of the escaping steam became so distinct, that it was readily indicated by a common gold-leaf electroscope; the electricity of the steam, on the contrary, escaping under ordinary circumstances being so feeble, that it cannot be detected without the aid of a condensator.

The development of electricity during the crystallization of certain salts is very well known, and many chemical deposits occur only under a certain pressure, to which the liquid containing them is subjected. Thus the carbonaceous deposits in common gas retorts are entirely obviated when the gas from the coals is evolved without pressure in the retorts, or even in a partial vacuum.

The columns of vapour and smoke arising from the craters of volcanos generally discharge flashes of lightning in all directions, and it is obvious that the discharged electricity is owing to the expansion or condensation of the escaping water gas, if not to a chemical separation in the column of smoke ascending from the crater with an immense force.

The electricity in thunder-clouds seems likewise to arise from condensation. I had once the good fortune to be immersed in a thunder-cloud hovering round the summit of Mount Brenner in the Tyrol, having with me at the time a barometer, thermometer, hygroscope, and an electroscope. I saw the clouds forming around me on the summit of the mountain into vaporous bodies of an irregular roundish shape, which seemed to retain their form by an attractive force arising from the centre of each individual cloud, as they had not the slightest tendency to amalgamate with each other. The hygroscope close to the cloud was not at all affected, and only when immersed in the cloud, it turned first a few degrees, indicating after a few minutes the highest degree of moisture, and sinking gradually back to its first point. This fluctuation continued as long as I had time to observe it. The electroscope was likewise not affected at all outside the cloud. Immersed in the cloud the gold leaves began gradually to separate, the barometer at the same time slightly rising, and after each discharge of lightning both instruments returned to their original state. From these observations it would appear that with every flash of lightning the cloud became exhausted of its electricity and recharged itself for each succeeding flash.

The air in the cloud seems to move from the periphery to the centre, of the nature of a whirlwind, fluctuating with the leaves of the electroscope, and I had sufficient time to witness twenty-one electric discharges from the cloud in which I was immersed, when the wind became so violent, that the instruments were broken, and I was obliged to cling to the stump of a tree to save myself from being blown over the precipice; but the uproar around me was increasing and fluctuating with the electric discharges from the clouds, and the rapid alternations of wet and dry in the clouds, was during the whole time in exact coincidence with the electric discharges.

LXVI. *On the Electricity of Effluent Steam.* *By* W. G. Armstrong, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

My letters to Professor Faraday on the remarkable development of electricity which has recently been discovered in a jet of steam issuing from a steam-engine boiler in this neighbourhood, having already appeared in your publication, it is, of course, unnecessary for me here to repeat the circumstances detailed in those letters. I shall therefore take up the narrative of my proceedings, relative to this curious subject, at the point at which the second of those letters concludes.

Having found electricity in all the three boilers I had examined in which water from the neighbouring colliery was used, and not having discovered any indications of it in the boiler which was supplied with rain-water, I was naturally led to believe that the effects I have described were attributable to the peculiar nature of the water from which the electrical steam was produced; and, under this impression, I lost no time in visiting some other high-pressure boilers in the same district, which were also supplied with colliery water, strongly impregnated with lime and other mineral matter. The steam from the safety-valves of these boilers also proved to be electrical, but not to such an extent as I had reason to anticipate from the similarity of the circumstances to those under which electricity was developed in such an extraordinary degree at Seghill. I then proceeded to try a number of boilers in this town and neighbourhood, in which steam was propagated under different pressures, and from water of various descriptions; and by insulating myself and holding a conducting-rod in the steam discharged from the safety-valves, I succeeded in every instance in obtaining electrical sparks, which varied in the different cases from about one-fourth to about half an inch in length.

In company with Mr. Robert Nicholson, the engineer of the Newcastle and North Shields railway, I next tried the boilers of the locomotive engines used on that railway, and finding electricity in great abundance in the ejected steam from these boilers, I determined, with Mr. Nicholson's permission and assistance, to institute a set of experiments upon one of them, with a view to a fuller investigation of the subject.

I shall now briefly describe such of these experiments as have been the most marked in their results, and shall divide them into two classes, first taking those which were chiefly intended to exhibit the extent to which electricity existed in the issuing steam, and then proceeding with the experiments which were undertaken to ascertain the cause of the electric development. Nearly all the experiments were made at night, under cover of the engine-shed, and the atmosphere was generally humid; but when it happened to be otherwise, the quantity of electricity derived from the jet was greatly increased.

Upon trying the steam in the first instance by the method adopted in the previous cases, that is to say, by standing on an insulated stool and holding with one hand a light iron rod immediately above the safety-valve, while the steam was freely escaping, and then advancing the other hand towards any conducting body, sparks of about an inch in length were obtained: but it was soon observed, that by elevating the rod in the steam the electricity was gradually increased, and that the maximum effect was not attained until the end of the rod was raised five or six feet above the valve, at which point the length of the sparks occasionally reached two inches. Small sparks were even obtained when the rod was wholly removed from the steam and held in the atmosphere at the distance of two or three feet from the jet, and the electricity thus drawn from the air was positive, like that of the steam. When the rod was extended into the cloud of vapour which accumulated in the upper part of the shed, electricity was drawn down as by a lightning-conductor from a thunder-cloud. I endeavoured to ascertain whether any precipitation of moisture, analogous to the formation of rain, accompanied the abstraction of electricity from the steam, and a sprinkling of wet was undoubtedly felt on the face and hands by the person holding the rod, so long as he remained insulated, but the effect ceased as soon as the insulation was destroyed.

After fully trying the steam with a simple iron rod, as a conductor, recourse was had to other conductors which presented a larger surface to the steam, but the effect was not materially increased until a bunch of pointed wires of different lengths was attached to an iron rod and held in the issuing steam, with the points presented downwards. The iron rod terminated in a round knob at the end next the hand, and from this knob sparks of the *measured length of four inches* were actually drawn, almost as rapidly as they could be counted, while a stream of electricity was at the same time passing off from the rod, at the part which most nearly approached the chimney of the engine. Very perceptible sparks were also obtained when the points were held in a clear atmosphere, at the distance of at least eight feet from the nearest part of the jet.

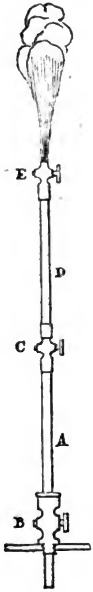
In all the preceding experiments, the effect appeared to be proportionate to the quantity of steam discharged from the valve, when other things remained the same; and the electricity became quite imperceptible when the escape was very inconsiderable.

By abruptly raising the valve when the engine-shed was dark, the edges of the lever and margin of the brass cup which surrounded the valve, were rendered distinctly luminous with rays of positive electricity which were strongest the instant the valve was lifted, and then quickly subsided, becoming very faint after the lapse of a second.

In proceeding to investigate the cause of this extraordinary development of electricity, the first question which I proposed for inquiry was, Where does the steam first become electrical, that is to say, is it electrical in the boiler, or if not, does it become so in passing through the orifice, or not till it escapes into the air? In order to determine which of these three suppositions was correct, the apparatus represented in the annexed figure, and of which the following is a description, was employed.

A is a glass tube passing into the steam chamber through the cock B, which was screwed into a hole in the top of the boiler, and was furnished with a stuffing-box to prevent escape between the outside of the tube and inner surface of the cock; C is a stop-cock affixed to the upper end of the glass tube, and upon which cock is screwed a second glass tube D terminating in another stop-cock E.

The application of this apparatus will be easily understood. If the steam were in the same state of electricity in the boiler as when it issued into the air, it would necessarily communicate positive electricity to the insulated cock C, in passing through the tube. Or, if the steam acquired its electricity by friction, or otherwise, in the channel through which it was discharged, it could only, in the present instance, do so *at the expense* of the cock C, which, being insulated, would in that case indicate negative electricity. Or, lastly, if the electricity were developed by condensation, expansion, or any other cause which came into operation after the steam escaped into the air, then the cock C would have neither positive nor negative electricity.



Previously to inserting the lower glass tube in the boiler, the steam was allowed to blow off through the large cock B, and the jet which issued from it proved, to the surprise of every one present, almost destitute of electricity. This result completely vitiated the inference I had drawn from the circumstance of not finding electricity in the steam from the rain-water boiler before alluded to, in which case, as I have already stated in my second letter to Professor Faraday, the jet was obtained from the gauge cock.

The lower glass tube, without the upper one attached to it, was then passed into the boiler, and a highly electrical jet was obtained from it, which communicated positive electricity to the stop-cock C, from which the steam was discharged. The upper tube was accidentally broken in screwing it on to the lower one, leaving only about three inches of glass above the cock C. Under these circumstances the cock C still continued highly charged with positive electricity, and a pale lambent light flashed at short intervals down the inside of the tube from the cock towards the boiler.

Having replaced the broken glass tube with a new one, the experiment was tried again on a subsequent evening, and the jet being now removed to a much greater distance than before from the cock C, no electricity whatever could be detected in that cock, while the one above it indicated positive electricity in a very high degree. It therefore became pretty evident that the electricity was not developed until the steam issued into the atmosphere, and that the upper stop-cock derived its electricity from its contiguity to the jet. One circumstance alone seemed in some degree to militate against this supposition, namely, that the electricity of the cock E was greatly increased when the cock C was partially closed, as if the expansion which in that case took place in the upper tube rendered the steam electrical previously to its reaching the cock from which the jet was discharged. No negative electricity, however, could be discerned in any part of the apparatus, and without a development of negative electricity, I cannot see how positive electricity can possibly arise from expansion. The more probable explanation of the effect appeared to be, that the partial closing of the middle cock shortened the transparent or non-conducting part of the jet, and thereby caused the electricity to be more readily communicated from the opake part of the jet.

In consequence, no doubt, of increased accumulation of electricity which was thus occasioned in the highest cock, together with the unavoidable dampness of the surrounding medium, the upper glass tube, and the cock above it, became illuminated in the most singular and beautiful manner. Flashes of wavering light flickered round the exterior surface of the glass, and darted from it to the distance of three or four inches, while strong rays of electrical light streamed from the angular parts of the cock, and the flashes from the glass were accompanied by a snapping noise which was distinctly audible amidst the hissing of the steam when the ear was advanced within a short distance from the tube.

The upper glass tube was then removed, and as an additional test of the non-existence of free electricity in the interior of the boiler, a pointed wire was thrust down through the cock C and tube A into the steam, and effectual means were used to prevent the escape which would otherwise take place at the cock C, in consequence of the tap remaining open to admit the wire. Now this wire being insulated by the glass tube and communicating with the insulated cock C, must have rendered that cock electrical, if the steam were electrical in the boiler; but not the slightest indication of electricity could, under these circumstances, be found in the cock.

Having withdrawn the pointed wire from the tube, another glass tube, of which the sectional area was about ten times greater than that of the one inserted in the boiler, was then attached to the cock C, in the same manner as the tube D had been before. The comparatively large bore of this tube allowed the steam to expand in a very great degree before it issued into the air, and caused it to be discharged in the state of low-pressure steam; but no diminution of electricity could be perceived in the jet, when thus attenuated; so that the electrical development does not appear to depend upon the degree of violence with which the steam comes in contact with the atmosphere.

The entire absence of negative electricity seemed to preclude the possibility of the phænomena arising from expansion, and the only remaining supposition appeared to be, that the condensation which took place in the jet, set free the electricity which the steam had absorbed in the process of evaporation. This supposition had been previously rendered probable, when it was discovered that the upper and most opake part of the jet yielded the most electricity, although I was at first inclined to attribute that circumstance to the dampness of the steam, in that part of the jet, rendering it a better conductor, and causing it to part more readily with its electricity. Experiments were next, therefore, commenced to ascertain the effect of insulating the boiler, and wholly condensing the steam; but these require repetition before they can be much relied upon. The great difficulty is to effect insulation amidst so much moisture, but I have no doubt that with a little perseverance this object will be accomplished, and I trust I shall be able to furnish, in time for insertion in the next Number of the Philosophical Magazine, such further results as will set the question at rest.

I am, yours, &c.

Newcastle upon-Tyne, Nov. 18, 1840. WM. GEO. ARMSTRONG.

LXVII. *Further Experiments on the Electricity of Steam.*
By H. L. PATTINSON, *Esq., F.G.S.*

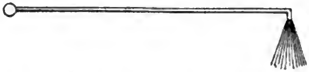
To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SINCE my last letter to you, dated the 19th ult. (published at p. 375 of this volume), relative to the electricity of steam issuing from two boilers at Cramlington Colliery, the subject has been further pursued both by myself and others, and sparks have been obtained from the steam of various boilers, in every direction. The mode of operating has generally been that described in my letter, viz. suffering the steam to escape from the safety-valve of the boiler tried, and testing its electricity by holding in it a shovel or an iron rod, the operator standing upon an insulating stool. Sometimes the indications have been very slight, and sometimes there has hardly been any appearance of electricity in the steam; but in such cases the trials have been generally made under unfavourable circumstances, and from all that has yet been done, the presumption is certainly that steam is always more or less electrical. It is not, however, always electrical to the same extent under the same pressure, as I shall presently show.

Mr. Armstrong was the first to experiment with a locomotive engine-boiler (one used on the Newcastle and North Shields railway), from which he obtained very striking results. The directors of the Newcastle and Carlisle railway, through their secretary, Mr. Adamson, gave me permission to experiment upon the boilers of the locomotive engines on that line, and I now beg to lay before you the results I have obtained. In preparing for, and performing these experiments, I have, as before, been assisted and accompanied by Mr. Henry Smith, and I have received the most willing and efficient aid from Mr. Anthony Hall of Blagdon, the mechanical engineer on the railway.

1. A copper rod, half an inch in diameter, and five feet long, was provided, made hollow for lightness; this was terminated at one end by a two-inch ball, and at the other (which was bent at a right angle) by ten or twelve sharp-pointed wires, spread out in every direction to collect the electricity more perfectly from the steam.



2. The Wellington locomotive engine, immediately after coming to the station with passengers, was first tried. At this time the steam was blowing forcibly out of the safety-valve, at a pressure of fifty-two pounds per inch. On holding the pointed conductor in this current of steam, with its points downwards, the individual holding it standing at the time on an insulating stool, sparks three to four inches long were given off from his person to the boiler. The sparks were largest when the valve was held down a minute or two and then suddenly lifted, so as to suffer a large volume of steam to escape with great rapidity. By this management the sparks were frequently four inches long, and occasioned considerable pain to the person on the stool, even when given from a brass ball held in his hand. The sparks were largest when the points of the conductor were held in the steam about two feet above the valve; but larger sparks were ob-

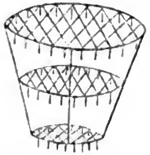
tained when it was held much higher; and indeed sparks were obtained by holding the conductor entirely out of the cloud of steam, and at a distance from it, for the air in the wooden shed in which we operated became speedily electrical throughout. The electricity was positive.

3. The steam in the boiler was now gradually run down to see how the electrical condition would vary with the pressure. At forty pounds per inch the sparks became much less, the largest not reaching three inches. At thirty pounds the largest spark did not reach two inches; at twenty pounds it became barely an inch; at ten pounds not more than from one-fourth to one-half of an inch; and at five pounds per inch pressure the spark was hardly perceptible. But if at any pressure the valve was held down a few minutes so as to suffer the steam to accumulate and then suddenly opened, there was always a great increase, for an instant, of the electrical effects.

4. Another boiler, that of the Lightning engine, which had also just come in from a trip, and had its steam blowing off forcibly, at a pressure of fifty pounds per inch, was now tried in exactly the same way as the Wellington. On holding the pointed conductor in the steam, whether regularly blowing off at the valve or escaping with great rapidity from the sudden lifting of the valve, it did not yield a spark more than one-fourth of an inch long. We then blew a quantity of water out of the boiler of the Lightning until it barely covered the tubes inside, and on afterwards testing its steam blowing off at fifty pounds per inch, the spark was found increased to nearly two inches in length. The steam of the Lightning was, however, much less electrical than the steam of the Wellington at the same pressure, under all the circumstances of our experiments.

5. The strong current of steam and water issuing from the boiler of the Lightning when the water was blown out of it as just stated, was tested for electricity, but no indications could be perceived whatever.

6. A very large conductor had been provided, made of zinc two-inch tubing, in this way,—three rings were made of this tubing, respectively three feet, two feet, and one foot diameter. These rings were attached to each other a foot and a half apart by side pieces, so as to form a hollow frustum of a cone, three feet high, with ends three feet and one foot diameter respectively. The inside of this cone was laced across with copper wire, and the whole bristled with pointed wires in every direction. By means of a long



iron bar, placed upright in a cask of rosin (both to insulate it and to serve as a foot), and a horizontal arm projecting from it, made to slide up and down on the vertical bar, the large conductor could be placed in any part of the cloud of steam issuing from the valve, and the electricity given off could be conveyed from it in any direction. Care was taken to round off all parts of this conductor, so as to avoid sharp points and angles as much as possible. On trying this large conductor in the current of steam from the Wellington, we were disappointed to find that it did not yield a longer spark than the small pointed copper rod with which we had previously experimented. The spark was larger in volume, but it did not possess greater intensity. It never struck through more than three inches of space, but its effect upon the person when taken was very violent and painful. Our intention was to have ascertained the rate at which large jars could be charged from the steam, in order to form some idea of the quantity of electricity given off; but the evening had become very damp, and the air was so moist, that we could not procure sufficient insulation, and were obliged to relinquish the attempt.

7. When the large conductor was held in the cloud of steam with its lower part or apex about two feet above the valve, it gave off numerous and powerful sparks; but if at this time the points of the small conductor were placed by a person connected with the ground in the steam below the large conductor a foot above the valve, the electricity given off by the large conductor was very materially diminished.

8. By means of screws, the entire engine (the Wellington) was raised off the rails and placed upon blocks of baked wood, so as to insulate it entirely. The steam being now blown off at the valve, the boiler and engine became strongly electrical with negative electricity; points placed upon any part of the engine exhibiting the peculiar star of the negative element, and threads suspended from the engine being repelled by excited sealing-wax. The steam was at the same time strongly positive, and when a point connected with the conductor held in the steam was brought near a point attached to the insulated boiler, the pencil upon the former and star upon the latter were beautifully decisive as to the electrical states of each.

9. I repeated Volta's experiment by placing a hot cinder upon the cap of a gold-leaf electrometer, and projecting a few drops of water upon it, when the leaves diverged strongly with negative electricity. I observed, that when the cinder was very hot, and the production of the steam consequently very rapid, the electricity given out was always most powerful.

10. I then insulated an iron pan, twelve inches diameter and two inches deep, and attached to it a pith-ball electrometer, with balls three-eighths of an inch diameter, and threads five inches long, and also attached to the pan a metallic wire, the pointed extremity of which was placed about one-twentieth of an inch distant from the point of another wire connected with the ground. The iron pan was then filled with cinders, very hot, from a wind-furnace, and on projecting upon them a few ounces of water, steam was evolved with great rapidity, and at the same moment the pith balls diverged to the distance of an inch, and sparks passed between the metallic wires. This was several times repeated.

These experiments enable us, I conceive, to give a clear explanation of the electrical phænomena presented by steam. There is no doubt whatever, as Dr. Faraday conjectures in his note to Mr. Armstrong's paper in your last Number, "that this evolution of electricity by vaporization is the same as that already known to philosophers on a much smaller scale." The electricity appears to originate at the instant of vaporization, and the steam as it collects within the boiler is electrified with positive electricity, the water and metallic boiler being at the same time negative. In this condition the electricity of both is latent, like the electricity of the two plates of an excited electrophorus; but the instant steam is suffered to escape, its positive electricity, being carried off along with it, and out of the influence of the equivalent quantity of negative electricity in the boiler, becomes free, and hence the steam is electrical with positive electricity. The same thing takes place with the boiler, in which negative electricity is set at liberty as the steam escapes, and which becomes evident on insulating the boiler.

When steam much mixed with water, or what engine-men call "wet steam," escapes from a boiler, it evidently cannot be very highly electrical, for the negative water will tend to neutralize the positive steam, and this may perhaps in some measure account for the increased effect in the Lightning on lowering the water within its boiler, and for the increase of intensity in every boiler, observed when the valve has been forcibly held down and is suddenly opened; but it does not seem sufficient to account *entirely* for these variations of intensity, nor for the difference of intensity in different boilers at the same pressure. It is therefore probable that chemical action between the metal of the boiler and the water has something to do with exalting the electrical condition of the steam at the moment it is generated; but this part of the subject certainly requires further investigation. By far the most powerful effects up to this time have been obtained from locomotive engines, in which water is heated in contact with brass tubes. How far this may influence the production of electricity, further experiments must determine. It is certainly somewhat curious to consider the splendid locomotive engines we see daily in the light of enormous electrical machines; but this they undoubtedly are; the steam is analogous to the glass plate of an ordinary machine, the boiler to the rubbers; and a conductor properly exposed to the escaping steam gives out torrents of electricity.

I am, Gentlemen,
Your obedient Servant,
H. L. Pattinson.

Bentham-Grove, Gateshead,
November 21, 1840.

LXVIII. *On the Motion of a small Sphere vibrating in a resisting Medium. By the Rev.* J. Challis, *Plumian Professor of Astronomy in the University of Cambridge*[*].

In the London and Edinburgh Philosophical Magazine for September, 1833 (vol. iii. p. 186.), I have given a solution of the problem of the resistance to the motion of a ball-pendulum vibrating in the air, by making use of the principle of the conservation of *vis viva*, and assuming that for slow vibrations the motion of the air surrounding the ball is the same as if the fluid were incompressible. I have given another solution in the Cambridge Philosophical Transactions (vol. v. part ii. p. 200.), by adopting the above assumption without using the principle of the conservation of *vis viva*; and in the latter solution it is not taken for granted, as in the other, that the same considerations apply to fluid motion directed to or from a moving centre, as to motion to or from a fixed centre. The two methods lead to the same result. In 1835, M. Plana published at Turin a Memoir (for a copy of which I am indebted to the kindness of the author) containing a solution of the problem in question, the same in principle as that of Poisson in vol. xi. of the *Mémoires* of the Paris Academy of Sciences[†], with the difference of treating separately the motions in a compressible and an incompressible fluid, and so obviating some objections to which Poisson's reasoning appeared liable. M. Plana adverts to my communication in the Philosophical Magazine, and subjoins a translation of it, but is unwilling to admit the correctness of the principle of the method I have employed, apparently for no other reason than that it leads to a result differing from his own.

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[*] Communicated by the Author.

[†] Poisson's memoir is also inserted in the *Connaissance des Tems* for 1834.

The two methods are, in fact, so dissimilar in principle, and in their results, that if one is right the other must be wrong. But after the lapse of some years I am not able to discover any error either in the principle or the details of the method I have employed in the Philosophical Magazine, nor of that in the Cambridge Philosophical Transactions. The object of my present communication is to give a *third* solution, which applies expressly to vibrations of the ball in a *compressible* fluid.

It will be proper to begin with proving generally that the same equations apply to the motion of the fluid when directed to or from a moving centre, as when directed to or from a fixed centre.

Considering, first, the motion of the fluid to be in the direction of radii from a fixed centre, conceive two spherical surfaces described about this centre at the distances r and r' differing very little from each other; and let the interior one pass through the point at which we consider the motion. Conceive also a conical surface, having its vertex at the centre of the spherical surfaces and its vertical angle indefinitely small, to intersect with its axis the interior spherical surface at that point. Let $m^2 =$ the small portion of the interior spherical surface included by the conical surface; then $\frac{m^2 r'^2}{r^2} =$ the corresponding portion of the outer surface. It will be assumed that during a very small time δt, the velocity and density of the fluid which passes the area m^2 are uniformly v and ρ; and, similarly, that the velocity and density of the fluid passing in the same time the corresponding area of the other surface are uniformly v' and ρ'. Then the quantity of fluid which passes m^2 in the time δt is $m^2 \rho v \delta t$; and that which passes the other area in the same time, $\frac{m^2 r'^2}{r^2} \cdot \rho' v' \delta t$. The increment of matter in the included space is, therefore, $-m^2 \delta t \left(\frac{r'^2 \rho' v'}{v^2} - \rho v\right)$, the velocities v and v' being reckoned positive when directed *from* the centre. The space itself is ultimately $m^2 (r' - r)$. Hence the increment of density $\delta \rho$ is equal to $-\frac{m^2 \delta t \, (r'^2 \rho' v' - r^2 \rho v)}{m^2 (r' - r)}$.

Consequently,

$$\frac{\delta \rho}{\delta t} + \frac{r'^2 \rho' v' - r^2 \rho v}{r^2 (r' - r)} = 0;$$

and passing from differences to differentials,

$$\frac{d\rho}{dt} + \frac{d \,.\, r^2 \rho v}{r^2 dr} = 0 \,; \quad . \quad . \quad . \quad . \quad . \qquad (1.)$$

where, from the nature of the investigation, the differential coefficients are evidently partial.

Now suppose the motion of the fluid to be directed to or from a moving centre, and let two spherical surfaces separated by a very small interval be described about this centre, the interior one always passing through the point of space at which we consider the motion. On account, therefore, of the motion of the centre, the spherical surfaces will not be stationary. We may, however, conceive a conical surface, described as in the former case, to have its axis always passing through the moving centre and the point of space at which the motion is considered, and to include a given small portion m^2 of the interior spherical surface. The velocity and density of the fluid passing the area m^2 may, as before, be considered uniform during a very small time δt; as may also, without entailing error, the velocity and density of the fluid passing the portion of the outer surface always included by the conical surface. Hence, using the same letters as in the case of a fixed centre, the quantity of fluid which passes m^2 in the time δt is $m^2 \rho v \delta t$. We have now to ascertain the quantity of fluid which in the same time passes the corresponding area of the exterior surface. Let r and r' be the radii of the two concentric surfaces at the beginning of the interval δt, and let α be the velocity of the centre resolved in the direction of r. Then after an interval τ, less than δt, the radii of the surfaces are $r \pm \alpha\tau$ and $r' \pm \alpha\tau$ ultimately. Hence the area of the outer surface corresponding to $m^2 = m^2 . \left(\frac{r \pm \alpha\tau}{r \pm \alpha\tau}\right)^2$

$$= \frac{m^2 r'^2}{r^2} . \left(\frac{1 \pm \frac{\alpha\tau}{r'}}{1 \pm \frac{\alpha\tau}{r}}\right)^2 = \frac{m^2 r'^2}{r^2},$$ by neglecting terms that may be neglected, since by hypothesis r' differs very little from r, and $\alpha\tau$ is very small. This result is independent of τ, and is the same as if the centre had been fixed. The rest of the reasoning would consequently conduct to the equation (1.). Hence from this equation combined with the known equations p (the pressure) $= a^2 \rho$, and $\frac{dp}{\rho dr} + \left(\frac{dv}{dt}\right) = 0$, equations applicable to motion directed to or from either a

fixed or a moving centre may be deduced. I will not stop to make the deduction, which presents no difficulty, but at once employ the equations given in the Treatises on Hydrodynamics for motion propagated *from* a *fixed* centre. (Propagation *towards* the centre is excluded by the nature of the question.) These equations are (putting $1+s$ for ρ),

$v = \frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}$ (2.), and $as = \frac{f'(r-at)}{r}$ (3.), which as they contain arbitrary functions, apply immediately to the arbitrary disturbance given to the fluid. In the problem before us they apply, therefore, to the motion given to the fluid by the vibrating sphere *at its surface*. For as the sphere is supposed to be perfectly smooth and consequently to impress motion only in a direction normal to its surface, the motion at the surface is plainly directed to or from a moving centre. The arbitrary condition of the motion is that at a given distance (r), equal to the radius of the sphere from the centre regarded as fixed, and at a given point of the surface of the sphere, the velocity impressed follows either exactly or very approximately the law of a vibrating pendulum. Let the velocity of the centre of the sphere at any time t be $V \sin bt$. Then for any point the radius to which makes an angle θ with the direction of the motion, we shall have the normal velocity v equal to $V \cos\theta \sin bt$. Hence, putting for brevity $u = f(r-at)$, and substituting in the equation (2.), it will be found that $\frac{du}{dt} + \frac{a}{r}u + Var\cos\theta\sin bt = 0$,

an equation in which u and t are the only variables, and which is true whatever be t. The integral of this equation is

$$u = Ce^{-\frac{at}{r}} - Vr^2 \cos\theta \cos\phi \sin(bt-\phi),$$

$\tan\phi$ being put for $\frac{br}{a}$. The term involving C will be insensible for all but very small values of t, on account of the factor $e^{-\frac{at}{r}}$, and may therefore be omitted. Hence, by differentiating and putting $a\tan\phi$ for br,

$$\frac{du}{dt} = -Var\cos\theta\sin\phi\cos(bt-\phi).$$

Now the pressure at the point of the sphere we are considering is equal to $a^2 s$, or by equation (3.) $a \cdot \frac{f'(r-at)}{r}$, or $-\frac{1}{r}\cdot\frac{du}{dt}$. Hence this pressure is $Va\cos\theta\sin\phi\cos(bt-\phi)$.

And by integrating in the usual way to obtain the pressure on the whole sphere, it will be found to be $\frac{4\pi V a r^2}{3} \cdot \sin\phi \cos(bt-\phi)$. This is reckoned positive in the direction contrary to that of the motion of the sphere. Hence if $\sigma =$ the ratio of the specific gravity of the fluid to that of the sphere, the accelerative force of the resistance in the positive direction of the motion is $-\frac{V a \sigma}{r} \cdot \sin\phi \cos(bt-\phi)$. If $\lambda =$ the distance to which motion is propagated in the fluid in the time of one vibration of the sphere, $b = \frac{2\pi a}{\lambda}$, and consequently, $\tan\phi = \frac{2\pi r}{\lambda}$ This is an exceedingly small quantity. Hence very approximately $\sin\phi = \frac{2\pi r}{\lambda} = \frac{b r}{a}$, and the accelerative force of resistance $= -V b \sigma \cos bt$. Again, if $x =$ the distance of the centre of the sphere at the time t from the mean place about which it is oscillating, $\frac{dx}{dt} = V \sin bt$, and $\frac{d^2x}{dt^2} = V b \cos bt$. Hence the accelerative force of the resistance $= -\sigma \cdot \frac{d^2x}{dt^2}$. The length of the pendulum being l and the force of gravity g, the accelerative force of gravity, taking account of the buoyancy of the fluid, is $-\frac{gx}{l}(1-\sigma)$. Hence,

$$\frac{d^2x}{dt^2} = -\frac{gx}{l}(1-\sigma) - \sigma \cdot \frac{d^2x}{dt^2},$$

and consequently

$$\frac{d^2x}{dt^2} = -\frac{gx}{l} \cdot \left(\frac{1-\sigma}{1+\sigma}\right).$$

This is the result I obtained by my two former methods. As it does not contain a, it is applicable to any resisting medium, supposing the vibrations to be slow. Putting the factor in brackets, under the form $1 - n\sigma$, we shall have $n = \frac{2}{1+\sigma}$. For a brass ball of specific gravity 8, vibrating in air, $n = 2$ very nearly; and for the same vibrating in water,

$n = 1\cdot78$. The experiments of Bessel give for these two cases, 1·95 and 1·63.

I do not consider the above solution of value for the numerical results to which it leads, so much as because it serves to establish the principles to be adopted in the treatment of another problem (perhaps the most important that could be proposed in the present state of physical science), the solution of which has hitherto been unattempted, viz. *if a minute spherical atom were subject to the mechanical action of the vibrations of a very elastic medium, like those which take place in air, would it, in addition to a vibratory motion, receive also a permanent motion of translation?* I propose at a future opportunity to state my reasons for considering this an important question, and to advance some ideas respecting the method in which I conceive it may be answered.

Cambridge Observatory, Nov. 16, 1840.

LXIX. *On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, *Esq., Treas. R.S., F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from p. 280.]

On the Conditions of the Atmosphere, and on the Calculation of Heights by the Barometer. (Resumed.)

As the expression which has served to calculate the temperatures evidently represents the state of the atmosphere far within the limits of the applicability of this or any other formula founded upon a state of repose to an atmosphere continually agitated by currents, it must of course serve to eliminate the density and to obtain an expression for the height in terms of the pressures and temperatures at the extremities of any atmospheric column.

If z be the altitude of the place above any fixed point, a the distance of the fixed point from the centre of the earth, g the force of gravity,

$$\frac{\mathrm{d}\,p}{\varrho} = -\frac{g\,a^2}{(a+z)^2}\,\mathrm{d}\,z,$$

and putting the expression for ϱ', at vol. xvi. p. 440,

$$\frac{k\,(1+\alpha\,\theta)\,(p^{\beta}-E)\,\mathrm{d}\,p'}{p'\,(p'^{\beta}-E)} = -\frac{g\,a^2\,\mathrm{d}\,z'}{(a+z')^2}.$$

This expression can be integrated, and I find, supposing $z = 0$, after a proper determination of the constants,

$$\frac{z'}{1 + \frac{z'}{a}} =$$

$$\frac{k\,(1 + \alpha\,\theta)}{g}\,\frac{\left\{p^{\beta} - p'^{\beta}\right\}\left\{\frac{1}{\alpha} + \theta'\right\}}{\beta\left\{p'^{\beta}\left(\frac{1}{\alpha} + \theta'\right) - p^{\beta}\left(\frac{1}{\alpha} + \theta\right)\right\}}\ \text{Nap. log}\left\{\frac{\left(\frac{1}{\alpha} + \theta'\right)}{\left(\frac{1}{\alpha} + \theta\right)}\left(\frac{p'}{p}\right)^{\beta}\right\}$$

If the variation of the force of gravity be neglected, the pressures p, p' may be represented by the heights of the barometer h, h'. If M be the *modulus* or the quantity by which Naperian logarithms must be multiplied to give common logarithms, Laplace makes

$$\frac{k}{g\,M} = 18337^{\text{m}}{\cdot}46. \qquad \log M = 9{\cdot}6377843.$$

In order to give an example of the use of this expression, I take the 21st observation of Gay Lussac,

$$h = {\cdot}76568 \qquad \theta = 30{\cdot}75$$

$$h' = {\cdot}3339 \qquad \theta' = -\,7{\cdot}00$$

log 18337·46	= 4·2633392
$\log\,(1 + \alpha\,\theta)$	= 0·0474015
$\log\left\{\dfrac{\left(h^{\beta} - h'^{\beta}\right)\left(\frac{1}{\alpha} + \theta'\right)}{h'^{\beta}\left(\frac{1}{\alpha} + \theta'\right) - h^{\beta}\left(\frac{1}{\alpha} + \theta\right)}\right\}$	= 0·2696699
$\log\left\{\log\left\{\dfrac{\left(\frac{1}{\alpha} + \theta'\right)}{\left(\frac{1}{\alpha} + \theta\right)}\left(\frac{h'}{h}\right)^{\beta}\right\}\right\}$	= 8·7762776
	3·3566882
$\log \beta$	9·5176049
	3·8390833

$$\frac{z'}{1 + \frac{z'}{a}} = 6903{\cdot}7$$

log a = 6·8041168 in metres.

$z' = 6921{\cdot}7$ metres.

If

$$\left(\frac{p'}{p}\right)^{\frac{\gamma-1}{\gamma}} = 1 - q \qquad \frac{E\,p^{\frac{1-\gamma}{\gamma}}}{1 - E\,p^{\frac{\gamma-1}{\gamma}}} = -H \text{ as before, vol. xvi. p. 440.}$$

The expression for z' may be put into the form

$$\frac{z'}{1+\frac{z'}{a}} = -\frac{k(1+\alpha\theta)}{g\,H\,\beta} \text{ Nap. log } (1 - Hq).$$

If $\gamma = 1{\cdot}49138$ when $p' = 0$, $q = 1$, we get for the superior limit of the atmosphere an altitude of about 24 miles, or 38918 metres.

Ultimately the intensity of the cold deprives the air of its elasticity[*]. The density therefore requires in strictness to be represented by a discontinuous function; for the formula suggested in this treatise is of course only applicable so long as the air exists in the state of an elastic vapour. The freezing point of air is unknown, and we cannot decide when this condition ceases to obtain.

Delambre estimates the height of the atmosphere as deduced from the phenomena of twilight[†] at 70,800 metres; but this calculation is open to objection. See *Conn. des Temps*, 1841, p. 58.

I have given the example of the calculation of a height by an observation of the barometer, in order to show how my formula for the density may be employed; but however inaccurate in principle the method in use may be, it is sufficiently exact for elevations accessible to man. In all inquiries, however, connected with the condition of the higher regions of the atmosphere, and in the various hypotheses which may be made respecting the decrement of temperature, the corresponding height must be calculated by an appropriate formula, procured agreeably to the hypothesis which may be adopted. Our information respecting the state of the higher regions of the atmosphere is I think more likely to be improved by observations made in aëronautic ascents than by those made on the sides of mountains.

Let $u = -\text{ Nap. log } (1 - Hq)$ $\qquad i = \dfrac{k(1+\alpha\theta)}{a\,g\,H\,\beta}$

$$\frac{z'}{1+\frac{z'}{a}} = a\,i\,u.$$

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[*] See Poisson, *Théorie de la Chaleur*, p. 460. "On peut se représenter une colonne atmosphérique qui s'appuie sur la mer, par example, comme un fluide élastique terminé par deux liquides, dont l'un a une densité et une température ordinaires, et l'autre une température et une densité excessivement faibles." See also Biot, *Conn. des Temps*, 1841.

[†] See Delambre's *Astronomie*, vol. i. p. 337, and Lalande's *Ast.*, vol. ii. art. 2270

At the summit of the atmosphere $q = 1$, if u'' be the corresponding value of u,

$$u'' = -\text{Nap.}\log(1 - H) \qquad c^{-u''} = 1 - H,$$

c being the number of which the hyperbolic logarithm is unity.

$$\frac{E\,p^{-\beta}}{1 - E\,p^{-\beta}} = -H. \quad \text{See vol. xvi. p. 440.}$$

p being the pressure at the lower station; the pressure for ·76568^{m} or 30·14 inches of mercury in the barometer being unity.

I get, when

$$\gamma = 1{\cdot}5 \qquad \beta = -\frac{1}{3} \qquad E = -1{\cdot}192,$$

the following formula for calculating heights by observations of the barometer:

$$\frac{z'}{1 + \frac{z'}{a}} = [4{\cdot}7404605]\frac{(1 + \alpha\theta)}{H}\log(1 - Hq) \text{ in French metres,}$$

$$= [5{\cdot}2564585]\frac{(1 + \alpha\theta)}{H}\log(1 - Hq) \text{ in English feet,}$$

$$= [1{\cdot}5338195]\frac{(1 + \alpha\theta)}{H}\log(1 - Hq) \text{ in English miles,}$$

the temperature θ at the lower station being reckoned from the freezing point.

$$\text{Log}\,\alpha = 7{\cdot}3187588 \text{ for Fahrenheit's scale.}$$

If we assume the 21st observation of Gay Lussac, and suppose $\gamma = 1{\cdot}4$, I find

$$\beta = -{\cdot}2857 \qquad E = -{\cdot}8405 \qquad \log H = 9{\cdot}6596173.$$

In Fahrenheit's scale

$$\tau = \frac{[2{\cdot}9935785]}{p^{\beta} + {\cdot}8405} - 448^{\circ}.$$

Height in miles = $[1{\cdot}9885722]\log(1 - Hq)$.

If we suppose $\gamma = 1{\cdot}5$, I find

$$\beta = -{\cdot}3333 \qquad E = -1{\cdot}1920 \qquad \log H = 9{\cdot}7354232$$

$$\tau = \frac{[30{\cdot}694832]}{p^{\beta} + 1{\cdot}1920} - 448^{\circ}.$$

Height in miles = $[1{\cdot}8457978]\log(1 - Hq)$.

If we suppose $\gamma = 1{\cdot}6$, I find

$$\beta = -{\cdot}375 \qquad E = -1{\cdot}5112 \qquad \log H = 9{\cdot}7794573$$

$$\tau = \frac{[3{\cdot}1285240]}{p^{\beta} + 1{\cdot}5112} - 448^{\circ}.$$

Height in miles $= [1{\cdot}7506111] \log(1 - Hq)$.

Mr. Russell has calculated for me the following table in order to show in what manner the density and temperature of the atmosphere vary in the higher regions under these three different suppositions.

Height in miles.	$\beta = -{\cdot}2857$.			$\beta = -\frac{1}{3}$.			$\beta = -{\cdot}375$.			Height in miles.
	p.	τ.	ϱ.	p.	τ.	ϱ.	p.	τ.	ϱ.	
0	1·0000	+ 87°	1·0000	1·0000	+ 87°	1·0000	1·0000	+ 87°	1·0000	0
4	·4628	+ 24	·5248	·4630	+ 24	·5248	·4631	+ 25	·5249	4
8	·1906	− 45	·2534	·1902	− 48	·2543	·1900	− 50	·2553	8
12	·0656	−122	·1076	·0645	−130	·1086	·0635	−137	·1093	12
16	·0167	−206	·0367	·0153	−223	·0365	·0141	−240	·0362	16
20	·0022	−298	·0080	·0015	−330	·0068	·0009	−361	·0055	20
24	·0000	−399	·0003							24
	Limit 25·81 miles.			23·896 miles.			22·52 miles.			

By making $\gamma = 1{\cdot}5$, the expression for the density becomes simplified, $\frac{1}{1-\gamma} = -2$,

$$\varrho' = \frac{\varrho}{H^2} c^{-u} \left\{ c^{-u} - 1 + H \right\}^2. \quad \text{See vol. xvi. p. 440.}$$

If $\frac{\varrho'}{\varrho} = 1 - \omega$

$$1 - \omega = \frac{1}{H^2} \left\{ c^{-3u} - 2(1-H) c^{-2u} + (1-H)^2 c^{-u} \right\}$$

It must be recollected that the difficulty of determining the densities at different altitudes, and that of determining altitudes by observations of the barometer, rest in finding the accurate law of the temperature. So that if the expression which I have here suggested for the temperature be adopted, the expression for the density, and those for finding the elevation by observations of the barometer, follow as a matter of course, and their accuracy is unquestionable.

The employment of the formula in p. 467, for calculating heights, amounts to determining the constant E from the observations themselves, and not from previous observations. But if the constants are supposed to be known, as in calculating a series of observations made under the same circumstances, it is more simple to employ the expression

$$\frac{z'}{1 + \frac{z'}{a}} = a\,i\,u.$$

The day on which M. Gay Lussac made his ascent was very warm, and the values of γ and H determined from his observations may differ slightly from those mean values which will be obtained hereafter from more complete data. The preceding theory supposes implicitly that a given temperature at the earth's surface always corresponds in any given place to a given pressure; this, owing to the currents, the winds, and to other causes, is not the case; for the atmosphere is never in a state of repose, and its temperature and density are in a continual state of oscillation about their mean values. The constants γ and E may also be subject to variations from fluctuations in the quantity of aqueous vapour diffused through the atmosphere.

If the decrements of temperature are the same for equal increments of altitude, which observation shows is nearly the case at small elevations,

$$\theta - \theta' = A\,z',$$

θ being the temperature at the lower station, θ' at the upper, and z' as before, the altitude of the latter reckoned from the former,

$$1 + \alpha\,\theta' = 1 + \alpha\,(\theta - A\,z'),$$

and if the variation of the force of gravity be neglected

$$\frac{\mathrm{d}\,p'}{p'} = -\frac{g\,\mathrm{d}\,z'}{k\,\{1 + \alpha\,(\theta - A\,z')\}}$$

$$z' = \frac{(1 + \alpha\,\theta)}{\alpha\,A}\left\{1 - \left(\frac{p'}{p}\right)^{\frac{k\,\alpha\,A}{g}}\right\}$$

$$\varrho' = \varrho\left\{\frac{1 + \alpha\,(\theta - A\,z')}{1 + \alpha\,\theta}\right\}^{\frac{g}{k\,\alpha\,A} - 1}$$

p' being the pressure at the upper station, and p at the lower.

Mr. Ivory assumes, Phil. Trans., 1838, p. 192,

$$\frac{1 + \alpha\,\theta'}{1 + \alpha\,\theta} = 1 - f\log\frac{\varrho}{\varrho'} + (f - f')\,\frac{\left(\log\frac{\varrho}{\varrho'}\right)^2}{1\,.\,2}$$

$$- (f - 2f + f'')\,\frac{\left(\log\frac{\varrho}{\varrho'}\right)^3}{1\,.\,2\,.\,3} + \&c.$$

But Mr. Ivory afterwards neglects the terms depending upon f', f'', &c., so that he virtually assumes

$$\frac{1+\alpha\,\theta'}{1+\alpha\,\theta} = 1 - f\left\{\log\frac{\varrho}{\varrho'} - \frac{\left(\log\frac{\varrho}{\varrho'}\right)^2}{1\,.\,2} + \&c.\right\}$$

$$= 1 - f\left\{1 - \frac{\varrho'}{\varrho}\right\} = \frac{p'\,\varrho}{p\,\varrho'}$$

$$\frac{p'}{p} = (1-f)\,\frac{\varrho'}{\varrho} + f\,\frac{\varrho'^2}{\varrho^2}$$

Mr. Ivory makes the constant $f = \frac{2}{9}$, p. 197, so that

$$\frac{p'}{p} = [9{\cdot}8908555]\,\frac{\varrho'}{\varrho} + [9{\cdot}3467875]\,\frac{\varrho'^2}{\varrho^2}$$

$$\mathrm{d}\,p' = p\,(1-f)\,\frac{\mathrm{d}\,\varrho'}{\varrho} + 2\,p\,f\,\varrho'\,\frac{\mathrm{d}\,\varrho'}{\varrho^2}$$

$$= -\,\frac{g\,\varrho'\,\mathrm{d}\,z'}{\left(1+\frac{z'}{a}\right)^2}$$

$$\frac{z'}{1+\frac{z'}{a}} = a\,i\,u^* = \frac{p\,(1-f)}{g\,\varrho}\,\log\frac{\varrho}{\varrho'} + \frac{2\,p\,f}{g\,\varrho}\left(1 - \frac{\varrho'}{\varrho}\right)$$

$$= \frac{k\,(1+\alpha\,\theta)\,(1-f)}{g}\,\log\frac{\varrho}{\varrho'} + \frac{2\,k\,(1+\alpha\,\theta)\,f}{g}\left(1 - \frac{\varrho'}{\varrho}\right)$$

$$= [0{\cdot}9635418]\,\log\frac{\varrho}{\varrho'} + [0{\cdot}3582881]\left(1 - \frac{\varrho'}{\varrho}\right)$$

for 50° Fahr. at the lower station.

As we cannot make direct observations of the temperature and density of the highest regions of the atmosphere, it becomes very important to avail of all indirect means of investigation. The problem of Astronomical Refractions furnishes us with valuable data in this respect, and any hypothesis relative to the state of the atmosphere which will not satisfy the known phænomena of refraction, must of course be discarded. In any investigation of this kind it is indispensable to employ a formula for z in terms of the density consistent with the hypothesis, which may be made respecting the decrement of temperature; it is equally indispensable to carry the integral which affords the amount of refraction through limits which are in conformity with the same supposition.

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* $a\,i\,u = \sigma$ in Mr. Ivory's notation. In this page p is the pressure and ϱ is the density at the earth's surface.

[To be continued.]

LXX. *Proceedings of Learned Societies.*

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Twenty-Seventh Annual Report of the Council.

THE subject which naturally first presents itself to the Council in the preparation of their Report, is the irreparable loss the Society has sustained by the death of its illustrious and venerable President (Mr. Davies Gilbert); to whose early, active and liberal patronage, in conjunction with the efforts of Dr. Paris, it first owed its existence, and by its connexion with his name its labours have been more extensively known, and far more generally acknowledged, than they might otherwise have been.

The Council would fain have indulged in *more grateful* allusions, than a bare official form permits, to the numerous advantages and benefits the Society has owed to his kind and continued benefactions during the twenty-six years it was honoured by his presidence over its affairs—but that they have been anticipated by other pens —yet they cannot look back on his kind and considerate conduct without the feeling that every member of the Society has, by his loss, a friend less in the world.

At the foundation of the Society, twenty-seven years ago, the value of, and necessity for scientific education among our practical miners was barely thought of; yet it was among the very first and most important objects of its founders: and to them it is the sincerest matter of gratulation that a subject so often enforced from its chair, and recommended by its patrons and in its reports, is at length recognized as one of paramount utility and importance.

This has been shown, not only by the foundation of Professorships for civil and mining engineering in the academic institutions of London and Durham, but by the institution of a Mining School in Cornwall, which, although first thought of as the result of a due appreciation of the public virtues of one of the earliest and most munificent friends of this Society (the late Lord de Dunstanville), has been carried into practical effect by the liberality of Sir Charles Lemon, by whose enlightened and patriotic exertions its permanent existence will, we trust, be secured. We look forward to the period when the result of the studies pursued in this institution, united to the extensive practical knowledge to be derived from exploring the variety of our rocks,—the different characters of our "*lodes*," and the vast mechanical powers employed in working our mines,—will raise our miners far above their present position, although even now they bear the character (which they richly deserve) of the best and most useful practical miners in the world.

Much has been said of a reception-room for plans,—and such a depository has from the foundation of the Society been opened here, and to some extent made useful by the liberality and intelligence of some of our mine agents. But when the labour of preparing them—highly valued as they are—and the want of adequate remuneration, are considered, we need not wonder at the comparative poverty of our archives in this respect. Mr. Henwood's Survey of the Mines has probably brought together a larger collection of copies of mining plans than had ever before been obtained; but unless similar labour be still devoted to their accumulation, it is vain to hope for them, unless at an expense which would perhaps but seldom be repaid by their value to any but practical miners.

The rock formations of Cornwall had, until recently, been thought among the most ancient; but the late researches of Messrs. Sedgwick and Murchison induce them to place our "*killas*" at an epoch not anterior to the old red sandstone, on zoological evidence of much force; whilst to the granite and its congeners they ascribe a still more recent date. From the labours of these eminent philosophers, associated with the visit of Prof. Phillips (under the direction of the Government), and, in some humble degree, aided by the efforts of our own members, we hope this obscure portion of geological investigation will receive an elucidation equally luminous with that which Mr. Murchison's herculean labours have shed on the closely-allied rocks of the "*Silurian*" region.

The rapidly-accumulating collections of the Society are even now more than sufficient to fill every species of accommodation the present museum affords; and it will be imperative on your new officers and Council to devise a method for rendering them more generally available and useful than their present circumscribed premises will permit.

It has been suggested, that with so much geological wealth as the Society possesses, the benefits it confers are but limited; and with a view to repeating an experiment which was unsuccessfully made by the late zealous and excellent Secretary (Dr. Boase), a few lectures will from time to time be given by one of the officers during the ensuing winter; their periods will, however, be determined by engagements in which the Society has no part.

It had been confidently anticipated that the Fifth Volume of the Society's Transactions would, ere now, have been in the hands of the members: considerable progress has been made in it during the present year, but its completion has been delayed by professional engagements of the editor; the Council, however, believe that it will be published at an early period of the ensuing season.

By order,
W. J. Henwood,
October 9th, 1840. *Secretary and Curator.*

The following papers have been read since the last Report:—

I. On some singular Metalliferous Deposits in the Mining district near St. Ives, called by the miners "Carbona." By Joseph Carne, Esq., F.R.S., F.G.S., M.R.I.A., &c., &c., Treasurer of the Society.

II. Remarks on the Land-slip between Axmouth and Lyme Regis. By the Rev. Canon Rogers, A.M., Member of the Society.

III. Notes on the Geology of the Counties of Gloucester and Restigouche, in New Bruuswick, and the Canadian bank of the river Restigouche. By W. J. Henwood, C.E., F.R.S., F.G.SS. London and Paris, Hon. M.Y.P.S., Secretary of the Society, and Curator of the Museum.

IV. Observations on a suite of Specimens from the neighbourhood of Exeter. By Joseph Parker, Jun., Esq., Corresponding Member of the Society.

V. On the Age of the Shingle Beach at Pevensey, in Sussex. By John S. Enys, Esq., F.G.S., &c., Member of the Society.

VI. On the Organic Remains contained in the Slates and Limestones of South Devon. By J. C. Bellamy, Esq., Curator of the Devon and Cornwall Natural History Society.

VII. On the Occurrence of Organic Remains in the Slate Rocks of the Southern Coasts of Cornwall. By Charles W. Peach, Esq., Associate of the Society.

VIII. On the Sulphur Ores (iron pyrites) of the Vale of Ovoca, county of Wicklow. By W. J. Henwood, C.E., F.R.S., Corresponding Member of the Plymouth Institute.

The Curator's Report notices the following Donations to the Museum:—

Cinnabar, with hematite iron ore and copper pyrites from Bavaria, vitreous and purple copper ores and copper pyrites from Kenmare mine, county of Kerry, and metallic copper precipitated on the pumps of Connorree mine, county of Wicklow. By Thomas Cornish, Esq.—Specimens from the recent land-slip on the coast between Axmouth and Lyme. By the Rev. Canon Rogers, A.M., Member of the Society.—Specimens from the trap dyke, and of the accompanying rocks, from Mr. Pennant's slate quarry at Penrhyn, Caernarvonshire. By Joseph Carne, Esq., F.R.S., F.G.S, M.R.I.A., &c., Treasurer of the Society.—Wood-tin and garnets, from Polberrow mine, St. Agnes. By John T. Tregellas, Esq.—Pseudomorphous quartz, from Caradon, and crysocolla, from near Five Lanes. By Mr. George Jennings, Jun.—Stream-tin ore from Carnon mine. By Mr. Nicholas S. Cloak.—Pearl spar and iron pyrites from Trevaskus mine. By Mr. — Joseph.—Galena, copper pyrites and sulphuret of antimony from Sicily. By — Floyd, Esq.—A suite of specimens from the neighbourhood of Exeter, and organic remains resembling Alcyonia, from the new red sandstone of Devon. By Joseph Parker, Jun., Esq., Corresponding Member of the Society.—Organic remains from the slate series and limestones near Plymouth. By J. C. Bellamy, Esq., Curator of the Devon and Cornwall Natural History Society.—A fine specimen of Jew's-house tin from St. Austell. By John Michell, Esq.—Chert from Halkin mountain, Flintshire. By Richard Thomas, Esq. (of London).—Organic remains from the summit of Snowdon, and carbonate of manganese from Caernarvonshire. By Henry Thomas, Esq., F.G.S., Associate of the Society.—A suite of specimens from the coast of Antrim and other parts of Ireland. By C. A. Johns, Esq.—Native copper, crystallized copper pyrites, and iron pyrites from Providence mines, near St. Ives; galena from North Wheal Alfred, with specimens from the copper and sulphur (iron pyrites) ores of Wicklow. By Mr. Higgs, Member of the Society.—Chalcedony, organic remains from the chalk, and conglomerates from near King's Langley, Herts. By H. Campbell White, Esq., F.G.S., &c., &c., &c.—Quartzose slickenside and purple copper ore from Carn Brea mines, crystals of the red oxide of copper, silicate of tin from Weald Coats, copper pyrites and carbonate of iron from Wheal Tolgus, and a new ore of copper from Great Saint George mine. By John Garby, Esq., Associate of the Society.—Crystalline quartz from Knockmahon mine, county of Waterford. By John Petherick, Esq.—Acicular oxide of copper from Knockmahon mine. By Captain James Clemes.—Conglomerate from Slievnamann mountain, county of Tipperary, and sandstone from Mohir, county of Clare. By Day P. Le Grice, Esq., Member of the Society.—Specimens from various localities. By the Rev. Henry Holden.—Organic remains from Fowey and Caerhayse. By C. W. Peach, Esq., Associate of the Society.—Vitreous and purple copper ores, and copper pyrites from Kenmare mine, county of Kerry. By Dillon Croker, Esq.—Recent sandstone from Lelant. By the Rev. W. D. Longlands.—Wood from the diluvium at St. Erth stream. By Mr. Samuel Peters.—Hematite iron ore from Launceston, Van Diemen's Land. By Richard Edmonds, Esq.—Specimens from the neighbourhood of Killarney, county of Kerry, from Knockmahon mine, county of Waterford, and from Cronebane, Tigrony, Connorree, Ballymurtagh, and Ballygahn, in the Vale of Ovoca, county of Wicklow. By W. J. Henwood, C.E., F.R.S., F.G.S., Secretary of the Society and Curator of the Museum.

Officers and Council for the present year:—

President.—Sir Charles Lemon, Bart., M.P., F.R.S., &c. *Vice-Presidents.*—Sir T. D. Acland, Bart., M.P., F.R.S.; John Paynter; John Taylor, F.R.S., &c.; Stephen Davey. *Treasurer*.—Joseph Carne, F.R.S. *Joint Secretaries*.—Samuel Pidwell, Jun.; W. J. Henwood, F.R.S. *Librarian*.—Richard Hocking. *Council*.—John Batten, John J. A. Boase, Thomas S. Bolitho, Samuel Borlase, Charles Fox, Thomas Lean, J. N. R. Millett, Rev. M. N. Peters, W. Petherick, N. Phillips, William Reynolds, W. M. Tweedy.

LXXI. *Intelligence and Miscellaneous Articles.*

ATOMIC WEIGHT OF CARBON.

MM. DUMAS and Stas have, together, performed fourteen experiments relative to the atomic weight of carbon; the results all agree, and were obtained either by the combustion of pure charcoal or of highly carbonated and well-known substances. The combustion was performed in oxygen, and care was taken to dry the gases obtained either by sulphuric acid or chloride of calcium. Thus dried, they were passed through two pieces of apparatus filled with solution of potash, and a third filled with potash in powder. The increased weight of the solutions and dry potash gave the weight of the carbonic acid obtained. Thus the weight of the carbon burnt, and of the carbonic acid gas obtained, were known; and from these, without any hypothesis, the proportions in which the bodies combine could be deduced. According to M. Berzelius, the proportions are 200 of oxygen to 76·52 of carbon. According to the recent experiments, above-mentioned, by MM. Dumas and Stas, the result will be very different, for they gave by the combustion of naphthalin, four experiments, 75·21, 75·01, 75·08, 75·07; by the combustion of camphor, three experiments, 75·1, 75·1, 75·0; by the combustion of benzoic acid, two experiments, 75·09, 75·03; by that of the native graphite of Ceylon, three experiments, 74·91, 75·05, 74·99; by artificial graphite extracted from an iron which contained most of it, two experiments, 74·87, 74·90. "All these numbers," M. Dumas remarks, "agree in showing that the true atomic weight of carbon is 75, and not 76·52. There is consequently an error in the indispensable elements in fixing the formulæ now employed in organic chemistry. That is to say, there will be many formulæ to modify, many analyses to repeat, especially of those substances which are rich in carbon, in which very considerable errors may have been committed."

M. Dumas adds, that the Academy will remark with interest, that this long and laborious series of experiments has brought us to the atomic weight indicated by Dr. Prout, who had long supposed that the atomic weight of charcoal was exactly equal to six times that of hydrogen, or $12·5 \times 6 = 75$, which is the number given by the mean of our results. If, as believed by Prout, and as now appears very probable, all atomic weights are multiples of that of hydrogen by whole numbers, there will be many things to rectify in the atomic weights at present adopted. Future experiment will decide this point, but it is evident that they must be submitted to a serious verification.

"The Academy," continues M. Dumas, "will remark also with interest, that the atomic weight of carbon which results from these experiments agrees much better than the former with the old analyses of Iceland spar, arragonite and marble, made by Thenard and Biot, as well as with the densities of oxygen and carbonic acid, determined either by MM. Biot and Arago, or by M. de Saussure, whose results also approximate to ours with regard to the combustion of charcoal."

M. Boussingault has communicated some analyses of bitumen, which entirely agree with our results."—*L'Institut*, No. 347.

PYRRHITE—A NEW MINERAL.

Only one example of this substance is known, and occurs in a splendid drusy cavity of felspar, which is in the possession of Vice-President Perowski, of Petersburgh. While the cavity chiefly contains felspar crystals several inches in size, finely defined, and of an ochre-yellow colour, it likewise includes six-sided tables of reddish-white, pearly lithion mica; white translucent crystals of albite; crystals of clove-brown rock-crystal; and a few white topazes. The crystals of the new mineral are superimposed on the felspar, are eight in number, and are octahedrons of about three lines in length. Their surfaces are smooth, but possess little lustre, so that their angles cannot be measured with great accuracy; but from observations made on several angles, the mean may be regarded as 109° 28′, so that we may probably assume that the crystals are regular octahedrons. No cleavage is observable. The colour is orange-yellow, and the lustre feebly vitreous. The substance is translucent on the edges, its hardness is that of felspar, but the specific gravity could not be determined. It occurs at Alabaschka, near Mursink, and on account of its yellow colour has been named Pyrrhite.—*Jameson's Journal*, July 1840.

PIHLITE—A NEW MINERAL.

Sefström has discovered at Fahlun a new mineral which replaces mica in granite, and which he has termed Pihlite, in honour of the late M. Pihl, Director of Mines. It is an intermediate substance between talc and mica, and its composition is expressed by the following formula:

$$\left.\begin{matrix}\mathrm{M}\\ m\ n\end{matrix}\right\}\mathrm{S}^3+3\left.\begin{matrix}\mathrm{A}\\ \dot{\mathrm{F}}\end{matrix}\right\}\mathrm{S}.$$

Ibid.

DYSODIL.

This mineral, arranged in systems of mineralogy under the name of *Foliated Mineral Pitch*, Ehrenberg has shown to consist of bitumen, or mineral pitch, mixed with siliceous shells of infusoria, and occasionally with pollen of pines, &c. The wax-yellow variety found in Sicily, is made up of shells of *Naviculæ* and mineral pitch: the nearly black-brown coal of the Westerwalde, is a variety of dysodil; so also is the foliated leather-like bituminous coal of the Geistinger Busch at Rott and Siegburg in the Siebengebirge, and a foliated brown coal of the Vogelsberge. Hence the mineral species named dysodil appears to be a polir-slate impregnated with bitumen. Its colours are black-brown, or black. It never forms very thick beds, but sometimes widely-spread deposits. It is used as fuel.—*Annals of Nat. History*, April 1840.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1840.

Chiswick.—October 1. Overcast. 2, 3. Very fine. 4. Rain. 5. Fine: rain. 6. Fine. 7. Frosty and foggy. 8. Very fine. 9. Hazy. 10. Dense fog: very fine. 11. Hazy. 12—15. Foggy in the mornings: fine. 16. Overcast. 18. Cloudy: rain. 19. Cloudy. 20. Clear. 21. Fine. 22. Hazy; rain. 23. Overcast: rain. 24. Overcast. 25. Very fine. 26. Overcast. 27. Heavy rain: clear. 28. Fine. 29. Foggy: rain: dense fog at night. 30. Cloudy and fine: clear. 31. Foggy: clear at night.

Boston.—Oct. 1, 2. Cloudy. 3. Fine. 4. Cloudy. 5. Cloudy: rain early A.M. 6, 7. Fine. 8. Fine: rime frost this morning. 9. Cloudy. 10, 11. Fine. 12—14. Foggy. 15. Fine. 16. Cloudy. 17. Rain: rain early A.M. 18. Cloudy: rain P.M. 19. Stormy. 20. Fine. 21. Cloudy. 22. Cloudy: rain early A.M. 23. Fine: rain P.M. 24. Fine. 25. Fine: rain early A.M. 26. Fine. 27. Cloudy: rain early A.M. 28. Foggy. 29. Cloudy: rain A.M. and P.M. 30, 31. Foggy.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Oct.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·022	30·039	29·996	29·34	The Meteorological Observations from Applegarth Manse, Dumfries-shire, for October, have not yet been received.		55·3	57·7	51·7	58	46	53·5			S.	W.	calm		...	...	...		50
2.	30·172	30·124	30·039	29·56			53·7	58·7	51·3	61	36	51			W.	NW.	calm		...	...	...		52
☽ 3.	30·194	30·153	30·061	29·66			47·7	59·0	44·3	56	46	44			NW.	N.	NW.		...	...	...		46
4.	30·084	30·056	30·033	29·60			51·2	54·6	46·3	57	45	53			NNW.	NE.	calm		...	·02	...		49
5.	30·100	30·073	29·997	29·54			49·3	57·2	46·4	57	33	50			NW.	N.	calm		...	·07	·16		48
6.	30·126	30·094	30·082	29·60			45·4	55·3	42·6	53	30	44			NW.	W.	calm		·116	...	...		45
7.	30·114	30·121	30·071	29·53			43·7	53·6	39·0	58	30	47			SSW.	N.	calm		...	...	...		42
8.	30·266	30·241	30·190	29·70			40·7	53·9	36·8	60	30	43			S.	NE.	calm		...	...	...		41
9.	30·326	30·293	30·260	29·75			40·3	51·6	40·0	59	30	48			WSW.	S.	calm		...	...	...		39
10.	30·318	30·280	30·254	29·79			45·7	55·4	39·8	60	35	45·5			N.	E.	calm		...	...	...		41
○ 11.	30·400	30·401	30·323	29·92			52·5	56·7	44·4	58	35	40			NE.	NE.	calm		...	...	...		46
12.	30·604	30·549	30·543	30·11			50·3	59·3	44·7	60	31	39			N.	N.	calm		...	...	...		46
13.	30·590	30·548	30·328	30·05			43·2	57·3	42·2	63	31	40			NNW.	W.	calm		...	...	...		43
14.	30·404	30·359	30·271	29·83			43·2	56·6	39·7	66	32	45			SW.	NW.	calm		...	...	...		42
15.	30·258	30·225	30·071	29·69			43·7	60·7	41·9	57	36	41·5			S.	NW.	calm		...	...	...		42
16.	29·936	29·915	29·755	29·34			48·3	54·8	42·0	56	51	49			S.	W.	W.		...	·02	...		46
☾ 17.	29·812	29·965	29·757	29·36			54·8	56·7	48·0	56	46	48·5			NW.	N.	calm		·066	...	·10		49
18.	30·026	29·977	29·755	29·20			50·8	56·8	49·0	56	49	49			SSW.	W.	calm		...	·12	...		47
19.	29·704	29·803	29·664	29·04			53·5	55·7	49·0	57	42	50·5			NW.	NW.	NW.		·133	...	·20		48
20.	30·100	30·075	30·052	29·58			47·7	56·6	45·8	57	29	46			NW.	N.	calm		...	...	...		45
21.	30·156	30·124	30·046	29·65			45·7	54·6	42·2	54	39	42			NW.	N.	calm		...	·01	...		41
22.	29·904	29·897	29·861	29·40			48·8	52·8	44·8	53	38	45			S.	NW.	calm		...	·02	·03		45
23.	29·960	29·933	29·662	29·43			45·2	52·7	43·7	53	40	41·5			SW.	W.	calm		·036	·27	...		42
24.	29·556	29·586	29·523	29·06			45·2	53·2	42·5	53	35	42			W.	NW.	W.		·286	·16	·27		43
● 25.	29·784	29·877	29·746	29·33			42·2	50·6	39·3	51	28	40			NW.	NW.	NW.		·102	...	·10		38
26.	29·886	29·875	29·126	29·40			39·2	49·3	35·4	52	43	38			W.	NW.	calm		...	·28	...		37
27.	29·178	29·204	29·160	28·75			51·3	52·8	38·6	50	30	49			S.	W.	calm		·302	·12	·17		45
28.	29·136	29·266	29·114	28·76			42·7	52·3	41·3	52	36	37·5			S.	SW.	calm		·061	·10	...		42
29.	29·340	29·369	29·292	29·00			45·6	49·4	41·4	50	32	45			NE.	E.	calm		·033	·12	...		43
30.	29·522	29·520	29·477	29·15			47·3	49·6	40·3	56	36	42			E.	SE.	calm		·338	·03	·58		44
31.	29·564	29·566	29·515	29·19			47·3	52·8	43·2	57	36	47			ENE.	S.	calm		...	·01	·02		45
Mean.	29·985	29·984	29·871	29·46			47·2	54·8	43·2	56·32	36·64								Sum. 1·171	1·35	1·63		Mean. 44

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE,
SUPPLEMENT TO VOL. XVII. THIRD SERIES.

LXXII. *Remarks on* Professor Challis's *Investigation of the Motion of a Small Sphere vibrating in a Resisting Medium. By* George Biddell Airy, *Esq. M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN your Number for December there is a paper by my friend Professor Challis, on the theoretical resistance to the motion of a sphere vibrating in an elastic medium. The problem is so difficult, and so important in its application to geodesy, and therefore of such general interest, that I have thought it best to state, in a public communication, the difficulty which I feel with regard to one step of the investigation, and to request Professor Challis to remove my difficulty by communication to your journal.

I see nothing liable to objection in pages 463 and 464; but with the top of page 465 my difficulty commences. The differential equation, tacitly used by Professor Challis, is that whose solution is $v = \frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}$; which equation is perfectly correct for waves, diverging with equal intensity and with corresponding phase, in all directions from a centre; or, if not in all directions, it is yet true if the waves diverge with equal intensity and corresponding phase through all the angular directions included in a spherical sector bounded by material planes, which (produced if necessary) would meet at the centre of the sphere. But it is not true in any other case. Thus we may have two such spherical sectors, separated only by a material partition, and with waves of different intensities and non-corresponding phases propagated in the two sectors, from the centre, or from the surface of a small concentric sphere; and the equation applies to each sector separately; but if the partition be removed it no longer applies to the whole compounded sector. For the pressures against the partition, produced by the fluids on the two sides, were different; and, therefore, on removing the partition, a new motion of a totally different kind, modifying the old pressures, and therefore modifying the old motions, will be introduced. This remark applies even when the phases correspond, if the intensities are different.

Now it appears to me, as far as I can follow the investigation, that some such process has been used as adopting the solution above alluded to, and supposing it to hold with waves of different intensities in different parts of the surface of the small sphere. For the expressions in lines 25 and 28 contain the factor cos θ, which, as it is not differentiated in the operation for forming $\frac{du}{dt}$, but remains still as a factor, seems to imply that the wave in each infinitesimal sector goes on just as if there were no other sector near it communicating lateral pressures.

If I am correct in the view which I have taken of the connexion of these steps of the process, I conceive that the investigation must be considered faulty. Of the truth of my first remarks I have no doubt; but I am less confident as to the exact connexion of the different steps in Professor Challis's investigation; and upon this point I am anxious to be informed. I am, Gentlemen,

Your very obedient servant,

Royal Observatory, Greenwich, December 9, 1840. G. B. AIRY.

LXXIII. *Address of the General Secretaries of the British Association*, RODERICK IMPEY MURCHISON, *F.R.S., F.G.S., and Major* EDWARD SABINE. *V.P.R.S.: read at the Meeting at Glasgow, September* 1840.

[Continued from p. 449, and concluded.]

FROM Zoological researches let us now turn to Physical Geology. One of the most interesting fruits of modern experimental research is the knowledge of the fact, that electrical currents are in continual circulation below the surface of the earth. Whether these currents, so powerful in developing magnetical and chemical phænomena, are confined to mineral veins and particular arrangements of metal and rock, or generally capable of detection by refined apparatus well applied, appeared a question of sufficient importance to deserve at least a trial on the part of the Association. Our present volume records the result of such a trial on the ancient and very regularly stratified rocks of Cumberland, consisting of limestone, sandstone, shale, and coal, so superimposed in many repetitions as to resemble not a little the common arrangement of a voltaic pile. Varied experiments, with a galvanometer of considerable delicacy, failed to detect, in these seemingly favourable circumstances, any electrical current.

The extensive and rapidly increasing applications of iron to public and private structures of all kinds in which durability of material is a first requisite, have made it highly desirable to possess accurate information respecting the nature of the chemical forces which effect the destruction of this hard and apparently intractable metal. The preservation of iron from oxidation and corrosion is indeed an object of paramount importance in civil engineering. The Association was, therefore, anxious to direct inquiry to this subject, and gladly availed itself of the assistance of Mr. Mallet, a gentleman peculiarly qualified for such investigations, both from his knowledge as a chemist, and from his opportunities of observation as a practical engineer. An extensive series of experiments has accordingly been instituted by him, with the support of the Association, on the action of sea and river water, in different circumstances as to purity and temperature, upon a large number of specimens of both cast and wrought iron of different kinds. These experiments are still in progress, and the effects are observed from time to time. They will afford valuable data for the engineer, and form the principal object of the inquiry; but a period of a few years will be required for its completion. In the meantime, Mr. Mallet has furnished a report on the present state of our knowledge of the subject, drawn from various published sources, and from his own extensive observations. In this report he examines very fully the general conditions of the oxidation of iron, and how this operation is greatly promoted, although modified in its results, by sea-water; also in what manner the tendency to corrosion is affected by the composition, the grain, porosity, and other mechanical properties of the different commercial varieties of iron. The influence of minute quantities of other metals, in imparting durability to iron, is also considered. Mr. Mallet devotes much attention to the consequences of the galvanic association of different metals with iron, a subject of recent interest from the applications of zinc and other metals to protect iron, which are at present agitated. He concludes this, his first report, by recommending a series of inquiries, ten in number, which will supply the desiderata immediately required by the engineer and by the chemist.

We have next to notice a report by Professor Powell, on the present state of our knowledge of refractive Indices for the standard rays of the solar spectrum in different media. The difficulty which the fact of the dispersion of light has offered to the universal application of the undulatory theory, has been in a great measure removed by the analysis of Cauchy and others, who have considered the distances of the undulatory particles as quantities comparable to the length of a wave; velocities of propagation of the different rays of the spectrum are made to depend upon the length of wave which constitutes a ray of a given colour, and upon certain constants proper to the medium; these constants being obtained from observations on refractive indices for certain definite rays (or dark lines) of the spectrum, the refrangibility of any other definite ray (whose wave-length has been ascertained by examining an interference-spectrum) becomes known, and may be compared with observation as a test of theory; such experiments have been made by Frauenhofer, Rudberg, and Professor Powell, who has given a tabular view of the various results, without, however, instituting the comparison between theory and observation, which it would be desirable to extend further than has yet been done. It would be important also to elucidate the disturbing effect of temperature, which prevents even existing observations from being rigorously comparable.

The calculations respecting the tides, which have been prosecuted by the aid of the Association ever since its institution, have been continued this year by Mr. Bunt, under the directions of Mr. Whewell. These calculations have now reached such a point, that the mathematician, instead of being, as at the beginning of this period, content with the first rude approximations, is now struggling to obtain the last degree of accuracy.

The country in which we are now assembled, has always been conspicuous for attention to meteorology, a branch of physical science, in which the British Association, with its power of combining the efforts of many observers in distant quarters of the globe, may hope to be especially useful.

In Scotland, Leslie opened a new train of inquiry, by examining the earth's temperature at different depths; and his successor in the University of Edinburgh, is now directing, at the request of the Association, a large and complete course of experiments on that interesting subject. Framed in conformity with the plans adopted for similar objects by Arago and Quetelet, these researches of Professor Forbes contain also the means of determining the power of conducting heat, which different sorts of rock possess; and may thus throw light on some of those peculiarities in the distribution of temperature at greater depths below the surface, which have become known by experience, but are not explained by theory.

In Scotland, Sir David Brewster was the first to obtain an hourly meteorological journal for a series of years, and to draw from that fertile source new and important deductions, which have had a powerful influence on the progress of scientific meteorology. How gratifying to receive, through the same hands, after the lapse of nearly 15 years, an additional contribution of the same kind, and from the same country; but embracing new conditions, on a new line of operations, in order to obtain new results! By the observations now in progress at Inverness, and at Kingussie, the influence of elevation in modifying the laws which have been found to govern the hourly distribution of heat near the level of the sea, may be discovered, and thus a great addition be made to the experimental results, for which science has long been grateful to the distinguished philosopher we have named, and which have been described as "of the highest value to meteorology, and as the only channel through which any specific practical information can be obtained in this most interesting department of physics."

This is no ordinary praise. It is the just tribute of one who is worthy to offer it; one, who at the call of the British Association, has conducted at Plymouth a still more extensive series of similar observations, and has added to them hourly comparisons of the temperature and moisture of the air, and an hourly record of barometric oscillations. Mr. Snow Harris has presented in a few pages of our last report, the precious results of (70,000) observations, and thus rendered them immediately available in the foundations of accurate meteorology. The documents thus patiently collected, are, however, not yet exhausted in value; they may be again and again called into the court of science, and made to yield testimony to other, and as yet, unsuspected truths. They must not be lost. Shall we lay them by in manuscript among other unconsulted records of the past labours of men, or by undertaking their publication, do justice to our workmen, and establish a new claim on the imitation of the present, and the gratitude of future days? This question is of serious import. Already, stimulated by success in thermometric registration, we have set to work on a more perplexing problem; we have resolved to bind even the wandering winds in the magic of numbers. While we speak, the beautiful engines of our Whewells and Oslers are tracing at every instant of time, the displacements of the atmosphere at Cambridge, at Plymouth, at Birmingham, in Edinburgh, in Canada, in St. Helena, and at the Cape of Good Hope; and ere long we may hope to view associated in one diagram, the simultaneous movements of the air over Europe, America, Africa, India, and Australia, recorded with instruments which we have chosen, by men whom we have set to work.

Among the causes which tend to retard the progress of science, few, perhaps, operate more widely than the impediment to a free and rapid communication of thought and of experiments, occasioned by difference of language. It appeared to the British Association, that this impediment might in some degree be removed, as far as regards our own country, by procuring, and causing to be published, translations of foreign scientific memoirs judiciously selected. Accordingly at each of the meetings at Newcastle and Birmingham a grant of 100*l.* was placed at the disposal of a committee appointed to carry this purpose into effect. Aided by the contributions of several translations which have been gratuitously presented to them, the committee have been enabled, in the two last years, to publish fourteen memoirs on subjects of prominent interest and importance in the mathematical and physical sciences, bearing the names of some of the most eminent of the continental philosophers.

Such, gentlemen, is an imperfect review of our recent proceedings. In two essential respects the British Association differs from all the annual scientific meetings of the Continent, no one of which has printed transactions or employed money in aiding special researches. We also differ from them in the communications which, in the name of the representatives of science assembled from all parts of the United Kingdom, we feel ourselves authorized to make from time to time to the Government, on subjects connected with the scientific character of the nation. On our first visit to Scotland, for example, we felt it to be an opprobrium that this enlightened kingdom should, in one essential feature of civilisation, be still behind many of the continental states, and we prepared an address to his late Majesty's Government, urging strongly the necessity of the construction, without delay, of a map of Scotland, founded on the trigonometrical survey. Representations to the same effect have since been made by the Royal Society of Scotland, and by the Highland Society, and the subject has now engaged that attention, which will, we trust, soon procure for this country the first sheets of a large and complete map.

If then it be asked, why are the men of highest station happy to associate and mingle with us in official duties?—why have the heads of the noble houses of Fitzwilliam, Lansdowne,* Northampton, Burlington, Northumberland, and Breadalbane, alternated in presiding over us, with our Bucklands, our Sedgwicks, our Brisbanes, our Lloyds, and our

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* The Marquis of Lansdowne, who had accepted the office, was prevented from attending by deep domestic affliction, and the Marquis of Northampton cheerfully supplied his place.

Harcourts?—why indeed, on this very occasion has Argyll himself, overlooking the claims due to his high position, and his ancient lineage, come forward to act with us, and even to serve in a subordinate office?—may we not reply, that it is, we believe, a consequence of the just appreciation on the part of these patriotic and enlightened noblemen, of the beneficial influences which this Association exercises in so many ways on the sources of the nation's power and honour?

If we have hitherto dwelt almost exclusively on the value of our transactions, researches, recommendations, and the good application of our finances, let it not, however, be supposed, that we are not also fully alive to the advantages which flow from the social intercourse of these meetings, by bringing together, into friendly communion, from distant parts, those who are struggling on (often remote and unassisted) in advancing experimental science. If, indeed, this principle of union (which we are proud to have borrowed from our German brethren) has been hitherto found to work so well amongst our own countrymen, we cannot but doubly recognise its value when we see assembled so many distinguished persons from foreign countries. In the presence of these eminent men, we forbear to allude to individual distinctions, conscious that any brief attempt of our own would fall far short of a true estimate of merits, the high order of which is indeed known to every cultivator of science in Britain. Well, however, may we rejoice in having drawn such spirits to our Isle; valuable, we trust, will be the comparisons we shall be enabled to make between the steps which the different sciences are making in their countries and in our own.

That advantages, indeed, of no mean order arise from such social intercourse, is a feeling now so prevalent, that foreign national associations for the promotion of natural knowledge have rapidly increased. Germany, France, and Italy have their annual Assemblies, and our allies of the Northern States hold their sittings beyond the Baltic. In all this there is doubtless much good, but an occasional more extensive intercourse of a similar nature, to be repeated at certain intervals, is greatly to be desired.

It has therefore appeared to us (and we say it after consultation with many of our continental friends, who equally feel the disadvantage), that the formation of a general congress of science might be promoted at this meeting, which, not interfering with any assemblies yet fixed upon, or even contemplated, may be so arranged, as to permit the attendance of the officers and active members of each national scientific institution.

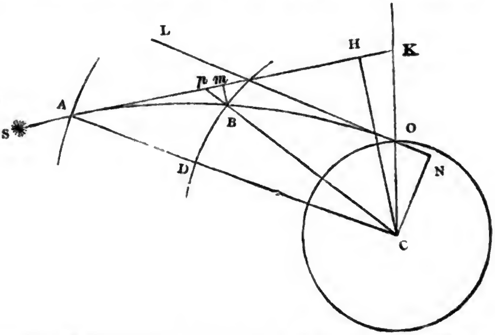
If the British Association should take the first step in proposing a measure of this kind, and should solicit the illustrious Humboldt to act as President, we are sure that scientific men of all nations would gladly unite in offering this homage to a man whose life and fortune have been spent in their cause, whose voice has been so instrumental in awakening Europe to the inquiry into the laws of terrestrial magnetism, and whose ardent search after nature's truths has triumphed over the Andes and the Altai.

If such be your suggestion, then will a fresh laurel be added to the wreath of this city. She who, through the power bequeathed to her by her illustrious offspring, conveys with rapid transit her inventions and her produce to the remotest lands, well can she estimate the value of an union of men whose labours can but tend to cement the bonds of general peace. In such a body the British representatives would, we trust, form no inconspicuous band; and with minds strengthened by the infusion of fresh knowledge, they would, on reassembling for our own national ends, the better sustain the permanent and successful career of the *British Association.*

LXXIV. *On the Heat of Vapours and on Astronomical Refractions.* *By* John William Lubbock, *Esq., Treas. R.S., F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

[Continued from p. 473, and concluded.]

If the constitution of the atmosphere be such as I have concluded, by proper substitutions in the differential equations of refraction, an accurate table of refractions is to be



procured, which may be compared with that of M. Bessel obtained empirically.

Let S A O N be the trajectory described by light emanating from the star S in its passage through the atmosphere to the earth's surface at O, θ the apparent zenith distance, or the angle which the tangent to the trajectory makes with the line C O K at O, C H perpendicular to S A K, the direction of the ray before it enters the atmosphere $= y$, $a = $ C O, then

$$\mathrm{d}\,.\,\delta\theta = \frac{\mathrm{d}\,y}{\sqrt{(a+z)^2 - y^2}}$$

$$y = a \sin\theta \sqrt{\frac{1 + 2\,\mathrm{K}\,\rho}{1 + 2\,\mathrm{K}\,\rho'}}$$

$${}^{*}\alpha = \frac{\mathrm{K}\,\rho}{1 + 2\,\mathrm{K}\,\rho} \qquad y = \frac{a \sin\theta}{\sqrt{1 - 2\,\alpha\,\varpi}}$$

I assume these equations, which are proved by Mr. Ivory in the *Phil. Trans.*, 1838[†], and which are equivalent to similar equations given in the *Méc. Céleste.*

$$\mathrm{d}\,.\,\delta\theta = \frac{\alpha \sin\theta\, \mathrm{d}\,\varpi}{(1 - 2\,\alpha\,\varpi)\sqrt{\cos^2\theta + \left(\frac{z^2}{a} + \frac{z^2}{a^2}\right)(1 - 2\,\alpha\,\varpi) - 2\,\alpha\,\varpi}}$$

$\dfrac{z}{1 + \frac{z}{a}} = a\,i\,u.$ i being a constant and u a certain function of the density, which depends upon the constitution of the atmosphere, and which for the present may remain undefined.

$$\frac{z^2}{a} + \frac{z^2}{a^2} = 2\,i\,u + 3\,i^2 u^2 + \&\text{c.}$$

$$\mathrm{d}\,.\,\delta\theta = \frac{\alpha \sin\theta\, \mathrm{d}\,\varpi \,\{1 + 2\,\alpha\,\varpi + \&\text{c.}\}}{\sqrt{\cos^2\theta + 2\,i\,u + 3\,i^2 u^2 + \&\text{c.} - 2\,\alpha\,\varpi}}$$

if $x = u - \frac{\alpha}{i}\,\varpi$, $\qquad i\,u - \alpha\,\varpi = i\,x \qquad \varpi = 1 - \frac{\varrho}{\varrho'}$

$$2\,i\,u + 3\,i^2 u^2 + \&\text{c.} - 2\,\alpha\,\varpi = 2\,x + 3\,x^2.$$

"The quantities rejected being plainly of no account relatively to those retained. Further, because ϖ is always less than 1, $\frac{\alpha}{1 - 2\,\alpha\,\varpi}$ is contained between α and $\alpha\,(1 + 2\,\alpha)$, and it may be taken equal to α, or to the mean value $\alpha\,(1 + \alpha)$[‡]." Thus we have (See Phil. Trans. 1838. p. 205.)

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[*] This quantity must not be confounded with the α which accompanies θ.

[[†] Mr. Ivory's paper here referred to has been reprinted in L. & E. Phil. Mag., beginning in vol. xv. p. 3, and concluding in vol. xvi.—Edit.]

[‡] Laplace introduces the same simplification. *Méc. Cél.*, vol. iv. p. 24.

$$\mathrm{d}\,.\,\delta\,\theta = \sin\theta \times \frac{\alpha\,(1+\alpha)\,\mathrm{d}\,\omega}{\sqrt{\cos^2\theta + 2\,i\,x + 3\,i^2\,x^2}}$$

$$= \sin\theta \times \underset{[1]}{\frac{\alpha\,(1+\alpha)\,\mathrm{d}\,\omega}{\sqrt{\cos^2\theta + 2\,i\,x}}}$$

$$- \frac{3}{2}\,\underset{[2]}{\frac{\sin\theta\,\alpha\,(1+\alpha)\,i^2\,x^2\,\mathrm{d}\,\omega}{(\cos^2\theta + 2\,i\,x)^{\frac{3}{2}}}} + \&c.$$

The refraction will thus consist of two terms, which I proceed to consider separately. The second term is minute, not amounting to 2″ sex. at the horizon.

Throughout this treatise on Astronomical Refractions one accent will be affixed to any symbol that it may denote the particular value of the variable which obtains at the surface of the earth, and two accents will be affixed when the particular value which obtains at the superior limit of the atmosphere is intended.

The limits of x, or x' and x'' corresponding to u' and u'', are

$x' = 0$, $x'' = u'' - \frac{\alpha}{i}$, because $\omega' = 0$, $\omega'' = 1$.

$x = u - \frac{\alpha}{i}\,\omega = u - \frac{\alpha}{i}\,\mathrm{f}\,u$; the function indicated by the letter f for the present may remain undefined.

By Lagrange's theorem

$$u = x + \frac{\alpha}{i}\,\mathrm{f}\,x + \frac{\alpha^2}{2\,i^2}\,\frac{\mathrm{d}\,(\mathrm{f}\,x)^2}{\mathrm{d}\,x} + \frac{\alpha^3}{2\,.\,3\,i^3}\,\frac{\mathrm{d}^2\,(\mathrm{f}\,x)^3}{\mathrm{d}\,x^2} + \&c.$$

$$= x + \frac{\alpha}{i}\,\omega.$$

Hence

$$\omega = \mathrm{f}\,x + \frac{\alpha}{2\,i}\,\frac{\mathrm{d}\,(\mathrm{f}\,x)^2}{\mathrm{d}\,x} + \frac{\alpha^2}{2\,.\,3\,i^2}\,\frac{\mathrm{d}^2\,(\mathrm{f}\,x)^3}{\mathrm{d}\,x^2} + \&c.$$

Let $x = x'' - X$, $u = u'' - U$, $\omega = 1 - \mathrm{F}\,U$, then

$$x'' - X = u'' - U - \frac{\alpha}{i}\,\{1 - \mathrm{F}\,U\}$$

$$X'' = 0, \quad X' = x''$$

$$X = U - \frac{\alpha}{i}\,\mathrm{F}\,U$$

$$U = X + \frac{\alpha}{i}\,\mathrm{F}\,X + \frac{\alpha^2}{2\,i^2}\,\frac{\mathrm{d}(\mathrm{F}\,X)^2}{\mathrm{d}\,X} + \&c.$$

$$\omega = 1 - \mathrm{F}\,X - \frac{\alpha}{2\,i}\,\frac{\mathrm{d}(\mathrm{F}\,X)^2}{\mathrm{d}\,X} - \frac{\alpha^2}{2\,.\,3\,i^2}\,\frac{\mathrm{d}^2(\mathrm{F}\,X)^3}{\mathrm{d}\,X^2} - \&c.$$

This series may be used if the atmosphere extends only to a finite altitude.

Let

$$\sqrt{\frac{\cos^2\theta + 2\,i\,x}{i}} = z \qquad \cos^2\theta + 2\,i\,x = i\,z^2$$

$$\mathrm{d}\,x = z\,\mathrm{d}\,z \qquad x^2 = \frac{i^2 z^4 - 2\,i\,z^2\cos^2\theta + \cos^4\theta}{4\,i^2}.$$

The integral of $\mathrm{d}\,.\,\delta\,\theta$ is to be taken from

$$z = \frac{\cos\theta}{\sqrt{i}} = z', \qquad \text{to } z = \frac{\sqrt{\cos^2\theta + 2\,i\,x''}}{\sqrt{i}} = z''.$$

Let $\left(\frac{\mathrm{d}^n\omega}{\mathrm{d}\,x^n}\right)'$ represent the value of $\left(\frac{\mathrm{d}^n\omega}{\mathrm{d}\,x^n}\right)$ at the former of these limits, and $\left(\frac{\mathrm{d}^n\omega}{\mathrm{d}\,x^n}\right)''$ its value at the latter, then integrating continually by parts,

$$\int\frac{\alpha\sin\theta\,\mathrm{d}\,\omega}{\sqrt{\cos^2\theta + 2\,i\,x}} = \int\frac{\alpha\sin\theta\,\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\,\mathrm{d}\,x}{\sqrt{\cos^2\theta + 2\,i\,x}}$$

$$\left.\begin{aligned} = \frac{\alpha\sin\theta}{\sqrt{i}}\Bigg\{&\left(\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\right)'' z'' - \left(\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\right)' z' \\ &- \frac{1}{3}\left\{\left(\frac{\mathrm{d}^2\omega}{\mathrm{d}\,x^2}\right)'' z''^3 - \left(\frac{\mathrm{d}^2\omega}{\mathrm{d}\,x^2}\right)' z'^3\right\} \\ &+ \frac{1}{3\,.\,5}\left\{\left(\frac{\mathrm{d}^3\omega}{\mathrm{d}\,x^3}\right)'' z''^5 - \left(\frac{\mathrm{d}^3\omega}{\mathrm{d}\,x^3}\right)' z'^5\right\}\Bigg\}, \\ &\&\text{c.}\end{aligned}\right. \qquad [1]$$

The second term is

$$-\frac{3\,\alpha\sin\theta\,i^2\,x^2\,\mathrm{d}\,\omega}{2\,(\cos^2\theta + 2\,i\,x)^{\frac{5}{2}}}$$

$$= -\frac{3\,\alpha\sin\theta}{8\,i^{\frac{3}{2}}}\left\{i^2 z^2 - 2\,i\cos^2\theta + \frac{\cos^4\theta}{z^2}\right\}\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\,\mathrm{d}\,\omega,$$

the integral of which is

$$-\frac{3\,\alpha\sin\theta}{8\,i^{\frac{3}{2}}}\left\{\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\left\{\frac{i^2 z^3}{3} - 2\,i\,z\cos^2\theta - \frac{\cos^4\theta}{z}\right\}\right.$$

$$- \frac{d^2 \omega}{d x^2} \left\{ \frac{i^2 z^5}{3 . 5} - \frac{2 i z^3 \cos^2 \theta}{3} - z \cos^4 \theta \right\}$$

$$\left. + \frac{d^3 \omega}{d x^3} \left\{ \frac{i^2 z^7}{3 . 5 . 7} - \frac{2 i z^5}{3 . 5} \cos^2 \theta - \frac{z^3}{3} \cos^4 \theta \right\} \right\},$$

$$- \&c.$$

$$= \frac{3 \alpha \sin \theta}{S \sqrt{i}} \left\{ \frac{1}{z} \left\{ \frac{1}{i} \frac{d \omega}{d x} \cos^4 \theta \right\} \right.$$

$$+ z \left\{ - \frac{1}{i} \frac{d^2 \omega}{d x^2} \cos^4 \theta + z \frac{d \omega}{d x} \cos^2 \theta \right\}$$

$$+ \frac{z^3}{3} \left\{ \frac{1}{i} \frac{d^3 \omega}{d x^3} \cos^4 \theta + z \frac{d^2 \omega}{d x^2} \cos^2 \theta - i \frac{d \omega}{d x} \right\} \qquad [2]$$

$$+ \frac{z^5}{3 . 5} \left\{ - \frac{1}{i} \frac{d^4 \omega}{d x^4} \cos^4 \theta + z \frac{d^3 \omega}{d x^3} \cos^2 \theta + i \frac{d^2 \omega}{d x^2} \right\}$$

$$\left. + \frac{z^7}{3 . 5 . 7} \left\{ \frac{1}{i} \frac{d^5 \omega}{d x^5} \cos^4 \theta - z \frac{d^4 \omega}{d x^4} \cos^2 \theta - i \frac{d^3 \omega}{d x^3} \right\} \right\}$$

In order to take this quantity between the proper limits, it is only necessary to write it first with two accents and then with one accent, and take the difference of the quantities so expressed.

Instead, however, of employing the preceding expressions, I shall now introduce the auxiliary quantity e employed by Mr. Ivory. Let

$$\tan \phi = \frac{\sqrt{2 i x''}}{\cos \theta} \qquad e = \tan \frac{\phi}{2}$$

$$\tan \phi = \frac{2 e}{(1 - e^2)^2}$$

$$\sqrt{\cos^2 \theta + 2 i x} = \frac{\sqrt{2 i x''}}{2 e} \sqrt{(1 - e^2)^2 + \frac{4 e^2 x}{x''}}$$

I assume with Mr. Ivory

$$\sqrt{(1 - e^2)^2 + \frac{4 e^2 x}{x''}} = 1 - e^2 + 2 e^2 z$$

$$\frac{d x}{\sqrt{\cos^2 \theta + 2 i x}} = \frac{2 e x'' d z}{\sqrt{2 i x''}}$$

then $\quad x = x'' z - x'' e^2 (z - z^2).$

Suppose $d \omega$ contains any term of the form $A c^{-b x} d x$, then

$$\frac{\alpha \sin \theta \, d \omega}{\sqrt{\cos^2 \theta + 2 i x}} \text{ will contain the term}$$

$$\frac{2\,\alpha \sin\theta\, A\, e\, c^{-x''z} \times c^{x''\,e^2\,(z - z^2)}\; x''\,\mathrm{d}\,z}{\sqrt{2\,i\,x''}},$$

which is to be integrated from $z = 0$ to $z = 1$. This integral may be exhibited under two aspects; in the first, which is that given by Mr. Ivory, the coefficients of the different powers of e consist of a finite number of terms. In other forms of the integral, which will be given here, applicable to all atmospheres of finite altitude, the coefficients are composed of an infinite number of terms, converging with rapidity and in a form suited for numerical computation.

$$c^{-b\,x''z} = 1 - b\,x''\,z + \frac{b^2\,x''^2\,z^2}{1\,.\,2} - \&\text{c.}$$

and the single term $A\,c^{-b\,x}\,\mathrm{d}\,x$ in $\mathrm{d}\,\omega$ will give in

$$\int \frac{\alpha\,(1+\alpha)\sin\theta\,\mathrm{d}\,\omega}{\sqrt{\cos^2\theta + 2\,i\,x}} \text{ the term}$$

$$\begin{aligned}\frac{2\,A\,\alpha\,(1+\alpha)\sin\theta}{\sqrt{2\,i\,x'}}\Bigg\{ & \left\{x'' - b\,x''^2 z + \frac{b^2\,x''^2}{2}\,z^2 - \frac{b^3\,x''^3}{2\,.\,3}\,z^3 + \&\text{c.}\right\} e\,\mathrm{d}\,z \\ & + b\,(1-z)\left\{x''^2 z - b\,x''^3 z^2 + \frac{b^2\,x''^4\,z^3}{2} - \frac{b^3\,x''^5}{2\,.\,3}\,z^4 + \&\text{c.}\right\} e^3\,\mathrm{d}\,z \\ & + b^2(1-z)^2\left\{x''^3 z^2 - b\,x''^4 z^3 + \frac{b^2\,x''^5\,z^4}{2} - \frac{b^3\,x''^6}{2\,.\,3}\,z^5 + \&\text{c.}\right\} e^5\,\mathrm{d}\,z \\ & + \&\text{c.}\Bigg\}\end{aligned}$$

$$\int_0^1 z^m\,(1-z)^n = \frac{n\,(n-1)\,(n-2)\,(n-3)\,\ldots\ldots\,1}{(m+1)\,(m+2)\,\ldots\ldots\,(m+n+1)}.$$

Hence it will be seen that if $\mathrm{d}\,\omega$ contains any number of terms of the form $A\,c^{-b\,x}\,\mathrm{d}\,x$, the definite integral required

$$\begin{aligned}= \frac{2\,(1+\alpha)\,\alpha\sin\theta}{\sqrt{2\,i\,x''}}\Bigg\{ & \left\{x'' + \frac{x''^2}{2}\left(\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\right)' + \frac{x''^3}{2\,.\,3}\left(\frac{\mathrm{d}\,\omega^2}{\mathrm{d}\,x^2}\right)' + \frac{x''^4}{2\,.\,3\,.\,4}\left(\frac{\mathrm{d}^3\,\omega}{\mathrm{d}\,x^3}\right)' + \&\text{c.}\right\} e \\ & + \left\{\frac{x''^2}{2\,.\,3}\left(\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\right)' + \frac{x''^3}{3\,.\,4}\left(\frac{\mathrm{d}^2\,\omega}{\mathrm{d}\,x^2}\right)' + \frac{x''^4}{2\,.\,4\,.\,5}\left(\frac{\mathrm{d}^3\,\omega}{\mathrm{d}\,x^3}\right)' + \&\text{c.}\right\} e^3 \qquad [1] \\ & + \left\{\frac{x''^3}{3\,.\,4\,.\,5}\left(\frac{\mathrm{d}^2\,\omega}{\mathrm{d}\,x^2}\right)' + \frac{x''^4}{4\,.\,5\,.\,6}\left(\frac{\mathrm{d}^3\,\omega}{\mathrm{d}\,x^3}\right)' + \frac{x''^5}{2\,.\,5\,.\,6\,.\,7}\left(\frac{\mathrm{d}^4\,\omega}{\mathrm{d}\,x^4}\right)' + \&\text{c.}\right\} e^5 \\ & + \&\text{c.}\Bigg\}\end{aligned}$$

Another expression for $\delta\,\theta$ may be obtained in the following manner. Suppose $\mathrm{d}\,\omega$ contains any term

$$A_n\,(x'' - x)^n\,\mathrm{d}\,x = A_n\,X^n\,\mathrm{d}\,x$$

$$x'' - x = x_{\prime\prime} (1 - z) (1 + e^2 z)$$

$$\frac{A_n (x'' - x)^n \, d\, x}{\sqrt{\cos^2 \theta + 2\, i\, x}} = \frac{2\, A_n \, x''^{n+1} (1 + e^2)\, z^n}{\sqrt{2\, i\, x''}}$$

$$\int_0^1 (1 - z)^n \, z^m = \frac{m\, (m - 1)\, (m - 2) \, \ldots\ldots \, 1}{(n + 1)\, (n + 2 \, \ldots\ldots \, (n + m + 1)}$$

n and m being whole numbers. Hence

$$\delta = \frac{2\, \alpha (1 + \alpha) \sin \theta}{\sqrt{2\, i\, x''}} \left\{ \begin{array}{l} \left\{ A_0 \, x'' + \frac{A_1 \, x''^2}{2} + \frac{A_2 \, x''^3}{3} + \frac{A_3 \, x''^4}{4} + \frac{A_4 \, x''^5}{5} + \&c. \right\} e \\ + \left\{ \frac{A_1 \, x''^2}{2\,.\,3} + \frac{2\, A_2 \, x''^3}{3\,.\,4} + \frac{3\, A_3 \, x''^4}{4\,.\,5} + \frac{4\, A_4 \, x''^5}{5\,.\,6} + \&c. \right\} e^3 \\ + \left\{ \frac{2\,.\,1\, A_2 \, x''^3}{3\,.\,4\,.\,5} + \frac{3\,.\,2\, A_3 \, x''^4}{4\,.\,5\,.\,6} + \frac{4\,.\,3\, A_4 \, x''^5}{5\,.\,6\,.\,7} + \frac{5\,.\,4\, A_5 \, x''^6}{6\,.\,7\,.\,8} + \&c. \right\} e^5 \\ + \left\{ \frac{3\,.\,2\,.\,1\, A_3 \, x''^4}{4\,.\,5\,.\,6\,.\,7} + \frac{4\,.\,3\,.\,2\, A_4 \, x''^5}{5\,.\,6\,.\,7\,.\,8} + \frac{5\,.\,4\,.\,3\, A_5 \, x''^6}{6\,.\,7\,.\,8\,.\,9} + \frac{6\,.\,5\,.\,4\, A_6 \, x''^7}{7\,.\,8\,.\,9\,.\,10} + \&c. \right\} e^7 \\ + \&c. \end{array} \right\} \qquad [1]$$

A_0, A_1, A_2, &c. are constants, the numerical value of which depends upon the constitution of the atmosphere.

$$\omega = 1 - A_0 \, X - \frac{A_1 \, X^2}{2} - \frac{A_2 \, X^3}{3} - \&c.$$

the first term is necessarily equal to unity, because when $X = 0$ $\omega = 1$, when $\omega = 0$, $X = X' = x''$, therefore generally

$$A_0 \, x'' + \frac{A_1 \, x''^2}{2} + \frac{A_2 \, x''^3}{3} + \&c. = 1.$$

Let ω_1 be written for brevity instead of $\left(\frac{d\, \omega}{d\, u}\right)$,

ω_2 . . . . $\left(\frac{d^2\, \omega}{d\, u^2}\right)$,

ω_3 . . . . $\left(\frac{d^3 \omega}{d\, u^3}\right)$,

then the quantities $\left(\frac{d\, \omega}{d\, x}\right)$, $\left(\frac{d\, \omega^2}{d\, x^2}\right)$ might be deduced from ω_1, ω_2, &c., in the following manner, without having recourse to the series

$$\omega = \mathrm{f}\, x + \frac{\alpha}{2\, i} \, \frac{d\, .\, (\mathrm{f}\, x)^2}{d\, x} + \frac{\alpha^2}{2\,.\,3\, i^2} \, \frac{d^2\, .\, (\mathrm{f}\, x)^3}{d\, x^2} + \&c.$$

Since

$$u - \frac{\alpha}{i}\,\omega = x \qquad\qquad \frac{d\,x}{d\,u} = 1 - \frac{\alpha}{i}\,\frac{d\,\omega}{d\,u} = 1 - \frac{\alpha}{i}\,\omega_1$$

$$\frac{d\,\omega}{d\,u} = \frac{d\,\omega}{d\,x}\,\frac{d\,x}{d\,u}$$

therefore

$$\frac{d\,\omega}{d\,x} = \frac{\omega_1}{1 - \frac{\alpha}{i}\,\omega_1},$$

similarly

$$\frac{d^2\,\omega}{d\,x^2} = \frac{\omega_2}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^3}$$

$$\frac{d^3\,\omega}{d\,x^3} = \frac{\omega_3}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^4} + \frac{3\,\frac{\alpha}{i}\,(\omega_2)^2}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^5}$$

$$\frac{d^4\,\omega}{d\,x^4} = \frac{\omega_4}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^5} + \frac{10\,\frac{\alpha}{i}\,\omega_3\,\omega_2}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^6} + \frac{15\,\frac{\alpha^2}{i^2}\,(\omega_2)^3}{\left(1 - \frac{\alpha}{i}\,\omega_1\right)^7}$$

The quantities $\frac{d\,\omega}{d\,X}$, $\frac{d^2\,\omega}{d\,X^2}$, &c., might also be deduced from $\frac{d\,\omega}{d\,U}$, $\frac{d^2\,\omega}{d\,U^2}$, &c., by similar expressions, only changing the signs of those terms which are multiplied by uneven powers of $\frac{\alpha}{i}$. I have not, however, found it convenient to have recourse to this method of obtaining the development of ω in terms of X. I have employed the series

$$\omega = 1 - \mathrm{F}\,X - \frac{\alpha}{2\,i}\,\frac{d\,(\mathrm{F}X)^2}{d\,X} - \frac{\alpha^2}{2\,.\,3 i^2}\,\frac{d^2\,(\mathrm{F}X)^3}{d\,X^2} - \&c.,$$

and I have found $\frac{\alpha}{2\,i}\,(\mathrm{F}\;X)^2$ by actual multiplication, $\frac{\alpha^2}{2\,.\,3\,i^2}(\mathrm{F}\,X^3)$ by multiplying $\frac{\alpha}{2\,i}\,(\mathrm{F}\,X^2)$ by $\frac{\alpha}{2\,i}\,(\mathrm{F}\;X)$, &c. This process, though somewhat tedious, is extremely easy. As it may be carried on systematically, and the numbers follow each other, it is not liable to error.

So far all is general; it now remains to make some supposition with regard to the function f u, upon which the constitution of the atmosphere depends. If we take, as in vol. xvi., p. 440, see also pres. vol. p. 279.

$$\omega = 1 - H^{\frac{1}{1-\gamma}} c^{-u} \left\{c^{-u} - 1 + H\right\}^{\frac{1}{\gamma-1}}$$

$u = u'' - U$. See p. 490.

$$\omega = 1 - H^{\frac{1}{1-\gamma}} c^{-u''} c^{U} \left\{ c^{-u''} c^{U} - 1 + H \right\}^{\frac{1}{\gamma-1}}$$

$c^{-u''} = 1 - H$, therefore

$$\omega = 1 - \frac{(1 - H)^{\frac{\gamma}{\gamma-1}}}{H^{\frac{1}{\gamma-1}}} c^{U} \left\{ c^{U} - 1 \right\}^{\frac{1}{\gamma-1}}$$

If we take, as in p. 274, $\gamma = 1{\cdot}5$,

$$1 - \omega = \frac{c^{-u}}{H^2} \left\{ c^{-u} - 1 + H \right\}^2 = (1-q)^2 (1-Hq)$$

$$\text{f}\, x = 1 - \frac{c^{-x}}{H^2} \left\{ c^{-x} - 1 + H \right\} \qquad p = p' (1-q)^3$$

$$\omega = 1 - \frac{(1 - H)^3}{H^2} \left\{ c^{3U} - 2c^{2U} + c^{U} \right\},$$

$$i = \frac{k(1 + \alpha\theta')}{a g H \beta}. \text{ See p. 280,} \qquad \theta = \frac{\varrho'}{p'} \left\{ \frac{1}{\alpha} + \theta' \right\} \frac{p}{\varrho} - \frac{1}{\alpha}$$

$$\frac{E p^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} = -H. \text{ See vol. xvi., p. 440.}$$

In page 470 I found $H = {\cdot}54378$ (from the observations of M. Gay Lussac) corresponding to the temperature 87°·35 of Fahrenheit, and to 30·145 inches of mercury in the barometer. As the uncertainty with respect to the values of γ and E appertaining to the mean state of the atmosphere makes it useless to have recourse to greater refinement, I shall now suppose that this value of H will be sufficiently exact for the temperature 50° of Fahrenheit and for 30 inches of mercury in the barometer at the earth's surface; the sequel will show that this hypothesis is admissible, and the calculation of i will stand thus: when $\gamma = 1{\cdot}5$

$$\begin{array}{rlrl}
\log \frac{k}{g M} = & 4{\cdot}2633392 & \log \beta = & 9{\cdot}5228787 \\
\log M = & 9{\cdot}6377843 & \log H = & 9{\cdot}7354232 \\
\log (1 + \alpha\theta') = & 0{\cdot}0159881 & a = & 6{\cdot}8041168 \\
\hline
 & 3{\cdot}9171116 & & 6{\cdot}0624187 \\
 & 6{\cdot}0624187 & & \\
\hline
\log i = & 7{\cdot}8546929 & i = & {\cdot}0071564
\end{array}$$

$$u'' = \text{Nap. log} \frac{1}{1 - H} = {\cdot}78478.$$

The following table shows the constitution of the atmosphere with this system of constants. It should be recollected that in calculating this table, as well as those in p. 278 and p. 280, the law of Mariotte and Gay Lussac,

$$p = k \varrho (1 + \alpha \theta),$$

is implicitly supposed to hold good at very low temperatures, which is to a certain extent conjectural. For this reason, and for the reason that we have not at present sufficient data for determining with great precision the constants γ and E, it is not intended to attach precision to the temperatures, densities and pressures given in the following table for the altitudes beyond 5 miles. The following example will serve to show how the table was calculated:

Calculation of the Pressure, Temperature, and Density for the height of 10 miles.

$\log 10 = 1·0000000$	$i = 7·8546929$
$\log a = 3·5974758$ in miles	$\log M = 9·6377843$
$7·4025242$	$8·2169086$
$·002526$	
$7·4025242$	
$\log 1·002526 = 0·0011364$	$·152925$
	$9·8470750 = \log (1 - Hq)$
$7·4013878$	$·703194$
$8·2169086$	$·296806 = Hq$
$9·1844792$	
$\log Hq = 9·4725517$	
$\log H = 9·7354232$	
$9·7371285$	
$·54592 = q$	
$·45408 = 1 - q$	

$\log (1-q) = 9·6571324$	$9·6571324$	$1·2201080$
2	3	$\log p = 0·4485185$
$9·3142648$	$8·9713972$	$1·6686265$
$9·8470750$	$1·4771213$	$\log \varrho = 9·1613398$
$9·1613398$	$0·4485185$	$2·5072867$
$\varrho = ·14499$	$p = 2·81$	$321·6$
		$448·0$
$\tau = [1·2201080] \frac{p}{\varrho} - 448°$		$\tau = -126·4$

Table showing the constitution of the Atmosphere.

Height in miles.	Pressure p.	Temp. τ.	Density ϱ.
	Inch.	Fahr.	
0	30·00	+50·0	1·00000
1	24·61	35·0	·84611
2	20·07	19·5	·71294
3	16·25	+ 3·4	·59798
4	13·06	−13·3	·49903
5	10·41	30·6	·41403
10	2·81	126·4	·14499
15	·45	240·6	·03573
22·35	……	−448·0	………

According to this system of constants, the ascent for depressing Fahrenheit's thermometer 1° is about 352 feet.

If log α (in sex. sec.) = 1·7669538 α[*] = ·00028348

$$x'' = u'' - \frac{\alpha}{i} = \cdot 74514 \qquad \log\left\{\frac{(1-H)^3}{H^2}\right\} = 9\cdot 5066765.$$

$$\mathrm{F}\ U = \frac{(1-H)^3}{H^2}\left\{c^{3U} - 2c^{2U} + c^{U}\right\}. \quad \text{See p. 473.}$$

$$\begin{aligned} &= [9\cdot 5066765]\ U^2 + [9\cdot 8077064]\ U^3 + [9\cdot 8254352]\ U^4 \\ &+ [9\cdot 6827677]\ U^5 + [9\cdot 4289405]\ U^6 + [9\cdot 0902531]\ U^7 \\ &+ [8\cdot 6829116]\ U^8 + [8\cdot 2178251]\ U^9 + [7\cdot 7028036]\ U^{10} \\ &+ [7\cdot 1436673]\ U^{11} + [6\cdot 5450488]\ U^{12} + [5\cdot 9104758]\ U^{13} \\ &+ [5\cdot 2429802]\ U^{14} + [4\cdot 5449962]\ U^{15} + [3\cdot 8186763]\ U^{16} + \&c. \end{aligned}$$

F X is found by changing U into X in the above series. Although the development of ω might be obtained by procuring the quantities $\frac{d\,\omega}{d\,X}$, $\frac{d^2\,\omega}{d\,X^2}$, &c., from $\frac{d\,\omega}{d\,U}$, $\frac{d\,\omega^2}{d\,U^2}$, through the expressions given in p. 492. I have preferred employing the series

$$d\,\omega = 1 - \mathrm{F}\,X - \frac{\alpha}{2\,i}\,\frac{d\,(\mathrm{F}\,X)^2}{d\,X} - \frac{\alpha^2}{2\,.\,3\,i^2}\,\frac{d^2\,(\mathrm{F}\,X)^3}{d\,X^2}$$

$$\frac{d\,\omega}{d\,x} = \frac{d\,\mathrm{F}\,X}{d\,X} + \frac{\alpha}{2\,i}\,\frac{d^2\,(\mathrm{F}\,X)^2}{d\,X^2} + \frac{\alpha^2}{2\,.\,3\,i^2}\,\frac{d^3\,(\mathrm{F}\,X)^3}{d\,X^3} + \&c.$$

By involution from the expression for F X the following were obtained :

$$\begin{aligned} \frac{\alpha}{2\,i}\,(\mathrm{F}\,X)^2 = &[7\cdot 3105250]\ X^4 + [7\cdot 9125849]\ X^5 + [8\cdot 2225672]\ X^6 \\ &+ [8\cdot 3653575]\ X^7 + [8\cdot 3901249]\ X^8 + [8\cdot 3259417]\ X^9 \end{aligned}$$

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[*] I have adjusted the value of α so that the mean refraction at 45° might exactly agree with that of M. Bessel.

$$\begin{aligned}
&+[8{\cdot}1894155]\,X^{10}+[7{\cdot}9925434]\,X^{11}+[7{\cdot}7440827]\,X^{12}\\
&+[7{\cdot}4492548]\,X^{13}+[7{\cdot}1140008]\,X^{14}+[6{\cdot}7436508]\,X^{15}\\
&+[6{\cdot}3433889]\,X^{16}+[5{\cdot}9048724]\,X^{17}+\&c.
\end{aligned}$$

$$\begin{aligned}
\frac{\alpha^2}{2\,.\,3\,i^2}(\mathrm{F}\,X^3)=&[4{\cdot}9382822]\,X^{6}+[5{\cdot}7162512]\,X^{7}+[6{\cdot}1990788]\,X^{8}\\
&+[6{\cdot}5124208]\,X^{9}+[6{\cdot}7058125]\,X^{10}+[6{\cdot}8073789]\,X^{11}\\
&+[6{\cdot}8341245]\,X^{12}+[6{\cdot}7797954]\,X^{13}+[6{\cdot}7124763]\,X^{14}\\
&+[6{\cdot}5779048]\,X^{15}+[6{\cdot}3758464]\,X^{16}+[6{\cdot}1424833]\,X^{17}\\
&+[5{\cdot}9091202]\,X^{18}+\&c.
\end{aligned}$$

$$\begin{aligned}
\frac{\alpha^3}{2.3.4\,i^3}(\mathrm{F}\,X^4)=&[2{\cdot}4411007]\,X^{8}+[3{\cdot}3458338]\,X^{9}+[3{\cdot}9500667]\,X^{10}\\
&+[4{\cdot}3853642]\,X^{11}+[4{\cdot}6996664]\,X^{12}+[4{\cdot}9204991]\,X^{13}\\
&+[5{\cdot}0668412]\,X^{14}+[5{\cdot}1470479]\,X^{15}+[5{\cdot}1817987]\,X^{16}\\
&+[5{\cdot}1502344]\,X^{17}+[5{\cdot}0587813]\,X^{18}+[4{\cdot}9792296]\,X^{19}+\&c.
\end{aligned}$$

$$\begin{aligned}
\frac{\alpha^4}{2.3.4.5\,i^4}(\mathrm{F}\,X^5)=&[9{\cdot}8470092]\,X^{10}+[0{\cdot}1480392]\,X^{11}+[1{\cdot}5496223]\,X^{12}\\
&+[2{\cdot}0850470]\,X^{13}+[2{\cdot}4878504]\,X^{14}+[2{\cdot}8026972]\,X^{15}\\
&+[3{\cdot}0171128]\,X^{16}+[3{\cdot}1689304]\,X^{17}+[3{\cdot}2999429]\,X^{18}\\
&+[3{\cdot}3830969]\,X^{19}+[3{\cdot}4680509]\,X^{20}+\&c.
\end{aligned}$$

The coefficients of the different powers of X in these series become very small, but they acquire large multipliers from the successive differentiations which are required to give the corresponding terms in the expression for ω.

I find with this constitution of the atmosphere, A_n being the coefficient of X^n in the expression for $\frac{d\,\omega}{d\,x}$.

$$\begin{aligned}
A_1 &= {\cdot}6422\\
A_2 &= 1{\cdot}9268 + {\cdot}0245 = 1{\cdot}9513\\
A_3 &= 2{\cdot}6761 + {\cdot}1635 + {\cdot}0010 = 2{\cdot}8406\\
A_4 &= 2{\cdot}4085 + {\cdot}5008 + {\cdot}0109 + {\cdot}0001 - 2{\cdot}9203\\
A_5 &= 1{\cdot}6110 + {\cdot}9741 + {\cdot}0531 + {\cdot}0007 = 2{\cdot}6389\\
A_6 &= {\cdot}8617 + 1{\cdot}3750 + {\cdot}1640 + {\cdot}0045 = 2{\cdot}4052\\
A_7 &= {\cdot}3854 + 1{\cdot}5250 + {\cdot}3657 + {\cdot}0192 + {\cdot}0003 = 2{\cdot}2956\\
A_8 &= {\cdot}1486 + 1{\cdot}3920 + {\cdot}6353 + {\cdot}0595 + {\cdot}0019 = 2{\cdot}2373\\
A_9 &= {\cdot}0504 + 1{\cdot}0812 + {\cdot}9009 + {\cdot}1428 + {\cdot}0074 \ldots\ldots = 2{\cdot}1827\\
A_{10} &= {\cdot}0153 + {\cdot}7322 + 1{\cdot}0335 + {\cdot}2802 + {\cdot}0229 + {\cdot}0007 \ldots\ldots = 1{\cdot}0848\\
A_{11} &= {\cdot}0042 + {\cdot}4389 + 1{\cdot}1265 + {\cdot}4596 + {\cdot}0561 + {\cdot}0029 \ldots\ldots = 2{\cdot}6882\\
A_{12} &= {\cdot}0010 + {\cdot}2366 + 1{\cdot}0329 + {\cdot}6852 + {\cdot}1094 + {\cdot}0090 \ldots\ldots = 2{\cdot}0741\\
A_{13} &= {\cdot}0002 + {\cdot}1163 + {\cdot}8410 + {\cdot}8072 + {\cdot}2051 + {\cdot}0226 \ldots\ldots = 1{\cdot}9924\\
A_{14} &= \ldots\ldots + {\cdot}0529 + {\cdot}5644 + {\cdot}8410 + {\cdot}3371 + {\cdot}0492 \ldots\ldots = 1{\cdot}8466.
\end{aligned}$$

$$\begin{aligned}
\frac{d\,\omega}{d\,x} = {}&{\cdot}6422\,X + 1{\cdot}9513\,X^2 + 2{\cdot}8406\,X^3 + 2{\cdot}9203\,X^4\\
&+ 2{\cdot}6389\,X^5 + 2{\cdot}4052\,X^6 + \&c.
\end{aligned}$$

Hence by substituting these values of A_1, A_2, &c., in the expression for $\delta\,\theta$ given in p. 491, I find the first term in the refraction

$$= \sin\theta\,\{1132''\!\cdot\!8\,e + 639''\!\cdot\!9\,e^3 + 220''\!\cdot\!4\,e^5 + 60''\!\cdot\!5\,e^7 + 17''\!\cdot\!8\,e^9 + 5''\!\cdot\!5\,e^{11} + \&c.\} \qquad [1]$$

At the horizon $e = 1$, and this portion of the horizontal refraction $= 2076''\!\cdot\!9$.

The second term in the refraction is

$$-\frac{3}{2}\,\frac{\sin\theta\,\alpha\,i^2\,x^2\,\mathrm{d}\,\omega}{(\cos^2\theta + 2\,i\,x)^{\frac{3}{2}}}$$

$$= -\frac{3\,.\,4\sqrt{i}\,\alpha\sin\theta\,x''^2\,z^2\,\{1 - e^2 + e^2 z\}^2\,e^3\,\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\,\mathrm{d}\,z}{2\sqrt{2\,x''}\,\{1 - e^2 + 2\,e^2 z\}^2}$$

$$= -\frac{3\,.\,4\sqrt{i}\,\alpha\sin\theta\,x''^2\,z^2\,e^3}{2\sqrt{2\,x''}}\,\frac{\mathrm{d}\,\omega}{\mathrm{d}\,x}\,\mathrm{d}\,z\left\{1 - 2\,z\,e^2 + \{5\,z^2 - 2\,z\}\,e^4 - \&c.\right\}$$

Suppose $\mathrm{d}\,\omega$ contains any term

$$A_n\,(x'' - x)^n\,\mathrm{d}\,x$$

$$x'' - x = x''\,(1 - z)\,(1 + e^2 z)$$

$$\mathrm{d}\,.\,\delta\,\theta = -\frac{3\,.\,4\sqrt{i}\,\alpha\sin\theta\,x''^{n+2}}{2\sqrt{2\,x''}}\,A_n(1 - z)^n\,(1 + e^2 z)^n\,z^2\,e^3\,\mathrm{d}\,z\left\{1 - 2\,z\,e^2 + \{5\,z^2 - 2\,z\}\,e^4 - \&c.\right\}$$

Neglecting the higher powers of e

$$\theta = -\frac{3\,.\,4}{2}\,\frac{\sqrt{i}\,\alpha\sin\theta\,e^3}{\sqrt{2\,x''}}\left\{\frac{2\,.\,1\,A_1\,x''^3}{2\,.\,3\,.\,4} + \frac{2\,.\,1\,A_2\,x''^4}{3\,.\,4\,.\,5} + \frac{2\,.\,1\,A_3\,x''^5}{4\,.\,5\,.\,6} + \&c.\right\} \qquad [2]$$

With the same constants as before, $\gamma = 1\cdot5$, $H = \cdot54378$

$$\delta\,\theta = -[1\cdot3861838]\sin\theta\,e^3\left\{\frac{2\,.\,1\,A_1\,x''^3}{2\,.\,3\,.\,4} + \frac{2\,.\,1\,A_2\,x''^4}{3\,.\,4\,.\,5} + \frac{2\,.\,1\,A_3\,x''^5}{4\,.\,5\,.\,6} + \&c.\right\} \qquad [2]$$

$$= -\,1''\!\cdot\!5\sin\theta\,e^3.$$

This term thus amounts to only $1''\cdot5$ at the horizon; according to Mr. Ivory it does not amount to more than $1''$.

Hence, finally, the refraction is expressed by the following series :—

$$\text{Ref.} = \sin\theta\,\{1132''\!\cdot\!8\,e + 638''\!\cdot\!4\,e^3 + 220''\!\cdot\!4\,e^5 + 60''\!\cdot\!5\,e^7 + 17''\!\cdot\!8\,e^9 + 5''\!\cdot\!5\,e^{11} + \&c.\}$$

$$= \sin\theta \,\{[3{\cdot}0541728]\, e + [2{\cdot}8051475]\, e^3 + [2{\cdot}3443834]\, e^5 + [1{\cdot}7821564]\, e^7 + [1{\cdot}2501754]\, e^9 + [0{\cdot}7409070]\, e^{11} + \&c.\}$$

$$\tan\phi = \frac{[9{\cdot}0139814]}{\cos\theta} \qquad e = \tan\frac{\phi}{2}.$$

Mr. Russell has calculated a table of refractions from the above formula; and the following comparative view has been drawn up, with Bessel's table in the *Tabulæ Regiomontanæ* (which may be considered as the result of observations), with the table published annually in the *Conn. des Temps*, and with Mr. Ivory's table, recently published in the Phil. Trans. 1838, p. 224.

Tables of Mean Refractions.

Bar. 30 inch. Therm., Fahr., 50°.

App. Zenith Dist.	Mean Refractions.				App. Zenith Dist.
	Calculated.			Observed.	
	Conn. des Temps.	Ivory.	New Table*.	Tab. Reg.	
10°	10″·30	10″·30	10″·30	10″·30	10°
20	21·26	21·26	21·26	21·26	20
30	33·72	33·72	33·72	33·72	30
40	48·99	48·99	48·99	48·99	40
45	58·36	58·36	58·36	58·36	45
50	69·52	69·52	69·51	69·52	50
55	83·25	83·25	83·24	83·24	55
60	100·86	100·85	100·85	100·85	60
65	124·65	124·65	124·63	124·62	65
70	159·22	159·16	159·16	159·11	70
75	214·83	214·70	214·68	214·58	75
80	320·63	320·19	320·08	319·88	80
81	354·33	353·79	353·64	353·38	81
82	395·37	394·68	394·47	394·20	82
83	445·87	445·42	445·11	444·86	83
84	511·22	509·86	509·34	509·23	84
85	595·80	593·96	593·13	593·38	85
85½	648·34	646·21	645·15	647·10	85½
86	710·07	707·43	706·04	707·15	86
86½	783·07	779·92	778·08	777·36	86½
87	870·37	866·76	864·30	864·59	87
87½	975·89	971·93	968·84	972·21	87½
88	1105·1	1101·35	1097·26	1101·40	88
88½	1265·0	1262·6	1257·66	1265·5	88½
89	1464·9	1466·8	1461·49	1481·8	89
89½	1716·4	1729·5	1725·70	1764·9	89½
90		2072·6	2075·4		90

---

* The constitution of the atmosphere is shown by the table in p. 498.

The following table shows the errors of the table of the *Conn. des Temps*, of Mr. Ivory's table, and of my table, assuming Bessel's to be correct.

Zenith Dist.	Error of Table of Conn. des Temps.	Error of Mr. Ivory's Table.	Error of New Table.	Zenith Dist.	Error of Table of Conn. des Temps.	Error of Mr. Ivory's Table.	Error of New Table.
70°	+ ″·11	+ ″·05	+ ″·05	86°	+ 3″·12	+ ″·28	– 1″·11
75	+ ·25	+ ·12	+ ·10	86½	+ 5·71	+ 2·56	+ ·72
80	+ ·75	+ ·31	+ ·20	87	+ 5·78	+ 2·17	– ·29
81	+ ·95	+ ·41	+ ·26	87½	+ 3·68	– ·28	– 3·37
82	+ 1·17	+ ·48	+ ·27	88	+ 3·70	– ·05	– 4·14
83	+ 1·01	+ ·56	+ ·25	88½	– 0·50	– 2·90	– 7·80
84	+ 1·99	+ ·63	+ ·11	89	– 16·90	– 15·00	– 20·30
85	+ 2·42	+ ·58	– ·25	89½	– 48·50	– 35·40	– 39·20
85½	+ 1·24	– ·89	– 1·95				

I think that the discrepancies about 85½, 86, 86½, are caused by irregularities in the refractions of the *Tab. Reg.* Groombridge, who made many observations for the purpose of determining the amount of the refraction near the horizon, makes the horizontal refraction, for barometer 30 inch, and therm. Fahr. 50°, 2075″·4.[*] There is, however, some uncertainty respecting this quantity, and generally respecting the amount of refractions near the horizon. Upon this point see Delambre, *Ast.*, vol. i. p. 319. Mr. Ivory says "There is great probability that the horizontal refraction is very near 2070″, and does not exceed that quantity."

But for the irregularity in Bessel's table, which is clearly seen in the diagram inserted in the annexed plate, my table of mean refractions would be identical with the table of that distinguished astronomer to within 3 degrees of the horizon. It may therefore be safely concluded that the refractions, which belong to the atmosphere, constituted as I have supposed, in conformity with my theory of the heat of steam and other vapours, are consistent with observation.

The quantities denoted by u, (ϱ), s in the *Mécanique Céleste* correspond to the quantities, $i\,x$, ϱ', and $\frac{z}{a}$ of this treatise. The equation

$$s - \alpha\left[1 - \frac{\varrho}{(\varrho)}\right] = u, \quad \textit{Méc. Cél.}\text{, vol. iv. p. 262,}$$

corresponds to the equation

$$u - \frac{\alpha}{i}\,\omega = x \quad \text{of p. 498.}$$

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[*] This curious coincidence, with my value of the horizontal refraction, is of course partly accidental.

Laplace assumes the relation between ω and x.

$$\varrho = (\varrho)\left[1 + \frac{f u}{l'}\right] c^{-\frac{u}{l'}}$$

or in the notation of this treatise

$$\varrho = \varrho' \left\{1 + \frac{i f}{l'}\, x\right\} c^{-\frac{i x}{l'}}$$

$$\omega = -\frac{i f}{l'}\, x\, c^{-\frac{i x}{l'}}$$

f and l' being arbitrary quantities, such that

$$f = \cdot 49042 \qquad l' = \cdot 000741816.$$

A table, similar to that which I have given in p. 498, showing the constitution of the atmosphere, which Laplace has assumed, would be instructive, and would enable us to judge of the admissibility of the conditions attributed to the higher regions of the atmosphere by that great philosopher.

In this treatise I have obtained an expression* for the altitude in terms of the pressure, founded upon the conditions of elastic vapours generally; this gives the relation between u and ω (see p. 471) from which a relation between x and u must afterwards be sought. When on the contrary the relation between ω and x is assumed (as was done by Laplace) an advantage may be gained in the calculation of the refraction, at the expense, however, of a simple and intelligible definition of the constitution of the atmosphere; and such a relation is of course also unconnected with any considerations founded upon the nature of caloric.

Mr. Ivory assumes the relation

$$\frac{p}{p'} = \frac{7}{9}\,\frac{\varrho}{\varrho'} + \frac{2}{9}\,\frac{\varrho^2}{\varrho'^2}$$

p' denoting the pressure, and ϱ' the density of the atmosphere at the earth's surface. From this relation it follows that (see p. 473)

$$a\, i\, u = \frac{7\, k\,(1 + \alpha\,\theta')}{9 g} \log \frac{\varrho'}{\varrho} + \frac{4\, k\,(1 + \alpha\,\theta')}{9 g}\left(1 - \frac{\varrho}{\varrho'}\right)$$

k, α, θ' are L, β, τ', in Mr. Ivory's notation.

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[*] $\dfrac{z}{1 + \frac{z}{a}} = -\, a\, i \log (1 - H\, q).$

When u is a simple function of ω, this value of u may be substituted in the equation

$$x = u - \frac{\alpha}{i}\,\omega^*, \qquad \text{(p. 489)}$$

and the value of ω in terms of x may be found at once by the reversion of the series.

Mr. Ivory makes $\dfrac{k(1+\alpha\,\theta')}{a\,g} = i$, so that

$$x = -\log(1-\omega) + f\log(1-\omega) + \left\{2f - \frac{\alpha}{i}\right\}\omega$$

$$= -\log(1-\omega) + f\log(1-\omega) + h\,\omega.$$

This equation corresponds to the equation of Mr. Ivory

$$x = u - \lambda\,(1 - c^{-u}) - f\,\frac{\mathrm{d}\,c^{-u}\,R_2}{c^{-u}\,\mathrm{d}\,u} - f'\,\frac{\mathrm{d}^2\,c^{-u}\,R_4}{c^{-u}\,\mathrm{d}\,u^2} - \&\text{c.}$$

p. 203, when $f' = 0$. $R_2 = 1 - u - c^{-u}$ $\omega = 1 - c^{-u}$.

The table of mean refractions given by Mr. Ivory is founded upon the supposition that f', f'', &c. $= 0$.

$$\text{Let } i'\,x' = -\frac{k\,(1+\alpha\,\theta)\,(1-f)}{a\,g}\log(1-\omega)$$

$$+\left\{\frac{2\,k\,(1+\alpha\,\theta)}{a\,g}\,f - \alpha\right\}\omega$$

and let $i' = \dfrac{k\,(1+\alpha\,\theta)\,(1-f)}{a\,g} = (1-f)\,i$

$$h' = \frac{h}{1-f} \qquad \lambda = \frac{\alpha}{i} \qquad h = 2f - \lambda$$

$$x' = -\log(1-\omega) + h'\,\omega$$

i and h are identical with the quantities represented by those letters by Mr. Ivory, if

$\alpha = {\cdot}0002835$, $i = {\cdot}0012958$, $h = {\cdot}22566$, $f = \dfrac{2}{9}$

then $i' = {\cdot}0010078$, $h' = {\cdot}29012$.

By Lagrange's theorem I find

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* Mr. Ivory has the equivalent equation $\frac{\sigma}{a} = \frac{s}{a} + \alpha\,\omega = i\,x + \alpha\,\omega$, p. 203. Mr. Ivory's σ is $a\,i\,u$ in the notation of this treatise.

$$\omega = 1 - c^{h'}\, c^{-x'} + h'\, c^{2h'}\, c^{-2x'} - \frac{3\, h'^2}{1\,.\,2}\, c^{3h'}\, c^{-3x'}$$

$$+ \frac{4^2\, h'^3}{1\,.\,2\,.\,3}\, c^{4h'}\, c^{4x'} - \&\text{c.}$$

$$\frac{d\,\omega}{d\,x'} = c^{h'}\, c^{-x'} - 2\, h'\, c^{2h'}\, c^{-2x'} + \frac{3^2\, h'^2}{1\,.\,2}\, c^{3h'}\, c^{-3x'}$$

$$+ \frac{4^3\, h'^3}{1\,.\,2\,.\,3}\, c^{4h'}\, c^{-4x'} - \&\text{c.}$$

The first part of the refraction is given by the expression

$$\alpha\,(1 + \alpha)\sin\theta \int_0^{\infty} \frac{\frac{d\,\omega}{d\,x'}\, d\,x'}{\sqrt{\cos^2\theta + 2\, i'\, x'}}$$

Let

$$n\left\{\frac{\cos^2\theta}{2\, i'} + x'\right\} = z^2$$

$$n\, x' = z^2 - \frac{n\cos^2\theta}{2\, i'} \qquad\qquad d\,x = \frac{2\, z\, d\, z}{n}$$

$$\int_0^{\infty} \frac{c^{-n x'}\, d\, x'}{\sqrt{\cos^2\theta + 2\, i'\, x'}} = \frac{2\, c^{z'}}{\sqrt{2\, i'}\ \sqrt{n}} \int_{z'}^{\infty} c^{-z^2}\, d\, z$$

At the horizon $\cos\theta = 0$, $z' = 0$

$$\int_0^{\infty} \frac{c^{-n x'}\, d\, x'}{\sqrt{2\, i'\, x'}} = \frac{2}{\sqrt{2\, i'}\ \sqrt{n}} \int_0^{\infty} c^{-z^2}\, d\, z = \frac{\sqrt{\pi}}{\sqrt{2\, i'}\ \sqrt{n}}$$

This part of the horizontal refraction

$$= \frac{\alpha\,(1 + \alpha)\sqrt{\pi}}{\sqrt{2\, i'}} \left\{ c^{h'} - \frac{2\, h'\, c^{2h'}}{\sqrt{2}} + \frac{3^2\, h'^2\, c^{3h'}}{1\,.\,2\sqrt{3}} - \frac{4^3\, h'^3\, c^{4h'}}{1.2.3\sqrt{4}} + \&\text{c.} \right\}$$

$$= \frac{\alpha\,(1 + \alpha)\sqrt{\pi}}{\sqrt{2\, i}} \left\{ 1 + \frac{1}{2} f + h - \frac{2\, h}{\sqrt{2}} \right.$$

$$+ \frac{1\,.\,3}{2\,.\,4} f^2 + \frac{3}{2}\, h f + \frac{h^2}{1\,.\,2} - \frac{2\,.\,3}{\sqrt{2}\, 2}\, h f - \frac{2^2\, h^2}{\sqrt{2}} + \frac{3^2\, h^2}{\sqrt{3}\ 1\,.\,2}$$

$$+ \frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6} f^3 + \frac{3\,.\,5}{2\,.\,4}\, h f^2 + \frac{5}{1\,.\,2\,.\,2}\, h^2 f + \frac{h^3}{1\,.\,2\,.\,3}$$

$$- \frac{2\,.\,3\,.\,5}{\sqrt{2}\, 2\,.\,4}\, h f^2 - \frac{2^2\,.\,5}{\sqrt{2}\ 2}\, h^2 f \qquad \frac{2^3\, h^3}{\sqrt{2}\ 1\,.\,2}$$

$$+ \frac{3^2\,.\,5}{\sqrt{3}\ 2\,.\,2}\, h^2 f + \frac{3^3}{1\,.\,2\sqrt{3}}\, h^3 - \frac{4^3}{1.2.3\sqrt{4}}\, h^2$$

$$+\frac{1.3.5.7}{2.4.6.8}f^4+\frac{3.5.7}{2.4.6}hf^3+\frac{5.7}{1.2.2.4}h^2f^2+\frac{7h^3f}{1.2.3.2}+\frac{h^4}{1.2.3.4}$$

$$-\frac{2.3.5.7}{\sqrt{2}\ 2.4.6}hf^3-\frac{2^2.5.7}{\sqrt{2}\ 2.4}h^2f^2-\frac{2^3.7}{\sqrt{2}\ 1.2.2}h^3f$$

$$-\frac{2^4h^4}{\sqrt{2}\ 1.2.3}+\frac{3^2.5.7}{1.2\sqrt{3}.2.4}h^2f^2+\frac{3^3.7}{1.2\sqrt{3}\ 2}h^3f+\frac{3^4h^4}{1.2\sqrt{3}\ 1.2}$$

$$-\frac{4^3\ 7}{1.2.3\sqrt{4}.2}h^3f-\frac{4^4}{1.2.3.4\sqrt{4}}h^4+\frac{4^5}{1.2.3.4.5\sqrt{5}}h^4+\&c.$$

$$=\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}}\left\{1+\lambda(\sqrt{2}-1)-f\left(2\sqrt{2}-\frac{5}{2}\right)\right.$$

$$\left.+h^2\left\{\frac{1}{2}-2\sqrt{2}+\frac{3}{2}\sqrt{3}\right\}-\frac{3}{2}hf(\sqrt{2}-1)+\frac{3}{8}f^2\right\}$$

when the higher powers of f and h are rejected, and this expression agrees with that given by Mr. Ivory, Phil. Trans., 1838, p. 207.

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}}=2036''\cdot5$$

In atmospheres which extend to an infinite distance m (or x'' in the notation of this treatise) is infinite and e always $= 1$, so that in this case the method employed by Mr. Ivory in p. 211 of his memoir, Phil. Trans., 1838, would seem at least to require further elucidation. Mr. Ivory has avoided this consideration, which would otherwise arise with the atmosphere which he has assumed, by imposing an arbitrary limit to the altitude of his atmosphere, while, however, if I am not mistaken, upon his own assumption, the density and the pressure are still finite. When n is large the numerators of the separate quantities of which the quantity A_{2n+1} in p. 211 is composed become large also.

I do not find in Mr. Ivory's paper any remarks tending to prove that the quantities which he has discarded depending upon the higher powers of f and h are incapable of producing any sensible effect; taken separately they are by no means insignificant. Nor do I think it follows as a matter of course, even if the positive and negative terms are numerically of equal value at the horizon, and so fortunately cut one another out, that the same thing will happen necessarily at all other altitudes. Unless the approximation is pushed so far as to secure the retention of all the sensible terms, or those which fairly come within the limits of the errors of observation, any comparison of the result with the valuable table of M. Bessel is illusory and only calculated to lead to incorrect conclusions. It is also indispensable that the relation implied or expressed between z and ω should be in exact conformity with the conditions attributed to the atmosphere, and in this respect the table of mean refractions of the late Mr. Atkinson in the Memoirs of the Astronomical Society appears to me not to rest upon a solid foundation.

Mr. Ivory connects the pressure and the density by the relation

$$\frac{p}{p'} = {\cdot}77777\,\frac{\varrho}{\varrho'} + {\cdot}22222\,\frac{\varrho^2}{\varrho'^2}.$$

M. Biot finds

$$\frac{p}{p'} = {\cdot}761909002718\,\frac{\varrho}{\varrho'} + {\cdot}238167190564\,\frac{\varrho^2}{\varrho'^2} - {\cdot}000076193282,$$

when the coefficients are so taken as to apply as nearly as the question will admit of throughout the whole extent of the atmosphere. But, by a careful examination of the data, M. Biot finds that at the earth's surface the following relation is more accurate.

$$\frac{p}{p'} = {\cdot}956643870584\,\frac{\varrho}{\varrho'} + {\cdot}120146052460\,\frac{\rho^2}{\rho'^2} - {\cdot}076789923044 \qquad \text{(p. 69.)}$$

and at the upper limit of the atmosphere

$$\frac{p}{p'} = {\cdot}6604978157646\,\frac{\rho}{\rho'} + {\cdot}4159581823536\,\frac{\rho^2}{\rho'^2} - {\cdot}00006605394115.$$

According to my view this equation does not contain the true mathematical law which connects the density and pressure, but of course a parabola of this kind may always be found which will osculate the true curve at any given point.

In the *Comptes Rendus des Séances de l'Académie des Sciences*, tom. viii. p. 95, M. Biot verified and adopted a calculation of Lambert, who found from the phenomena of twilight the altitude of the atmosphere (hauteur des dernières particules d'air réfléchissantes) to be 29,115 metres.

It is unnecessary to dwell any further at present upon this subject, because if my theory of the Heat of Vapours be correct, the calculation of Astronomical Refractions, founded upon conditions which are not in conformity with that theory, becomes a problem of mere curiosity.

LXXV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

Anniversary Address of the Rev. Prof. Buckland, *President, Feb.* 21, 1840.

[Continued from p. 396, and concluded.]

POSITIVE GEOLOGY.—DEVONIAN SYSTEM.

IN the Home Department of Positive Geology, the most striking circumstance has been an announcement by Professor Sedgwick and Mr. Murchison of the conclusion to which they were led by Mr. Lonsdale's suggestion in December 1837, founded on the intermediate character of the fossils in the Plymouth and Torbay limestone—that the greater part of the slate rocks of the south of Devon and of Cornwall belong to the old red sandstone formation.

The order of the observations which have led to this important result, is nearly as follows:—

In a paper read at Cambridge, during the winter of 1836–37, Professor Sedgwick considered the fossiliferous slates on both sides of Cornwall to be of the same formation, and coeval, or nearly so, with the calcareous rocks that lie between the slates of South Devon.

In 1836 and 1837 also*, Messrs. Sedgwick and Murchison proposed to transfer the culmiferous or anthracitic shale and grits (Shillot and Dunstone) of *North* Devon to the carboniferous system; withdrawing them from the grauwacke in which they had before been included, and thus assigning a much more recent date than heretofore to the strata which occupy nearly one third part of the map of Devonshire.

But the relations of the slates and limestones of *South* Devon still remained to be determined; the mineral characters of the former being different from those of the old red sandstone beneath the carboniferous group, in many parts of South Wales and in Herefordshire, while the true position of the limestones (e. g. those of Plymouth, Torbay, and Newton Bushell,) was doubtful. At this period (1837), the fossils of this district were examined by Mr. Lonsdale and Mr. Sowerby, to whom the organic remains, both of the carboniferous and Silurian systems, were familiar. It was soon perceived, that while some of the South Devonshire fossils approached to those of the carboniferous strata, and others to those of Siluria, there were still many species which could not be assigned to either system; the whole, taken together, exhibiting a peculiar and intermediate palæontological character. Mr. Lonsdale therefore suggested, that the difficulties which had perplexed this inquiry could be removed by regarding the limestones of South Devon as subordinate to slaty rocks, which represent the old red sandstones of Hereford, Wales, Scotland, and Ireland,—their true place in the series of Devonshire being intermediate between the culmiferous basin of North Devon, and the Silurian strata,—if the latter exist in that county.

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* In August 1836, at the Meeting of the British Association at Bristol; and in a paper read before the Geological Society, May and June, 1837. now published in the Geological Transactions, Second Series, vol. v., Part 3,

The value of this suggestion was not at first appreciated; but after the lapse of more than a year, Mr. Lonsdale's views were adopted (March 1839) by Messrs. Sedgwick and Murchison*, who soon afterwards applied this new arrangement not only to the groups of Devonshire originally under review, but with a boldness which does credit to their sagacity, extended it to the whole of the slaty and calciferous strata of *Cornwall*, till then known only as grauwacke, clay-slate, or killas; assigning to those strata, likewise, the date of the old red sandstone, and resting this determination entirely on the character of the fossils. This change—the greatest ever made at one time in the classification of our English formations—was announced in a memoir read before the Geological Society in April 1839†; the authors then also proposing for the whole series (including both the old red sandstones of Herefordshire, and the fossiliferous slates and limestones of South Devon and Cornwall,) the new name of "*the Devonian system*," and expressing their belief, that many of the groups hitherto called grauwacke, in other parts of the British Islands and on the continent, would ere long be referred to the same geological epoch.

The proposed alteration, therefore, will terminate the perplexity hitherto arising from the circumstance, that the *old* red sandstone of Werner has been frequently confounded with the *new* red sandstone formation of English geologists. It also explains the cause of the English old red sandstone having been rarely recognised on the continent:—for if the Devonian slates afford the normal type of this formation, whilst the marly sandstones and conglomerates of Herefordshire are abnormal exceptions in it, we see the reason why their slaty continental equivalents, like the greater part of the South Devon slates, have been referred to the undivided Wernerian formation of grauwacke.

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* It is to be observed here, that Mr. Murchison, having previously shown that the fossils of the Silurian æra are distinct from those of the carboniferous period, had also pointed out "the vast accumulations" (in which few fossils had at that time been discovered) "then known to separate the two systems." He mentions especially, that "the *fishes* of the old red sandstone—entirely distinct as to form and species—are as unlike those of the Silurian system, as they are to those of the overlying carboniferous system:" adding, "that he has no doubt, although at present unprovided with geological links to connect the whole series, that such proofs will be hereafter discovered, and that we shall then see in them as perfect evidence of a transition between the old red sandstone and carboniferous rocks, as we now trace from the Cambrian, through the Silurian, into the old red system."—See Silurian System, p. 585, line 22, *et seq.*

† [Abstracts of this and all the other papers referred to in this Address as having been read before the Geological Society during the year preceding its delivery, will be found in L. & E. Phil. Mag. vol. xiv. xv. xvi. and in the present volume.—EDIT.]

Mr. Austen, in a communication relating to the structure of the south of Devon, has identified the calcareous slate and limestone of the south of Cornwall with the limestones of this district, and considers that of Torbay among the newest deposits in the latter series.

The Rev. D. Williams also has communicated two papers respecting these disputed rocks, which he refers to the transition or grauwacke system, and endeavours to show that the strata of Devonshire can be distinguished into certain groups by their lithological characters.

Mr. De la Beche in his map of Devon and Cornwall, published in 1839, has adopted divisions of the strata, similar to those of Professor Sedgwick and Mr. Murchison, as to their order of sequence; applying, provisionally, to the culmiferous rocks the name of *Carbonaceous series,* and to the Devonian and Cornish slates the appellation of *Greywacke.*

We know also on the authority of Mr. De la Beche that tin mines are worked in carbonaceous rocks at Owlescomb near Ashburton, on the east side of the Dartmoor granite, and on its west side at Wheal Jewel near Tavistock. He further informs us that one of the richest tin mines now worked in Cornwall, namely the Charlestown mine, east of St. Austle, is in a fossiliferous rock containing Encrinites and corals, and that the same corals occur also near tin mines at St. Just; and in the neighbourhood of Liskeard the Rev. D. Williams has found slates which contain vegetable impressions, dipping under other slates which are intersected by lodes of tin and copper.

From these new facts, we learn that the killas and other slate rocks of Cornwall and the south of Devon do not possess the high antiquity which has till lately been imputed to them; and that tin occurs, as copper, lead and silver have long been known to do, not only in slate rocks that contain organic remains, but even in the coal formation.

Soon after the publication of the views of Messrs. Sedgwick and Murchison, a similar change was applied by Mr. Griffith to the south-west portion of his geological map of Ireland. In a paper that accompanied the presentation of this map to us on 22nd of May last, he states that he has now coloured, as old red sandstone and carboniferous limestone, extensive districts of the counties of Kerry, Cork, and Waterford, previously considered of higher antiquity; imputing his former erroneous opinion to the identity in lithological character of the shales and grits of the old red sandstone and carboniferous systems, with the older rocks in the transition series.

Mr. Griffith has also demonstrated by sections the unconformable position of the carboniferous and old red sandstone formations, which overlie older and more highly inclined slates in the counties of Kerry, Cork, Waterford, and Wexford.

Mr. Charles William Hamilton has likewise adopted similar changes; and believes that the slates which occupy a large space between the Mourne Mountains and Dublin are equivalent to those near Cork, which he now transfers to the old red sandstone.

Mr. Greenough, in the new edition of his map of England, represents nearly the same boundaries and order of succession in Devon and Cornwall as we find in the maps of Mr. De la Beche and Messrs. Sedgwick and Murchison; but in his memoir connected with the map, adopting the name of *Carbonaceous series* for the culmiferous rocks, he substitutes that of *Upper killas* for the Devonian system of Sedgwick and Murchison, (including under that term the old red sandstone of Herefordshire,) and *Lower killas* for the slates inferior to the Silurian system, which they have termed Cambrian.

Mr. Greenough, in his memoir, also shows by quotations from Dr. MacCulloch, that the undisputed old red sandstone of the north of Scotland exhibits, at intervals, the same great changes of mineral character, that occur in the strata intermediate between the Carbonaceous and Silurian systems in the west of England and on the borders of Wales; and justly infers the inadequacy of any one term to characterize formations which vary so much in lithological composition, that at one place they present the condition of a fine-grained silky slate, at another of sandstone, and at a third that of coarse gravel and conglomerate rock.

Thus, with respect to the slate rocks of Devon, Cornwall and Wales, the difficulties are reduced to those of an unsettled nomenclature; whilst nearly all parties are in unison as to the fundamental fact of referring the slates of South Devon and Cornwall to the epoch of the old red sandstone formation. The term grauwacke, however, I rejoice to think, will not be condemned to the extirpation which has been threatened from the nomenclature of geology; it may still retain its place as a generic appellative, comprehending the entire transition series of the school of Freyberg, and divisible into three great subordinate formations:—the Devonian system of Sedgwick and Murchison being equivalent to the upper grauwacke, the Silurian to the middle grauwacke, and the Cambrian system to the lower.

In this threefold distribution of the vast series of strata which have hitherto been indiscriminately designated by the common term grauwacke, we are, as it were, extending the progressive operations of a general inclosure act over the great common field of geology; we propose a division, founded on measurements, surveys, and the study of organic remains, analogous to that of the secondary strata, from the chalk downwards to the coal formation, established by William Smith, and to the separations of the once undivided territory of the great tertiary system, effected by Cuvier and Brongniart, Desnoyers, Lyell, and Deshayes.

To the uninitiated in geology, rectifications in the distribution of strata upon so large a scale may seem calculated to shake confidence in all the conclusions of our science; but a contrary inference will be drawn by those who know that these corrections have never been applied to conclusions established on the sure foundation of organic remains, but to those rocks only of which the arrangement had been founded on the uncertain character of mineral composition.

COAL FORMATION.

The Society has received from Professor Ansted a paper on the Carboniferous and Transition Rocks of Bohemia, a country which he visited last summer, directing especial attention to the district between Prague, Luditz and Pilsen, which he has illustrated by sections made from personal observation. Above the fundamental granite and gneiss he found extensive deposits of grauwacke, on which lie, in unconformable superposition, disconnected patches of the coal formation. The age of this coal is well known, from the fossil Flora of Count Sternberg, who resided in the midst of it near Swina, to be identical with that of the great Coal formation of England. Mr. Ansted gives information also as to the action of trap rocks in producing disturbances of the strata in this district; and respecting dislocations, by which the grauwacke is several times placed on a level with the coal measures, whilst in some cases the strata are inverted and the coal measures laid beneath the grauwacke.

We have received an interesting communication from Mr. Hawkshaw respecting a remarkable disclosure made in the Bolton Railway, six miles north of Manchester, of five fossil trees in a position vertical to the plane of the strata in which they stand. The roots are imbedded in a soft argillaceous shale immediately under a thin bed of coal. Near the base of one tree, and beneath the coal, more than a bushel of hard clay nodules was found, each inclosing a cone of *Lepidostrobus variabilis.* The bark of the trees was converted to coal, from one quarter to three quarters of an inch thick; the substance which has replaced the interior of the trees is shale; the circumference of the largest of them is 15½ feet at the base, 7½ at the top, and its height 11 feet. One tree has spreading roots, four feet in circumference, solid and strong. By the care of Mr. Hawkshaw these trees have been preserved, and a covering is erected over them. The attendant phænomena seem to show that they grew upon the strata that lie immediately beneath their roots*.

Mr. Barber Beaumont, in a communication respecting these same trees, considers that no drifted plants occur in coal fields, and that all the vegetables which are now converted into coal, grew upon swampy islands covered with luxuriant vegetation, which accumulated in the manner of peat bogs; that these islands, having sunk beneath the sea, were there covered with sand, clay and shells, till they again became dry land, and that this operation was repeated in the formation of each bed of coal. In denying altogether the presence of drifted plants, the opinion of the author seems erroneous; universal negative propositions are in all cases dangerous, and more especially so in geology: that some of the trees which are found erect in the coal formation have not been drifted, is, I think, established on sufficient evidence; but there is equal evidence to show that other trees, and leaves innumerable which pervade the strata that alternate with the coal, have been removed by water to con

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[* See the abstract of this paper in L. & E. Phil. Mag. vol. xv. p. 539, and also that of a further communication from Mr. Hawkshaw, in the present number.—EDIT.]

siderable distances from the spots on which they grew. Proofs are daily increasing in favour of both opinions: viz. that some of the vegetables which formed our beds of coal grew on the identical banks of sand and silt and mud, which being now indurated to stone and shale, form the strata that accompany the coal; whilst other portions of these plants have been drifted, to various distances, from the swamps, savannahs, and forests that gave them birth, particularly those that are dispersed through the sandstones, or mixed with fishes in the shale beds.

The cases are very few in which I have ever seen fossil trees, or any smaller vegetables erect and petrified in their native place. The Cycadites and stumps of large Coniferous trees on the surface of the oolite in Portland, and the stems of Equisetaceous plants described by Mr. Murchison in the inferior oolite formation near Whitby, and erect plants which I have found in sandy strata of the latter formation near Alencon, are examples of stems and roots overlaid by sediment and subsequently petrified without removal from the spots in which they grew. At Balgray, three miles north of Glasgow, I saw in the year 1824, as there still may be seen, an unequivocal example of the stumps of several stems of large trees standing close together in their native place in a quarry of sandstone of the coal formation.

In a paper on the sinking of the surface over coal mines, Mr. Buddle has shown that the depressions produced on the surface by the excavation of beds of coal near Newcastle-on-Tyne are regulated by the depth and thickness of the coal, the nature of the strata above it, and also the partial or total extraction of the beds of coal. The accumulation of water forming ponds in these superficial depressions, and the sinkings of a railway, have afforded accurate measures of the amount of the subsidences in question.

WEALDEN AND PORTLAND FORMATIONS.

In the north of Germany Mr. Roëmer, of Hildesheim, has identified beneath the Cretaceous system, the Purbeck stone and beds of the Wealden formation, with nearly all its characteristic shells, and three minute species of Cypris. He has also found the Portland sand and the upper and lower Green sand and the Gault clay, in the north of Germany. He has, moreover, found the Wealden formation near Bottingen in the High Alps.

CHALK FORMATION.

In extension of our knowledge of the Chalk formation, the Rev. J. Gunn has sent us a short communication, accompanied by a lithograph representing the columnar disposition of some Paramoudras to the height of many feet one above another in the chalk of Norfolk. The history of these enormous urn-shaped flints, which were first noticed by Professor Buckland in an early volume of our Transactions, 1st series, vol. iv. p. 413. pl. 24., is still involved in much obscurity. Their form is most probably due to siliceous matter collected around and penetrating throughout the substance of gigantic spongiform bodies; but we have yet to learn the reason why they are occasionally placed in single vertical rows, almost like the joints of a basaltic column, sometimes nearly touching, but not articulating with one another.

A paper has been read by Mr. Henry Lawes Long on the occurrence of numerous *subterraneous chasms* or *swallow-holes* in the chalk on the west of Farnham, with observations on the drainage of the country near the west extremity of the highly-inclined ridge of chalk, called the Hog's Back, between Guildford to Farnham. The land-springs immediately on the north of Farnham descend southwards in open gulleys over tertiary strata, until they arrive at the narrow band of chalk which passes under Farnham Park, where they are suddenly engulphed in transverse fissures or swallow-holes, through which they pass under ground to a considerable distance, and again break forth on the southern side of the chalk. Seven of these swallow-holes occur near Farnham, from some of which the water emerges in sufficient force to turn a mill. They are probably connected with subterranean faults and transverse fractures, the origin of which was coeval with the elevation of the narrow band of chalk, which forms the Hog's Back, and which, near Farnham, is inclined at a high angle to the north. The water that now passes through the Farnham swallow-holes may tend to enlarge the chasms through which it takes its subterraneous course, by dissolving slowly the chalk of their sides in the small quantities of carbonic acid which rain-water usually contains.

Similar transverse fractures, on a greater scale, have given origin to the chasms, which, being enlarged by denudation into transverse valleys, afford outlets through the high escarpment of the chalk to the rivers that, rising within the Weald, flow through the escarpment of the north Downs into the valley of the Thames, and through the escarpment of the south Downs into the sea, viz. to the Wey, the Mole, the Darent, the Medway, and the Stour, through chasms in the north Downs; and to the Arun, the Aduz, the Ouse, and the Cuckmere, through chasms in the south Downs.

Dr. Mitchell has communicated a paper on Artesian and other wells, in the gravel and London clay in Essex, showing that water occurs under the London clay at various depths; the deepest at Foulness Island, being 460 feet. He attributes this inequality in part to uneveness in the surface of the subjacent chalk. On reaching the chalk a large volume of water usually rushes up. Artesian wells are now general in Essex, where they are of the greatest utility in districts that have no natural springs. He also gives an interesting list of localities, both of constant and intermitting springs, some of them very powerful, that burst out from the chalk.

Dr. Mitchell has also communicated an account of deleterious gases that occur in wells in the chalk and strata above it near London. The most abundant of these, namely, carbonic acid gas, issues very partially and only from certain strata, and produces sometimes effects fatal to persons employed in digging wells. Sulphuretted hydrogen is occasionally met with in chalk; and both sulphuretted hydrogen and carburetted hydrogen occur in beds immediately above the chalk.

SUPERCRETACEOUS FORMATIONS.

In illustration of the history of the Eocene division of the tertiary strata, Mr. Bowerbank has concluded, from his personal observations at White cliff bay in the Isle of Wight, that there are no well-defined zoological distinctions between the London and plastic clays, but that in the cliffs of this bay the same shells are common to alternations of these clays with one another. At Alum bay also he found many London clay fossils in beds of greenish grey sand and clay below the variegated sands and clays referred by Mr. Webster to the plastic clay. A similar rectification was sometime ago proposed by Professor Sedgwick.

We have also witnessed during the past year the commencement of a valuable publication by Mr. Bowerbank on the fossil fruits and seeds of the London clay, illustrated with very numerous and accurate engravings by Mr. James Sowerby.

The great attention the author has long paid to the remains of fruits and seeds which occur in such vast abundance in the Isle of Sheppy, whence he has collected not less than 25,000 specimens, place him in a position peculiarly advantageous for the object before him. In this work drawings will be given of the anatomical structure of many of these fossils, as seen under the microscope. The simple expedient Mr. Bowerbank has adopted of preserving these fruits in jars of water, has kept him in the entire possession of every specimen ever placed in his collection; whilst of the thousands of similar fossils that have been deposited in other collections, including that at the British Museum, nearly all have perished from the decomposition of the iron pyrites with which they are always penetrated.

Mr. Lyell has communicated to us a paper full of elaborate detail of facts, and of ingenious speculations respecting the Boulder formation, or drift, associated with freshwater deposits, in the mud cliffs of Eastern Norfolk. These cliffs are in some places 400 feet high, and consist of chalk, crag, freshwater deposits, drift mud and sand, stratified and unstratified;—with superficial accumulations of flint gravel. The centre of his observations is the town of Cromer; he considers the Boulder formation to have been accumulated on land permanently submerged, and not, by one or many, transient advances of water over dry land, and therefore proposes, as Mr Murchison and others have already done, to substitute the term of Drift for that of Diluvium, which many other writers have assigned to it. The Drift, or Diluvium, is of two kinds; one composed of sand, loam, clay, and gravel, all regularly stratified; the other consisting of clay, not divided into beds, and containing boulders of granite, trap and other rocks.

This clay is known on the east and north-east coast of Scotland by the name of Till. He considers the stratified Drift and Till to be contemporaneous formations, and compares the latter to moraines formed at the termination of glaciers. He imagines that drifted masses of ice, charged with earthy matter and fragments of rock, may have deposited the Till as they melted in still water, and the occasional intercalation or juxta-position of stratified materials is ascribed to the action of currents on materials also falling from melting icebergs.

Mr. Lyell refers the complicated bendings and tortuous foldings of many beds of this formation near Mundesley and Cromer to lateral pressure from drifting ice, especially where extremely contorted beds repose upon undisturbed and horizontal strata. But he admits that some of them may be due to landslips of ancient date, and which had no connection with the present line of cliffs. At the bottom of the boulder formation, and immediately above the chalk, extensive remains of a buried forest occur, the stools of the trees being imbedded in black vegetable earth. From the position of this forest a vertical subsidence of several hundred feet and a subsequent rise of the land to the same amount is inferred. This forest and a bed of lignite are connected with fluviatile or lacustrine deposits, which occur about the level of low water below the drift; but at Mundesley they are partly above it, and the freshwater shells which they inclose being nearly all of British species, show that they, as well as the contemporaneous drift, all belong to the newer Pliocene period.

In an Address formerly delivered from this chair, in 1836, and in a subsequent edition of his "Principles of Geology," as well as in his "Elements," Mr. Lyell has called our attention to some differences of opinion which had been expressed by several eminent conchologists as to the number of fossil shells of the crag of Norfolk and Suffolk which could be identified with living species. So great was the discordance of the results at which M. Deshayes, Dr. Beck, and others seemed to have arrived, that their announcement was calculated materially to impair our confidence in the applicability of the chronological test so much relied on by Mr. Lyell for the classification of the tertiary formations; namely, that derived from the proportional number of recent and extinct species discoverable in each deposit. In the hope of arriving at some definite conclusion on this important point, Mr. Lyell visited Norfolk and Suffolk during the last year, and having obtained a considerable collection from the crag near Norwich and Southwold, he instituted, with the assistance of Mr. Searles Wood and Mr. George Sowerby, a thorough comparison between them and recent species. The fossil shells of this formation, which the author calls the Norwich crag, are partly marine, and partly freshwater, and indicate a fluvio-marine origin, and the proportion of living species was found to be between 50 and 60 per cent. This deposit, therefore, the author refers to the older Pliocene period. A similar examination was then made of 230 species of shells from the Red Crag in Mr. Wood's museum, and it was found that 69 agreed with living species, being in the proportion of about 30 per cent. This group therefore Mr. Lyell ascribes to the Miocene era. A collection of 345 species of Coralline Crag shells in Mr. Wood's cabinet was then compared in like manner, and sixty-seven were determined to be identical with recent species, being about 19 per cent. Mr. Lyell, therefore, considers that the Coralline Crag is also

Miocene, although belonging to a more remote part of that period than the Red Crag. Having obtained from M. Dujardin a collection of 240 shells from the Faluns of Touraine, he found with Mr. George Sowerby's assistance that the recent shells were in the proportion of twenty-six per cent., so that he has now come round to the opinion long ago announced by M. Desnoyers, that upon the whole the Crag of Suffolk corresponds in age with the Faluns of Touraine, both being Miocene, although the species in the two countries are almost entirely distinct, those of England having a northern and those of France a sub-tropical character. I am also informed by Mr. Lyell, that out of 400 marine and freshwater species, from the Eocene strata of the London and Hampshire basins, Mr. G. Sowerby was scarcely able to identify two per cent. with living shells. It is satisfactory therefore to observe that the test of age derived from the relative approach to the recent Fauna is in perfect accordance with the independent evidence drawn from superposition. We ascertain for example by superposition that the freshwater strata of the mud cliffs of East Norfolk rest on Norwich crag, and are the newest formation of all. They are then followed in the descending series by, 1st, the Norwich, 2ndly, the Red, and 3rdly, the Coralline Crag, beneath which is the London Clay. The same order of sequence is indicated by the organic remains considered independently, and simply with reference to the degree of their correspondence with the existing Fauna.

It has been known for many years, that near Bridlington, in Yorkshire, sand and clay containing marine tertiary shells had been exposed on the coast. From an examination of the shells collected there by Mr. Bean, Mr. Lyell finds the deposit to agree in age with the Norwich Crag.

I cannot conclude these remarks without observing, that some part of the confusion and apparent inconsistency of the opinions of different conchologists, respecting the age of the Crag, must have arisen from the intermixture of fossils derived equally from the Norfolk and Suffolk beds, or from strata, some of which now turn out to be referable to the Older Pliocene, others to the Miocene period.

From an examination of some fossil shells, identical with recent species collected by Capt. Bayfield from the most modern deposit near the Gulf of St. Lawrence, and near Quebec, Mr. Lyell infers, that the climate of Canada was colder than now during the era immediately antecedent to our own times. The shells, which were determined by Dr. Beck, differ in great part from those now living in the Gulf of St. Lawrence, agree more nearly with arctic genera and species, and resemble those which Mr. Lyell collected at Uddevalla, in Sweden; whereas, if the living shells most abundant in the Swedish and Canadian seas are contrasted, they differ almost entirely. From notes sent by Capt. Bayfield, it appears that at different depths in the stratified sand and clay containing the fossil shells, near Quebec, insulated boulders are numerous, which, it is presumed, have been brought down at distant intervals by drift ice, and have dropped to the bottom of the sea as the ice melted.

While Mr. Lyell, by the aid of Dr. Beck's determination of fossils, had adopted these views respecting the climate of Canada, Mr. James Smith, of Jordan Hill, had been led by independent observations to a similar conclusion respecting the climate of Scotland during the Newer Pliocene era, arguing from the arctic character of the Testacea found in the raised beds of the valley of the Clyde, and other localities. In the first of two papers communicated by this author, he regarded all the deposits abounding in recent shells in Scotland and Ireland as belonging to one group; but in his second memoir he contends that there are two distinct formations on the Clyde, in the older of which there are from ten to fifteen per cent. of extinct or unknown species of shells, which he refers to the Newer Pliocene system of Lyell; whereas all the species found in the newer, which he calls Post-tertiary, exist also in the present seas. During this Post-tertiary period, which is considered to have been anterior to the human epoch, an elevation of at least forty feet took place on the shores of the Clyde. Mr. Smith affirms that the Till, or unstratified accumulation of clay and boulders, belongs not to the Post-tertiary, but to the older Pliocene division.

IGNEOUS ROCKS.

The principal communication we have received on rocks of igneous origin has been from our Secretary, Mr. W. I. Hamilton, who has read an interesting paper on the north-west part of Asia Minor, from the Peninsula of Cyzicus to Koola, with a description of the Katakekaumene. Between Cyzicus and Koola the principal stratified rocks are schist, with saccharine marble, compact limestone resembling the scaglia of Italy and Greece, tertiary sandstones, and tertiary limestones. The igneous rocks are granite, peperite, trachyte and basalt. The tertiary limestones are referred to the great lacustrine formation which occupies so large a part of Asia Minor. Hot springs burst forth near Singerli from a porphyritic trap rock. The Katakekaumene is a volcanic region, extending about seven miles from north to south, and from eighteen to nineteen east and west. It presents two systems of volcanic craters and coulées: the older of them are placed on parallel ridges of gneiss and mica slate, and the newer in the intervening valleys; hence he argues, that when the latter eruptions took place, the lines of least resistance to subterraneous expansion were in the valleys. The streams of lava from the more recent cones are bare and rugged, like the coulées in central France. Three periods of eruption are traced: the first, having produced basalt, which caps the plains of white limestone, and was ejected before the formation of the valleys; the second, marked by currents of lava from the more ancient system of volcanos in action since the formation of the valleys; the third resembling the coulées of Etna and Vesuvius, and mentioned by Strabo, but of which there is no historical tradition as to the period when they were in activity.

We have a notice by the Rev. W. B. Clarke of a shower of ashes that fell on board the Roxburgh off the Cape de Verd islands in February, 1839, the cause of which was not apparent. The sails were covered with a fine powder, resembling the ashes of Vesuvius, which was probably derived from an eruption in the Cape de Verd group.

PALÆONTOLOGY.

In the department of ***Palæontology*** Prof. Owen has, during the past year, contributed many papers, with his usual zeal and ability, to the elucidation of this most essential and perhaps most generally interesting branch of our subject. At the head of these we must place his determination of a tooth and part of a jaw of a fossil monkey, of the genus *macacus*, with part of the jaw of an opossum, and the tooth of a bat, in Eocene strata of the English tertiary formation. These remains were found at Kingston, near Woodbridge in Suffolk, by Mr. Colchester, in strata which Mr. Lyell has referred to the London clay; thus proving the existence of quadrumanous, marsupial, and cheiropterous animals in this country during the Eocene period. We have now evidence of fossil Quadrumana in the tertiary formations, not only of India and Brazil, but also of France and England; respecting which Mr. Owen has observed, that they appear under four of the existing modifications of the quadrumanous type: viz. the tailless ape (*Hylobates*), found fossil in the South of France; the gentle vegetable-feeding *Semnopithecus*, found fossil in India; the more petulant and omnivorous *Macacus*, found in Norfolk; and the platyrrhine *Callithrix*, found in Brazil. This genus is peculiar to America, and its extinct species is of more than double the stature of any that exists at the present day This geographical distribution of Quadrumana adds further weight to the arguments derived from the tropical aspect of vegetable remains that abound in the London clay at Sheppy, showing that great heat prevailed in the European part of the world, as well as in India and South America, during the Eocene period.

The probability of high temperature is further corroborated by Mr. Owen's recent recognition of four petrified portions of a large serpent (*Palæophis Toliapicus*), eleven feet long, and in several points resembling a boa, or python; and also of a bird allied to the vultures (*Lithornis vulturinus*), all from the London clay of the Isle of Sheppy; wherein the occurrence of fossil Crocodilians and Testudinata, and of fossil fruits, having a tropical aspect, allied to cocoa-nuts and many other fruits of palms, has been long known. Can we account for these curious facts without supposing that at the Eocene period of the tertiary epoch, the very clay on which London now stands was in the condition of a nascent spice-island, its shores covered with basking reptiles, and the adjacent lands waving with cardomums and palms, and thuias and cypresses, with monkeys vaulting and gamboling upon their branches, and gigantic serpents entwined around their trunks; the seas also swarming with sting-rays and saw-fishes, with chimæras and enormous sharks? for all these together with countless shells of pearly nautili occur among the fossil remains of the numerous extinct species of fishes, which, during the early ages of the tertiary period, crowded the tepid seas of our now humid and chilling climate.

Mr. Owen has also determined the character of a new genus of Pachydermatous animal (*Hyracotherium*) intermediate between the Hyrax, hog, and Chæropotamus, found in the London clay at Herne Bay, near Margate, by Mr. Richardson.

Mr. Lyell having submitted to Mr. Owen some fossil teeth from the Red Crag of Newbourne in Suffolk, they proved to be referrible to the leopard, bear, hog, and a large kind of deer, and afford the first example of mammalian remains being found in England in any of those divisions of the Crag which Mr. Lyell, in a paper already alluded to, has ascribed to the Miocene period; these genera are known to occur in the Miocene formations of France and Germany. The numerous Mammalia in the fluvio-marine crag of Norwich, are decidedly of a later date; among these Mr. Lyell enumerates the teeth and jaw of *Mastodon longirostris*, a tusk of an elephant with serpulæ attached, and bones of a horse, hog, and field-mouse; there occur bones of birds, many fishes, and numerous shells, partly marine, and partly freshwater and terrestrial.

The recent discoveries in Brazil by Dr. Lund of extinct Mammalia, that probably lived in some late portion of the tertiary epochs, form a new and important chapter in Palæontology. The largest of these are referrible to more gigantic forms than at present exist of families now peculiar to South America—*e. g.* to Sloths and Armadillos; just as most of the fossil mammalia of New Holland belong to families and genera which are still peculiar to that country.* In a paper on one of these animals from Buenos Ayres, Mr. Owen has shown that the bony armour, which several authors have referred to the Megatherium, belongs to the Glyptodon, an animal allied to the Armadillo, and of which a head containing teeth, and attached to a tessellated bony covering of the body and tail, resembling those of an Armadillo, has been lately found near Buenos Ayres, and is figured by Sir Woodbine Parish in his interesting work on that country, 1838.

The Glyptodon differed from the Megatherium in the structure and number of the teeth, and from all known Armadillos in the form of the lower jaw, and the presence of a long process descending from the zygoma; and approached in both these respects to the Megatherium. The teeth differ from those of Armadillos, in having two deep grooves both on the outer and inner surface, are more complex than those of any known Edentate, and indicate a passage from that family into the Toxodon. The ungueal phalanges are wholly unlike those of the Megatherium, and most nearly resemble those of Dasypus, but are short broad and flat, and seem to have been covered with hoof-like claws. The form of the foot most nearly resembled that of the fore foot of the Mole. Having ap-

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[* On this subject see L. & E. Phil. Mag. vol. x. p. 405; vol. xi. p. 208; vol. xii. p. 516.—Edit.]

propriated to the Glyptodon the armour supposed to belong to the Megatherium, Mr. Owen next proves that the latter animal was unprovided with any such bony covering, arguing from a comparison of its vertebral column and pelvis with that of the Armadillo; and from the absence of the oblique processes, which in the loricated Edentata resemble as to form and use the *tie-bearers* in carpentry, that support the weight of a roof. The vertebral conditions of the Megatherium are nearer to those of the Sloths and Ant-eaters. We have accounts of twelve skeletons of Megatherium, not one of which was found to be accompanied by bony armour. Cuvier considered the Megatherium more nearly allied to the Ant-eaters and Sloths than to the Armadillos.

Captain Martin has found that many parts of the bottom of the English Channel and German Ocean contain in deep water the bones and tusks of Elephants. They have been dredged up between Boulogne and Dungeness, in the mid-sea between Dover and Calais, and at the back of the Goodwin Sands; also mid way between Yarmouth and the coast of Holland. In 1837 a fisherman enclosed in his net a vast mass of bones between the two shoals called Varn and Ridge, that form a line of submarine chalk-hills between Dover and Calais. Captain Martin says these bones do not occur on the top of banks or shoals, but in deep hollows or marine valleys. Sir John Trevelyan possesses the molars of a large Elephant from gravel in the bed of the Severn, near Watchet, and we have long known that the bones of Elephants occur in great abundance in the oyster grounds off Yarmouth.

In subterranean Ornithology three important discoveries have been made during the past year; the first in the Eocene formation by Professor Owen, who has recognised the fossil Vulture before alluded to in the London clay of Sheppy; the second, by Lord Cole and Sir P. Egerton, who have acquired from the chalk of Kent the humerus of a bird most like that of an Albatross, but of larger and longer dimensions; the third by Professor Agassiz, who has found in Switzerland a nearly entire skeleton of a small bird (not unlike a Swallow), at Glaris, in the indurated blue slate beds of the lower region of the chalk formation. We know that the bones of a Wader, larger than a Heron, have been found by Mr. Mantell in the Wealden formation of Tilgate Forest; and that the Ornithichnites in the New Red Sandstone of Connecticut have been referred to seven species of birds.

We have an interesting accession to our knowledge of the anatomy of the Ichthyosaurus in Mr. Owen's description of the hinder fin of an *Ichthyosaurus communis*, discovered at Barrow-on-Soar by Sir Philip Egerton; this fin distinctly exhibits on its posterior margin the remains of cartilaginous rays that bifurcate as they approach the edge of the fin, showing in this respect a new approximation to the fin of a fish, and more fully justifying the propriety of the name Ichthyosaurus. Traces are also preserved of scutiform compartments on the integument of the fin. It is singular that this structure should never have been observed in any of the numerous specimens from Dorset and Somerset that have come under our notice; whilst at Barrow-on-Soar, from whence the paddle in question was derived, even the fibres of the skin and folds of the epidermis are sometimes accurately retained*.

Mr. Owen's first part of his report on fossil Saurians, read at the British Association at Birmingham in August last, forms the commencement of a most important addition to the history of extinct reptiles. His recent investigations in Odontography have also supplied to the geologist a new and most efficient instrument of investigation, enabling him to distinguish genera of extinct animals by the microscopic structure of their teeth; and as, of all fossil remains, the teeth are the parts most perfectly preserved, and in the case of cartilaginous fishes the teeth and spines are generally the only parts that have escaped decomposition, this method assumes an especial importance in fossil Ichthyology, as affording exact characteristics of animals long swept from the surface of the earth, and whose very bones have been obliterated from among the fossil witnesses of the early conditions of life upon our planet. By this microscopic test applied to the family of Sharks, Mr. Owen has confirmed the views of Agassiz respecting the affinities between the living Cestracion and the extinct genera Acrodus, Ptychodus, Psammodus, Hybodus, Cochliodus; in the case of animals also of the higher orders, he has settled the much-disputed places of several extinct gigantic Mammalia by the same unerring test. Thus he has shown the supposed reptile Basilosaurus to be a Cetaceous mammifer, allied to the Dugong; the Megatherium to be, as Cuvier had considered it, more nearly allied to the Sloth than to the Armadillo; and the Saurocephalus to be, as Agassiz had supposed it, an osseous fish.

Dr. Malcolmson, in a memoir on the Old Red Sandstone of the north of Scotland, has done important service in showing that the rocks composing that group are divided into three formations, the two lower of which are clearly distinguished from each other by their fossil fishes. The cornstone or central formation is charged with numerous remains of Ichthyolites, including *Holoptychus nobilissimus*, a new species of Cephalaspis, and other forms not yet described. The lower division, consisting in this region of conglomerates, shales and sandstone, is characterized by the genera Dipterus, Diplopterus, Cheiracanthus, &c., of Agassiz, as well as by the occurrence of a singular Ichthyolite, which seems to offer close analogies to certain forms of Crustacea. By help of these Ichthyolites, the author has been enabled to connect certain strata of Orkney and Caithness, and determine their relations to the beds of Old Red Sandstone containing fossil fishes in the basin of the Tay, and in the border counties of England and Wales, where they had been described by Mr. Murchison.

Mr. Williamson, in a notice on the fossil fishes of the coal-fields of York and Lancaster, says that these coal measures are very rich in Ichthyolites, which abound so much at Middleton colliery, near Leeds, that the workmen have given to one bed the name of fish coal; they are usually in fine bituminous shale above and below the coal, and most frequent in the roof immediately above it, where, as at Burdie House, near Edinburgh, there is a thin seam of coprolitic matter; they are rarely mixed with any great quantity of vegetable remains. In the lower measures of Lancashire they are associated with Goniatites and Pectens, and in the higher measures of Lancashire and Yorkshire with freshwater shells allied to Unio, and with Entomostraca. Exact observations as to facts of this kind are of inestimable importance, for it is only by careful induction from a sufficient number of such-like phænomena, and from similar details as to the local distribution and condition of animal and vegetable remains in the marine and fluvio-marine and lacustrine deposits which compose the carboniferous series, that we shall arrive at a solution of the grand problem of the formation of coal.

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* See Buckland's Bridgewater Treatise, Pl. 10.

CRUSTACEANS.

The Rev. T. B. Brodie has discovered in the Wealden formation near Dinton, in the vale of Wardour, the remains of Coleopterous and Hymenopterous insects, and a new genus of *Isopodous Crustacea* in the family Cymothoidæ. The Isopods are clustered densely together; the lenses in their eyes are sometimes preserved; there are also traces of legs, but of no antennæ. With them he has found a large species of Cypris. The insects are chiefly small Coleoptera; there are several species of Dipterous, and one Homopterous insect, and the wing of a Libellula. Mr. Brodie's discovery is the first yet made of insects in the Wealden formation, and also the first example, in a secondary formation, of Isopods that approximate in form to the Trilobites of the Transition series.

WORMS.

An addition has been made to fossil Helminthology by Mr. Atkinson of Newcastle-on-Tyne, who has found in slabs of micaceous slaty sandstone, from the carbonaceous series near Haltwhistle, tortuous casts of vermiform bodies of various sizes, some almost an inch in diameter, and several feet in length; the surface of many of these is thickly marked by transverse rings and a longitudinal groove, similar to those in the largest recent marine sand worms, *e. g.* the *Leodice gigantea.* The integument of some of these worms containing *chitine*, like the covering of insects, seems to have endured long enough to fix impressions of the transverse rings upon the sand; and the habit of swallowing large quantities of earth and sand, which we observe in many recent worms, may explain the presence of the large portion of sand, now indurated to stone, which occupies the interior of the impression of the skin. Since many casts are found upon the same slab, these worms must have been very numerous at the bottom of the sea, when the sandstone was in process of formation. Similar impressions of Annelids on the Cambrian rocks are figured by Mr. Murchison in Pl. 27 of his great work on the Silurian System.

ICHNOLOGY.

About twelve years ago we witnessed the creation of a new department in geological investigations, viz. the science of Ichnology, founded on the evidence of footsteps made by the feet of animals upon the ancient strata of the earth; this new method commenced with the recognition of the footmarks of reptiles on the New Red Sandstone near Dumfries, and not long after (1834) was followed by most curious and unexpected discoveries in Saxony and America. The Chirotherium of Hessberg and Ornithichnites of Connecticut were among its early results. Our own country has during the last two years been abundantly productive of similar appearances in many localities.

In recent excavations for making a dock at Pembray, near Llanelly, in Pembrokeshire, tracks of deer and of large oxen have been found on clay subjacent to a bed of peat, the lower peat being moulded into the footsteps; similar impressions were also found upon the upper surface of the peat beneath a bed of silt, and bones both of deer and oxen in the peat itself. Footmarks of deer have been also noticed in Mr. Talbot's excavations for a harbour near Margam burrows on the east of Neath.

Near Liverpool Mr. Cunningham has successfully continued his researches begun in 1838, respecting the footsteps of Chirotherium and other animals in the New Red Sandstone at Storeton Hill, on the west side of the Mersey. These footsteps occur on five consecutive beds of clay in the same quarry, the clay beds are very thin, and having received the impressions of the feet, afforded a series of moulds in which casts were taken by the succeeding deposits of sand, now converted into sandstone. The casts of the feet are salient in high relief on the lower surfaces of the beds of sandstone, giving exact models of the feet and toes and claws of these mysterious animals, of which scarcely a single bone or tooth has yet been found, although we are assured by the evidence before us of the certainty of their existence at the time when the New Red Sandstone was in process of deposition.

Further discoveries of the footsteps of Chirotherium and five or six smaller reptiles in the New Red Sandstone of Cheshire, Warwickshire and Salop, have been brought before us by Sir P. Egerton, Mr. I. Taylor, jun., Mr. Strickland, and Dr. Ward.

Mr. Cunningham, in a sequel to his paper on the footmarks at Storeton, has described impressions on the same slabs with them, derived from drops of rain that fell upon thin laminæ of clay interposed between the beds of sand. The clay impressed with these prints of rain drops acted as a mould, which transferred the form of every drop to the lower surface of the next bed of sand deposited upon it, so that entire surfaces of several strata in the same quarry are respectively covered with moulds and casts of drops of rain that fell whilst these strata were in process of formation.

On the surface of one stratum at Storeton, impressed with large footmarks of a Chirotherium, the depth of the holes formed by the rain drops on different parts of the same footstep has varied with the unequal amount of pressure on the clay and sand, by the salient cushions and retiring hollows of the creature's foot; and from the constancy of this phænomenon upon an entire series of footmarks in a long continuous track, we know that this rain fell after the animal had passed. The equable size of the casts of large drops that cover the entire surface of the slab, except in the parts impressed by the cushions of the feet, record the falling of a shower of heavy drops on the day in which this huge animal had marched along the ancient strand; hemispherical impressions of small drops, upon another stratum, show it to have been exposed to only a sprinkling of gentle rain that fell at a moment of calm.

In one small slab of New Red Sandstone found by Dr. Ward near Shrewsbury, we have a combination of proofs as to meteoric, hydrostatic, and locomotive phænomena, which occurred at a time incalculably remote, in the atmosphere, the water, and the movements of animals, and from which we infer with the certainty of cumulative circumstantial evidence, the direction of the wind, the depth and course of the water, and the quarter towards which the animals were passing; the latter is indicated by the direction of the footsteps which form their tracks; the size and curvatures of the ripple-marks on the sand, now converted to sandstone, show the depth and direction of the current; the oblique impressions of the rain drops register the point from which the wind was blowing, at or about the time when the animals were passing.

Demonstrations founded solely upon this kind of circumstantial evidence were duly appreciated, and are well exemplified, by the acute author of the story of Zadig; who from marks he had noticed on the sand, of its long ears, and teats, and tail, and from irregular impressions of the feet, declared the size and sex, recent parturition and lameness of a bitch he had never seen; and who from the sweeping of the sand, and marks of horse-shoe nails, and a streak of silver on a pebble that lay at the bottom of a single footstep, and of gold upon a rock against which the animal had struck its bridle, inferred that a horse, of whose existence he had no other evidence, had recently passed along the shore, having a long switch tail, and shod with silver, with one nail wanting upon one shoe, and having a bridle studded with gold of twenty carats value.

In addition to the commencement of Mr. Bowerbank's publication on the Fossil Fruits and Seeds of the London Clay, before alluded to, we have hailed with satisfaction the announcement, by Professor Henslow and Mr. Hutton, of their intended continuation of the Fossil Flora of Great Britain, conducted for some years by Dr. Lindley and Mr. Hutton, and lately suspended.

A Dictionary of the terms and language of geology has long been a desideratum to young students, to whose early progress the technical terms of the science have hitherto presented formidable impediments. This want has been recently supplied by two publications of this kind, one by Mr. George Roberts, author of the History of Lyme Regis; the other by Dr. Humble.

During the last year the Society has received no communication on Mineralogy; and almost the only volume that has been published in England on this much-neglected subject, has been a small but highly elaborate treatise on Crystallography by Professor Miller, of the University of Cambridge. In this treatise the author has adopted the crystallographic notation proposed by Professor Whewell in his paper on a General Method of calculating the Angles of Crystals, and the laws according to which they are formed, published in the Transactions of the Royal Society of London, 1825; and Professor Naumann's method of indicating the positions of the faces of a crystal by the points in which radii, drawn perpendicular to the faces, meet the surface of a sphere. The expressions which have been thus obtained are remarkable for their symmetry and simplicity, and are all adapted to logarithmic computation, and for the most part new.

NOTICE OF DECEASED MEMBERS.

In proceeding to speak of the losses which, during the past year our science has sustained by death, I shall offer my first tribute of respect to the memory of one, whom a predecessor of mine in this chair has justly called the father of English geology; since to his discoveries we owe the first diffusion of exact knowledge as to the order of superposition of the secondary formations which occupy so large a portion of our island, and the first demonstration of that constancy of the organic remains, which he proved to be characteristic of the component strata of each different formation. It was the especial merit of Mr. WILLIAM SMITH to establish a series of types of these groups, many of which have been adopted as classical, in such a manner as will perpetuate his name among the original discoverers of the age in which he lived.

If, as it has been truly said, the honour of the first discoveries in tertiary geology belongs to France, where the labours of Cuvier and Brongniart gave to this great division of the strata of the earth a systematic arrangement before unknown, so the establishment of the types in secondary geology, from the chalk down to the new red sandstone, is due to England; and the discovery of the leading natural divisions of that important portion of them which constitutes the oolite formations, was almost exclusively the work of Mr. William Smith.

His earliest publication was a treatise on irrigation, 1806, a subject on which his experiments gained him a medal from the Society of Arts.

In 1801 he printed proposals for publishing accurate delineations and descriptions of the natural order of the various strata that are found in different parts of England and Wales, to be illustrated by a small geological map*. This work was never completed, but it led to the publication of his large map, in 1815, for which the Society of Arts awarded him their medal and a premium of £50. In the same year also his stratigraphical collection of organic remains was purchased for the British Museum; this collection having formed the basis of his two separate volumes, entitled "Strata identified by their Organized Fossils," 1815, and "a Stratigraphical System of Organized Fossils," 4to, 1817.

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* The original coloured copy of this map, dated 1801, was presented by Mr. Smith to our Society, and is now in the Museum.

During the six years which followed the publication of his map of England, he put forth twenty geological maps of English counties on a larger scale, and several coloured sections across the south of England, and a general Geological Section of England and Wales, from London to Snowdon.

Among his unpublished papers were found unfinished and in part printed, an introductory work on geology, and preparations for a volume on Œconomic Geology, both illustrating the originality of his views.

Mr. William Smith entered on the field of his honourable exertions as a Civil Engineer and Mineral Surveyor at a time when his labours in geology were but little appreciated, and almost solitary. Amidst difficulties and discouragements, and at intervals snatched from the duties of a laborious profession, he accomplished the gigantic work of a general mineralogical survey of England, founded almost entirely on his own personal observations, which he ultimately recorded in a map of fifteen coloured sheets, published by subscription in 1815.

Inevitable delays retarded the appearance of this work nearly to the time when a more detailed and perfect map, by a distinguished president of this Society, eclipsed in some degree the fame which would have accrued to its author had it been published earlier, even in the less perfect form to which he had advanced it some years before. The sense entertained by this Society of the value of the scientific services of Mr. Smith, was marked by their award to him of their first Wollaston Medal, in 1831; and was accompanied by the just and eloquent eulogium pronounced on that occasion by Professor Sedgwick. In the same year also the British Association assembled at York made successful application to government for a pension, which was settled upon Mr. Smith for life; and at the meeting of this Association at Dublin, 1835, the University conferred on him the honorary degree of Doctor of Civil Law.

Mr. Smith was one of those remarkable persons whom strong natural sense and acute powers of observation occasionally enable to triumph over the disadvantages of a defective education. His attention was first called to physical inquiries, by the observing, when a boy, that a large stone which he was lifting under water in search of eels, could be moved with much more ease, than if the same stone had been on land. His juvenile curiosity was excited to learn the cause of an occurrence so surprising to him; and this first step led him, at the age of eighteen, to enter the profession of a surveyor and civil engineer. His early professional occupations from the year 1791 to 1799, whilst surveying collieries, constructing a part of the Somerset coal canal near Bath, and preparing reports respecting a supply of water for the Kennet and Avon Canal, and the trade it was likely to derive from carriage of stone and coal, &c., placed him in daily contact with geological phænomena especially calculated to illustrate the order of superposition of the English strata, and laid the foundation of his future discoveries.

By carefully noting the characters of the beds which he found in juxtaposition, and making comparative sections in various directions in the vicinity of Bath, he ascertained that an uniform order of succession pervades the groups exposed in the escarpments of the hills in that part of England, and that this uniformity is attended by a similarity in the organic remains of certain beds, which differ entirely from those of the groups above and below them; by diligently collecting and collating these remains, he drew the inference, that each group of strata contains extraneous fossils peculiar to itself.

His next step was to infer that the strata thus identified by himself in Somerset and Wiltshire were not of insulated and local occurrence, but formed parts of the great system of deposits extending over England; and thus, after many years of intense labour and continual travel, he succeeded in extending the principles first caught sight of in the neighbourhood of Bath, into that philosophical generalization which became the basis of his geological map of England.

Before Mr. Smith had quitted his occupations in Somerset and his residence at Bath, he indicated on a coloured map the geological structure of that neighbourhood. This document, dated 1799, is in the museum of our Society. He had also arranged his collections of rocks and their organic remains in the order of succession and continuity of the several strata; but neglecting to appropriate to himself the merit of these discoveries by immediate publication, he liberally imparted a knowledge of each, as it gradually arose, to his private friends, through whose oral communications they obtained such general currency, that their real author was frequently lost sight of or unknown. I was myself indebted to Mr. Smith, though at that time a stranger to me, for my first knowledge of the order of succession in the oolitic series. This I derived from information imparted to me by the late Rev. B. Richardson of Farley Castle, who had himself acquired it from Mr. Smith. A tabular view of the superposition of the English strata, written by Mr. Richardson, from the dictation of Smith in 1799, at the house of the Rev. Joseph Townsend, in Bath, and since also presented to this Society, forms a documentary proof of the extent of his discoveries before the conclusion of the last century.

In 1817 he planned the beautiful museum of Scarborough, in which he employed his original and instructive method of representing, by sloping shelves passing one beneath another, the inclined position of the strata; each shelf bearing the fossils that are respectively characteristic of the stratum it is intended to represent.

These works of William Smith undoubtedly place him in the position of an original discoverer, who was the first to establish, on an enlarged basis of evidence, the important facts of constancy in the order of superposition, and continuity in the horizontal extension of the strata of this island; and to prove that each of these strata is characterized by organic remains peculiar to itself. But it must not be forgotten, that both in this country and on the continent, other investigators, many of them no doubt unknown to him, were simultaneously collecting similar evidence in support of this great physical generalization. It only enhances the value and confirms the accuracy of Mr. Smith's conclusions, that the results of other independent inquiries were found to be in perfect harmony with his own. It is known to all who are acquainted with the productions of the school of Freyberg, that Werner had pointed out the importance of petrifactions as affording a basis for the arrangement of geological formations, the same in principle, though confirmed by less extensive details, than those which Mr. Smith elicited from the oolitic series in England. Professor Jameson has expressly stated that Werner was aware that petrifactions are comparatively rare in the transition rocks, increasing in number in the newer series of that division, and becoming still more numerous in the Floetz formations: he had further remarked, that the animals of the earliest periods are of the lowest and most imperfect class, namely zoophytes; that in ascending through newer and newer formations, we meet with shells and fishes and marine plants, all different from any living animals and vegetables of the present earth; that in the newest formations we find the remains of existing genera with those of land animals and land plants.

Werner had also noted, in some detail, the order of succession of the strata of the Muschel-kalk of Germany, founding his divisions upon the changes he observed in the petrifactions it contains; and thus announcing the principle of making distinctions in strata upon the nature of their organic remains.

The same principle had been previously caught sight of and partially elaborated by Lehman in Germany, and by other observers in France, where its application to tertiary strata received the fullest demonstration, in the great discoveries of Cuvier and Brongniart within the basin of Paris. In our just admiration of our countryman, therefore, we must not lose sight of the merits of his contemporary labourers on the continent; and whilst we honour him as the father of English Geology, let us also pay just homage to those who had started before him in the same course, wherein it was his undisputed merit to have arrived first at the goal.

Mr. W. Smith was born on the oolite formation at Churchill, in the county of Oxford, in 1769. When a child he was in the habit of collecting Terebratulæ from the oolite rocks in the fields of his native village, which he used as substitutes for marbles.

As an engineer he was employed in works of irrigation and drainage in many parts of England; as well as in stopping out the sea from breaches through which it had invaded the marshes of Norfolk, 1806, 1807, &c., and in the draining off the water of Mismer lake in Suffolk into the sea. He was the engineer also of the Ouse navigation in Sussex. In 1809 he was engaged in the restoration of the hot springs at Bath. In 1821 he recommended to Col. Braddyl to search for coal (beneath the magnesian limestone) on an estate in which is now situated the great South Hetton Colliery. No colliery in Northumberland had been worked, at that time, under the magnesian limestone.

Mr. Smith's principles of drainage have been applied with much advantage near Bath, Woburn, and in Norfolk. Finding the town of Scarborough to be very ill supplied with water, he excavated in the interior of the hill of Falsgrave Moor, two or three miles distant, a subterranean reservoir, in which he collected, from streamlets percolating that hill, sufficient water for the permanent supply of the town*.

From his early days to the latest period of his life he tells us that he had the habit of looking on the ground†.

Mr. Smith's last public employment was in conjunction with Mr. De la Beche and Mr. Barry, in the Commission for reporting on the best building-stone for the new House of Commons‡. During the later years of his life he resided near Scarborough superintending the estates of Sir John Johnson at Hackness; and dying at Northampton, in August 1839, aged seventy-one, after a few days' illness, at the house of his friend Mr. Baker, the historian of Northamptonshire, on his way to the Meeting of the British Association at Birmingham, was interred in the church-yard near the west end of the beautiful Norman church of St. Peter, in Northampton, which stands on the Oolite formation. He had often expressed a wish to be buried in this formation, on which he was born and educated, and the history of which he had so much elucidated. A monument will be erected to his memory in St. Peter's Church by subscription of members of the Geological Society of London.

It was not the least of the services which have been rendered to our science by Mr. Smith, that he was during many years the geological preceptor of his accomplished nephew Mr. John Phillips, in whom he has bequeathed to us a pupil, who has shown, by publications of the highest order in various departments of Geology, the soundness of the instructions received from his affectionate uncle.

Mr. **Davies Gilbert** was one of the earliest members elected into this Society, at its formation in 1808. During two years he served as a Vice-President, and for six years was a member of our

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* An account of this curious work is published by himself in the Philosophical Magazine for June 1827.

† See a paper by himself on Quartz in Soils, published in Charlesworth's Magazine for July 1837.

‡ For more detailed accounts of the life of Mr. Smith, and of the amount and value of the services he rendered to Geology in England, I must refer to Dr. Fitton's masterly and candid investigation of this question in the Edinburgh Review, Vol. XXIX, p. 310, &c. [reprinted, with additions, in L. & E. Phil. Mag., vol. i. p. 147, &c.]; to Mr. Conybeare's Introduction to his Outlines of the Geology of England and Wales, 1822, p. 45; to the Address of Professor Sedgwick to this Society, 1831; and to a biographical notice by his nephew Professor John Phillips, in Charlesworth's Magazine of Natural History, New Series, 1839, p. 213.

Council; and though he communicated no papers, he took a lively interest in all our proceedings, and was ever prompt on all public occasions to promote the welfare and forward the great objects of our institution.

His paternal name was Giddy: he was descended in the line of both his parents from very respectable families in Cornwall, and on the maternal side of Davies, allied to the noble family of Sandys; in 1817 he assumed the name of Gilbert, on succeeding to the property of his wife's uncle, Mr. Charles Gilbert, of East Bourne, in Sussex.

Having been privately educated in Cornwall, he became, in 1785, at the age of eighteen, a gentleman-commoner of Pembroke College, Oxford, where, being of more studious habits and more mature attainments than is usual with students of his age, he associated chiefly with the senior members of his College. Dr. Parr, writing at this time to the late master of Pembroke, speaks of Mr. Giddy, then twenty-three years old, as "the Cornish philosopher," and adds, that "he deserves that name."

To this College, as well as to the University, his affectionate and devoted attachment endured to his latest hour, and he became on several occasions a liberal benefactor towards improvements in Pembroke and its vicinity. During many years it was his great delight to pass a few days at Oxford, and he always considered the diploma Degree of Doctor of Laws, conferred on him by the University in 1832, as one of the most gratifying events of his life.

During his early residence his taste for chemistry and other branches of physical science had introduced him to the acquaintance of Dr. Beddoes, at that time a resident Member of Pembroke College, and who subsequently dedicated to him his pamphlet on mathematical evidence. This acquaintance* was the remote cause of the first step in the public life of Sir Humphry Davy; when Mr. Giddy, who had discovered young Davy's genius for chemistry whilst yet a boy at Penzance, introduced him to Dr. Beddoes, to assist in his laboratory at Bristol, little dreaming that he should himself one day become the successor of this boy in the chair of the Royal Society.

Mr. Davies Giddy was elected a fellow of the Royal Society in 1791, and subsequently of the Antiquarian, Linnean, Geological, and Astronomical Societies of London. He was also an honorary member of the Royal Society of Edinburgh, the Royal Irish Academy, and of the New University of Durham. In 1814 he was elected first President of the Royal Geological Society of Cornwall, and afterwards Vice-Patron of the Cornwall Royal Polytechnic Society, in both which offices he continued till the day of his death. He held the distinguished office of President of the Royal Society, during three years, from 1827 to 1830, and contributed several important papers to their Transactions; one upon the Mathematical

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* [He also enjoyed the friendship of Dr. Priestley: and he was on his way to Birmingham on a visit to him, when, at a short distance from the town, he learnt that the residence of his venerable friend was in flames, and that a bigoted mob were in the act of destroying the library, manuscripts, and laboratory of that excellent man and distinguished philosopher.—Ed.]

Theory of Suspension Bridges (vol. 116, 1826, Part I., p. 202); also a Table for facilitating the Computations relative to Suspension Bridges (vol. 121, 1831, p. 341); a third paper, entitled Observations on Steam Engines (vol. 117, 1827, p. 25); and a fourth on the Efficacy of Steam Engines in Cornwall, with Investigations of the Methods best adapted for imparting great Angular Velocity (vol. 120, 1830, Part I., p. 121); likewise a paper on the nature of Negative and Imaginary Quantities (vol. 121, 1831, p. 91). He also printed three Addresses as President of the Royal Society, 1828, 1829, 1830*.

In 1804 he was returned to parliament for the borough of Helston; and in 1806 for Bodmin, which place he represented till 1832. During that time he was continually called on by the House of Commons to serve on committees of inquiry touching scientific and financial questions, on which latter subject he published a letter, entitled "A plain Statement of the Bullion Question." He was Chairman of the Committee for rebuilding London Bridge, which he caused to be widened ten feet. The rectification of the national standards of linear dimensions and capacities, was undertaken upon his motion for an address to the Crown.

In his native county also, his authority was continually appealed to on scientific questions, and calculations of practical importance in the machinery of mines and steam-engines; and he was ever ready on all occasions to devote his time and talents to the service of his friends and of the public. In 1792, on the occurrence of a riot in Cornwall, whilst he was a young man, holding the office of sheriff, there being no soldiers in the county, he performed, for the last time that such an event has occurred in England, the military duty of calling out the *posse-comitatus*.

Few persons excelled Mr. Gilbert in bringing the results of much contemplative study to bear on the business of life; his strong point lay in the application of high mathematical knowledge to practical purposes, and in calculating the amount of effective power to be derived from the use of mechanical forces, judiciously combined. For the exercise of this talent his beloved native county offered unusual opportunities; it also afforded him abundant materials for gratifying his taste for antiquarian researches; and the fruits of his labours as a biographer and local historian were presented to the public in 1838, in four 8vo vols.; this work is entitled The Parochial History of Cornwall, founded on the manuscript histories of Mr. Hals and Mr. Tonkin, with additions and various appendices by himself, and brings down the account of families and descent of property in that county from the death of those biographers, about the middle of the last century, to the present time. Mr. Gilbert's additions and criticisms form no small part of its value; he has introduced also copious scientific notices by Dr. Boase and other modern authors, relating to the geology of the county, a subject, he observes, of such recent origin, that the very word does not occur in Chambers's Encyclopædia printed in 1783. In acknowledgment of his indirect influence upon this science, I am bound to state with gratitude that my Bridgewater Treatise would never have existed, had not the appointment to write it been conferred upon me by Mr. Gilbert whilst President of the Royal Society.

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* Mr. Gilbert was also the author of the following papers in the Quarterly Journal of Science and the Arts: Observations on the properties of the Catenarian Curve with reference to Bridges by Suspension, vol. x. p. 230; On the Ventilation of Rooms, and the Ascent of Heated Gases through Flues, vol. xiii. p. 113; Investigation of the Methods used for approximating to the Roots of Affected Equations, vol. xiv. p. 353; Researches on the Vibrations of Heavy Bodies in Cycloidal and Circular Arches, vol. xv. p. 90; On the General Nature and Advantages of Wheels and Springs for Carriages, the Draft of Cattle, and the Form of Roads, vol. xviii. p. 95; On the Vibration of Heavy Bodies, vol. xx. p. 69.

Mr. Gilbert was an assiduous collector of ancient traditions, legendary tales, songs and carols, illustrating the manners, sports, and pastimes of the peasantry of Cornwall; and he was a writer of several anonymous letters and papers in the Gentleman's Magazine. He possessed great memory and powers of quotation and anecdote, enriched by vast stores of traditional information as to the personal history of many of the most distinguished individuals of his time, much of which will have perished with him. It has been truly said of him by a contemporary biographer, that "His most endearing talent was his power of conversation. It was not brilliant; it was something infinitely beyond and better than mere display; it was a continued stream of learning and philosophy, adapted with exquisite taste to the capacity of his auditory, and enlivened with anecdotes to which the most listless could not but listen and learn.

"His manners were most unaffected, child-like, gentle, and natural. As a friend, he was kind, considerate, forbearing, patient, and generous; and when the grave was closed over him, not one man, woman, or child, who was honoured with his acquaintance, but will feel that he has a friend less in the world. Enemies he can have left not a single one."

During the last twelve months his strength had been rapidly declining, but he retained full possession of his intellectual faculties till within a few hours of his death; he breathed his last in the bosom of his family at East Bourne, on the 24th of December last, in the seventy-third year of his age. An exact and admirable representation of his finely-formed head and intelligent countenance is preserved in a bust by Westmacott in the Hall of Pembroke College, Oxford.

Sir JOHN ST. AUBYN, who died during the last year, was one of the founders and early Vice-Presidents of the Geological Society, and was among its most firm and valuable friends and supporters at that perilous moment of its existence when the struggles and opposition which attended its first establishment had nearly crushed it in the bud; he was also a liberal contributor to the supplies at that time requisite for its advancement.

He subscribed largely also to the funds then raised for the publication of Count Bournon's crystallographic work on Carbonate of Lime, and for enabling Dr. Berger to undertake his tours in Cornwall, preparatory to his geological description of that county.

The meetings held for the purpose of forwarding Count Bournon's work by some of the most distinguished mineralogists of that day, when collections in geology were rare, was one of the steps that brought together our first founders: many of them were till then strangers to each other, and being thus accidentally introduced, they resolved from thenceforth to cooperate for the furtherance of objects in which they felt a common interest, and became the germ of the Geological Society.

Sir John St. Aubyn was at this time occupied, like his friends Sir Abraham Hume and Mr. Greville, in making large and costly additions to his cabinet of simple minerals, the nucleus of which consisted of the specimens he had purchased of Dr. Babington in the year 1799, and which are described by Babington in his catalogue (one vol. 4to) published in the same year. These specimens had previously been the property of Lord Bute.

The position of his seat at Clowance, in the centre of the greatest mining district of Cornwall, afforded facilities for acquiring the most choice productions of that great repository of mineralogical treasures, and of these facilities he assiduously availed himself during many years. His other seat on the picturesque granitic pinnacle of St. Michael's Mount in the bay of Penzance (the Ictis of Diodorus, from whence the Romans exported tin to Gaul), placed him in another position of high geological and mineralogical advantages; the granite veins that intersect the killas at the base of this classic mountain being among the first described and most instructive instances which Cornwall affords, of the important phænomena of the injection of granite into slate, and the metamorphic condition of the slate thence resulting; whilst a well-exposed tin vein at the base of the ancient fortress and monastery that crown this insulated mountain, affords specimens of Apatite, and is more richly studded with minute but perfect crystals of topaz than any other vein known to exist in this country. These easily accessible examples of phænomena, most highly interesting to the mineralogist and geologist, he carefully preserved for the inspection of the numerous visitors that are continually attracted to this spot—of threefold interest, to the antiquary, the artist, and the mineral philosopher.

A similar zeal for the preservation of interesting scientific objects induced Dr. Jenner to preserve, for the benefit of geological visitors, a rock which presented the rare phænomenon of organic remains intermixed with toad-stone, on the side of a trap dyke intersecting old red sandstone at Newport, near his residence at Berkeley.

To the nucleus formed by Dr. Babington's collection, Sir John St. Aubyn made large additions, not only from the productions of Cornwall, but also from foreign countries, particularly the mines of Germany and Hungary, many of which are no longer wrought. This collection was very rich in the ores of gold, silver, copper, and other metals, and particularly in native diamonds and gems. The arrangement of it was begun by Count Bournon, but subsequently completed after the system of Mohs.

In 1834 he presented the bulk of his collection to the Devonport Civil and Military Library, of which he had been annually appointed

President from its formation in 1827 until his death; and a collection of Duplicates to the museum of Saffron Walden, near which place he then resided. He was an active member of the Geological Society of Cornwall, and of many scientific institutions in London; had a knowledge of Chemistry, Conchology, and Botany; and was a patron of the fine arts and a collector during his whole life.

In Brigadier **Charles Silvertop** the Society has lost the author of many interesting communications to our Evening Meetings on the Geology of Spain, the mineral structure of which, notwithstanding its proximity to France and England, and the long-continued military operations of both these nations upon its territory, is less known than that of any other portion of civilized Europe.

The unhappy circumstances of the country have long abstracted the attention of the Spaniard from researches of science, and the difficulties of travelling in the midst of civil commotions have deterred even the enterprising spirit of neighbouring geologists from endeavouring to fill up the lamentable blank which Spain still presents upon the scientific map of Europe.

Brigadier Silvertop, though occupied in the professional engagement of arms, was not forgetful of the pursuits of science. He published the substance of his communications to this Society in a small volume, 1836, wherein he gives a sketch of the widely-disseminated deposits of tertiary beds in the provinces of Granada and Murcia, accompanied by a general view of the volcanic and other rocks of the same district, illustrated by sections, which represent the configuration of the ground, the relative height of the ridges, and the superposition of the strata. He died at Rennes, in June last, on his way to the Pyrenees and Italy.

Mr. **Louis Hunton** was the author of a paper printed in our Transactions on the Upper Lias and Marl-stone of Yorkshire, showing the limited vertical range of the species of Ammonites and other Testacea, and illustrating their value as geological tests. His observations are founded on the details of the section of Easington height, near Whitby.

Jens Esmark, Professor of Mineralogy in the University of Christiania, was one of the many disciples of the school of Freyberg, who imbibed from their master an enthusiastic devotion to his theories, which largely contributed to stimulate into activity that general spirit of geological inquiry, the expansion of which, during the present century, has produced such unexpected and extensive discoveries in the development of the structure of the earth.

In 1794, deeply imbued with the doctrines of Werner, he went to Vienna to prepare himself for a tour through Hungary; after this he remained some months at Chemnitz, and visited the other chief mining districts of Hungary, Transylvania, and the Bannat, and crossing the Carpathians to Wielitzka and Cracow, returned to Saxony by the mines of Tarnovitz in Silesia.

In 1798 he published, at Freyberg, the result of his observations, in a small octavo volume, giving descriptions of the mines he visited, and their respective productions, and expressing his conviction of the truth of Werner's opinion as to the Neptunian origin of the pumice and obsidian (even that of the Lipari Islands), as well as of trap and granite. A translation of his remarks on the Geological History of the Globe was published in the Edinburgh New Philosophical Journal (1827), vol. vi. p. 107. The most important portion of this paper consists in its bearing his evidence to show that the greater part of Norway has, at some period, been covered with ice, and that the granite blocks, so abundant in that country, have been brought to their present place by glaciers.

In 1829 Professor Esmark published a Tour in Norway*, containing many measurements of heights, and he was the first to measure the lofty mountain of Schneehätten. He also published various detached Memoirs on Mineralogy†.

He is said by Otte to have been the first discoverer of chromate of iron in Norway; and the Norwegian datolite, which was also discovered by him in 1806, was at that time named Esmarkite. He published a short notice on tellurium, in the 3rd vol., 1st series, of our Transactions.

His residence at Christiania, in the vicinity of iron, copper, and silver mines, and of the School of Mines and Agriculture at Kongsberg, gave full scope to his taste and talents, and also afforded occasion for the exercise of those courteous attentions which have, during many years, been gratefully acknowledged by scientific travellers in Norway.

He once came to England, and was a member of the Wernerian Natural History Society of Edinburgh.

He was an excellent chess-player; and in appearance, countenance, and the fine form of his head, resembled Mr. Davies Gilbert, whom it has been my painful duty to associate with him in the catalogue of the losses we have sustained during the last year.

Don Carlos de Gimbernat, Member of the Royal Academy of Sciences at Munich, was the son of a physician of Barcelona, and, from political motives left his native country at the commencement of the French Revolution for Paris, where he passed many years. He had previously studied at Freyberg under Werner, and visited England, where he became acquainted with Townsend, our Spanish traveller, and with Dr. Hope, of Edinburgh; giving to the physical sciences the attention usually required of students for the medical profession, and continuing to cultivate them in his later years. He was more particularly attached to Chemistry, Geology and Mineralogy, and analysed the waters of many hot mineral springs, and found azote in all. The medical virtues which he ascribed to these springs raised him high in the estimation of the Swiss.

M. Gimbernat published accounts of his discovery in the thermal waters of Aix in Savoy, Baden, and other warm springs in Switzerland, of a mucous organic substance, (formed, as he fancied, by chemical precipitation, from azote and carbonic acid,) which he thought was more nearly allied to animal than vegetable matter, and to which he gave the name of Zoogene; and he also supposed that he found the same substance in the thermal waters of Ischia, and in waters produced by the condensation of the steam disengaged from Vesuvius. A similar mucous substance, in the thermal sulphureous waters of Roussillon, was supposed by Professor Anglada, of Montpelier, to be a chemical product, from elements held in solution by the waters at the time they issued from the earth, and deposited by them in a flocculent form when they come in contact with the air. De Saussure, however, Decandolle, Dillwyn, and Daubeny*, founding their opinions on the structure it exhibits under the microscope, refer this gelatinous substance to minute Conferveæ; but the more recent discovery, by Ehrenberg, of infusorial animals in the warm springs of Bohemia, gives some probability to the supposition that these may be mixed with Confervæ in the so-called zoogene of Gimbernat. The decomposition either of Confervæ or of Infusoria would afford the azote found in zoogene; but their presence would transfer the origin of this organic substance from simple chemical agency to the instrumentality of organic life. On quitting Naples, in 1820, he retired to Switzerland, where he fell into bad health and reduced circumstances, and died at Geneva in 1839†.

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* Reis von Christiania nach Drontheim.

† In the Magazin for Naturvidenskaberne.

FREDERICK MOHS, Professor of Mineralogy in Vienna, was born at Gernrode, in the Harz Mountains, about 1770. He lost his father, a merchant, very early, and was expected to succeed him in the business; but his predilections for science, particularly for mathematics, had marked him out for higher destinies.

He began his studies, 1796-98, at Halle, and continued them in the mining institution at Freyberg.

We find him in 1802 at Vienna, occupied in describing the mineral cabinet of the banker Von der Nüll, where he first conceived those views which he afterwards developed in his system of mineralogy.

His fondness for geology and the art of mining induced him to visit Styria, Saltzburg, Carinthia, Carniola, Hungary and Transylvania, &c., and he received from the Austrian Government, in 1810, a commission to examine those parts of Passau, Austria, and Bohemia, where porcelain clay is found.

Having thus attracted the notice of the Archduke John, and undertaken a journey to Styria in 1811, he was nominated Professor of Mineralogy in the Johanneum, at Gratz.

In 1818 he visited England with Count Breüner, who had been his pupil at Gratz; his conferences at Edinburgh with Jameson, whom he had known at Freyberg, made a strong impression on the Professor, in favour of what he called the "natural-history-system" of mineralogy, which he in part adopted, and first made known to British mineralogists in 1820*, and afterwards more fully explained in 1821† and 1822‡.

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* On Organic Matter in Sulphureous Springs, Linn. Trans., London, vol. xvi. 1833.

† A short notice on Sulphate of Soda is published by Gimbernat in our Transactions, vol. ii., second series, p. 331.

On the death of Werner, in 1817, he was called to the chair of Mineralogy in the Mining Academy of Freyberg; but in 1826 went to reside at Vienna, as Professor of Mineralogy, and Superintendent of the Imperial Cabinet. In 1804 he published a volume of practical importance, containing "A Detailed Account, illustrated with a Ground Plan, of the Mines and Mining Operations at Himmelsfürst, near Freyberg." In this work he describes, not only the geological relations and mineral products of these mines, but gives full details as to the methods of working them; their buildings and machinery, ventilation and drainages, preparation of the ores, receipts, expenditure, &c.

His great work on Mineralogy, or "the Natural History of the Mineral Kingdom," is best known in this country by its translation, published at Edinburgh, with considerable additions, by his pupil, Mr. William Haidinger, in 1825, 3 vols. 8vo. In the method of arrangement proposed by Mohs in this work, he founds his classification solely on external resemblances and differences, and displays a most profound knowledge of all the productions of the mineral kingdom.

This devoted pupil, friend, and successor of Werner died in Italy, 20th September, 1839, at Agardo, near Belluno, having undertaken a tour into that country for the purpose of studying the phænomena of volcanos§. He was an honorary member of the Royal and Wernerian Societies of Edinburgh.

It has been said of Mohs, and may be said of many distinguished cultivators of this department of natural science, that he was too consummate a mineralogist to be a good geologist. The sustained attention to minute details, which is indispensable to the recognition of individual minerals, gives such a habit to the mind, that it cannot easily recoil from the state of tension, which is induced by the continual study of minutiæ, to that expanded condition which is essential to apprehend the magnificent generalizations of geology. For similar reasons, an extremely skilful delineator of botanic details would probably be incapable of expressing the grand and general features and effects of forest scenery, or landscape, from his habits of overstrained attention to the details of individual trees and plants that occupy the foreground of his picture.

Captain ALEXANDER GERARD, of the Bengal Native Infantry, was one of three brothers, all distinguished by their enterprising spirit, and zealous scientific researches in the Himalaya Mountains, the sons of Dr. Gilbert Gerard, who wrote the well-known "Institutes of Biblical Criticism," and grandsons of Dr. Alexander Gerard, author of works which have been translated into various European languages, and of a standard "Essay on Taste."

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* Third edition of his System of Mineralogy.

† Manual of Mineralogy.

‡ Encyclopædia Britannica.

§ His funeral was celebrated with much ceremony expressive of public respect, and attended by a long procession of miners, each bearing in his hand a burning torch.

Having been born at the University of Old Aberdeen, in which his father was Theological Professor, he had early imbibed a thirst for knowledge and for scientific pursuits; and at the age of sixteen he entered the military service of the East India Company. Having considerable abilities as a surveyor, and being desirous of travelling, he soon got an appointment, and was sent to survey the province of Malwa, where he prosecuted his instructions under a burning sun, with great accuracy and constancy of purpose. He procured at his own expense the most costly instruments, and undertook several surveys in the Himalaya Mountains, suffering every vicissitude of heat, cold, hunger, and all the ills which could beset a traveller, with a degree of cheerfulness which was remarkable; but a residence of thirty years in India, passed chiefly in the endurance of these hardships, laid the foundation of that decay of health, which has lately brought him to a premature grave.

Captain Alexander Gerard was well known in the East as a scientific traveller, having, in company with his brother, the late Dr. James Gerard, penetrated the Himalaya Mountains through several passes before unknown to Europeans. While contributing, by his maps, to benefit geographical science, he never lost sight of what was novel and interesting in the geology, botany, and zoology of these stupendous regions, and various occasional papers have appeared from his pen, comprising valuable information on these subjects. We owe to this enterprising officer and indefatigable barometrical observer, our first knowledge of the structure of that portion of the Himalaya Mountains which forms the upper region of the Valley of the Sutlej, and is chiefly primitive. In this northwest extremity of India, on the frontier of China, he ascended to the astonishing height of 19,411 feet, on the mountain Tahigang, the summit of which he estimated at 22,000 feet above the sea.

A small collection of geological specimens made by him has been recently laid before this Society; it was formed in the distric of Speetee, in Chinese Tartary, at the elevation of from 12,000 to 19,000 feet above the level of the sea, and between the latitudes 31° 30″ and 32° 30″ north, and longitude 77° and 79° east. On the confines of Chinese Tartary, at the height of 16,200 feet, he found a region of limestone containing Ammonites. The same shells occur nearly at the same height near the Niti and Manná Passes. In Thibet he observed millions of organic remains, lying at extraordinary altitudes, and forming vast and rocky cliffs. At the elevation of 17,000 feet were seen detached fragments of rocks, bearing the impression of shells, which must have been derived from still higher peaks; one clift was a mile in perpendicular height above the nearest level.

He first appears as the companion of Herbert in his survey of the course of the Sutlej, 1819. (Asiatic Researches, vol. xv. p. 339.) In the same vol. p. 469, he published observations on the climate of Subathu and Kotgerh. His labours in completing a geographical survey of the valley of the Sutlej are the subject of a paper by the late Mr. H. T. Colebrooke in the Transactions of the Asiatic

Society of London, vol. i. p. 343. From the diary of this survey Mr. Colebrooke selected notes of Geological observations; and from specimens then collected, duplicates were sent to our Society. Upon these notes, and on Captain Gerard's letters, written during his survey in the middle valley of the Sutlej, a sketch of the Geology of the Himalaya was prepared by Mr. Colebrooke and published in the Geological Transactions of London*.

The second volume of Sir W. Lloyd's recent narrative of a journey in the Himalaya, contains an account of Captain Gerard's attempt to penetrate on the north side of the Himalaya by Bekhur, to Garoo and the lake Manasarowara, near the source of the Sutlej. These letters are interspersed with many interesting geological observations respecting the mineral productions and nature of the rocks of the country over which he travelled. He found the inclination of the strata to be usually perpendicular to the direction of the range, presenting long continuous slopes on the side towards which they dip, and terminating abruptly in rugged precipices towards the axis of the mountain chain. Near Bekhur, at the north side of the Himalaya, on the margin of the great table land of Tartary, elevated 15,786 feet above the sea, he mentions the occurrence of gravel studded with Ammonites, not far from the Hookeo Pass, which presents mural precipices of limestone.

In one excursion in the Himalaya he fell in with the late Bishop Heber, who devotes a long and eloquent passage in his journal to the expression of his praise and admiration of the scientific talent and enterprising spirit of Captain Gerard. He was an excellent Persian scholar, and acquainted with several other oriental languages.

He performed many of his surveys under a burning sun, the thermometer ranging from 100 to upwards of 112 degrees. As many of his observations were required to be taken at mid-day, the consequences were frequent suffering and illness from strokes of the sun; but he continued his labours until his health totally failed. He died at Aberdeen in December last, at the age of forty-seven, having apparently sacrificed his life to the promotion of science, stimulated in his labours by the wish to benefit mankind, without the hope of worldly remuneration.

To his late equally zealous and indefatigable brother, Dr. James Gilbert Gerard, surgeon of the Hill corps stationed at Subathu, and the companion of Captain, now Sir Alexander, Burnes, in his perilous journey through Central Asia, we owe the discovery of extensive collections of fossil shells in the Himalaya mountains, at the height of 17,000 feet. The greater part of these closely resemble shells that occur in the Oolite formation of Europe, particularly Ammonites and Belemnites; whilst a few, *e.g.* Orthoceratites and Spirifers, are similar to shells we find in rocks of our Transition Series. The Rev. R. Everest has described and figured some of these in the eighteenth volume of the Asiatic Researches.

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* Vol. i., second series, p. 124.

His third brother, Captain Patrick Gerard, is remarkable as the author of a Meteorological Journal, kept in 1819–20 at Kotgerh, Subathu, and the intermediate places in the Himalaya mountains, and recording hourly observations during nearly two years*.

Feb. 26.—A paper was first read, entitled "Further observations on the fossil trees found on the Manchester and Bolton railway;" by John Hawkshaw, Esq., F.G.S.

Since Mr. Hawkshaw's former communication†, another fossil tree has been found on the opposite side of the railway. It is about three feet in height, and three feet in circumference, and stands on the same thin stratum of coal as those first discovered, and perpendicularly to the surface of the bed. Mr. Hawkshaw is, therefore, strengthened in his belief, that the trees grew in the position in which they are found.

After this notice of the recent discovery, he proceeds to describe the effects produced in hot and moist climates on felled or prostrated solid dicotyledonous trees. The tropical forests with which he is acquainted from personal examination, are situated in Venezuela on the shore of the Carribean sea, and between the 8th and 10th degrees of north latitude, and the 65th and 70th of west longitude. In these forests a few months are sufficient to destroy the interior of the largest tree, little more being left than an outer shell, consisting chiefly of the bark. Mr. Hawkshaw noticed this peculiarity more frequently in dicotyledonous trees, having a proper bark, than in monocotyledonous vegetation, excluding necessarily those always hollow; and he does not remember to have seen a single instance of a palm similarly acted upon. Sometimes the portion of the dicotyledonous tree remaining on the ground, presented very much the appearance of the founder's mould, when the pattern has been withdrawn from the sand, and before the metal has been run in; and by this kind of decay, a cavity is formed from which a fac simile of the tree might be cast. In other cases, prostrated trunks having the appearance of being solid, have yielded to the pressure of his feet, and proved to be only hollow tubes. Dangerous accidents have also occurred from temporary bridges constructed of dicotyledonous trees having given way beneath the passenger, though there was no outward indication of decay. The bark of these trees had changed but little, though nothing of the interior remained but dust, and a few remnants which crumbled beneath the slightest touch.

The low and flat tracts in which this destructive operation goes on most rapidly, are those in which, from the deep rich soil and excessive moisture, all below the tall forest trees and larger palms is occupied by canes, bamboos, and minor palms. Such tracts would be most easily submerged; and in Mr. Hawkshaw's opinion they might hereafter present a seam of coal, which would afford but few distinct traces of palms and forest trees. These phænomena, he says, may explain in part, why so few distinct forms remain of the numberless forest trees, which must have formed a portion of the vegetable kingdom, at the time of the accumulation of our coal deposits.

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* See Journal of Asiatic Society of Bengal, vol. ii. p. 615.

† L. & E. Phil. Mag. vol. xv. p. 539.

Mr. Hawkshaw does not attempt to explain the process by which dicotyledonous trees are rendered hollow in tropical forests. He expresses doubts respecting the probable nature of the Calamites of the coal measures, and offers no explanation of the means by which they have been preserved in so great abundance. If the coal be considered as the debris of a forest, he says, it is difficult to account for not finding more trunks of trees than have been discovered in our coal basins; and he observes, it is only perhaps by allowing the original of our coal seams to have been a combination of vegetable matter, analogous to peat, that the difficulty can be solved. In this case, he is of opinion, but a few isolated trees might be expected to be found, and that the remains of vegetable forms most frequently discovered, would only be confirmative of the antiseptic qualities of their original nature, as previously advanced by Professor Lindley, and not of the number or importance of their particular genera at the time of their deposit.

In conclusion, Mr. Hawkshaw says, that whatever opinion may be drawn from what is conjectural in his paper, it will be obvious, that though fossil remains may be found filled with a mechanical deposit, and containing traces of other vegetables, yet that this condition does not prove, that the plants were originally hollow, nor even render it the most likely hypothesis, as they may have been hard wood-trees, the centre of which had been removed by natural processes.

LXXVI. *Intelligence and Miscellaneous Articles.*

BLUE OXIDE OF TITANIUM.

M. KERSTEN has shown that oxide of titanium may be prepared containing less oxygen than titanic acid, and that this blue substance, which has hitherto been prepared only in the moist way, may be obtained in the dry way, and in several modes:

1st. By passing the vapour of zinc over titanic acid heated to whiteness; the acid then assumes a dirty blue colour, but loses it, and becomes again white when exposed to oxygen at a high temperature.

2ndly. By putting into a porcelain crucible, metallic zinc, or oxide of zinc mixed with charcoal, and then covering the whole with titanic acid or a titaniferous earthy silicate, and heating the crucible for several hours to whiteness. Fused masses of a lavender blue colour are thus obtained; they are opake, and contain blue oxide of titanium and oxide of zinc.

3rdly. By fusing at a white heat, and out of contact of the air several titaniferous compounds, as, for example, the titaniferous silicates of lime with iron. These silicates, which are at first colourless, become more or less blue.

4thly. By treating the same titaniferous silicates in the same manner with metallic tin. A slight addition of charcoal in powder appears to favour the formation of blue glass.

5thly. M. Kersten found that titanic acid might, under certain circumstances, be reduced, by means of hydrogen, to the state of blue oxide of a very fine colour. If into biphosphate of soda, kept in fusion in a porcelain crucible, fine titanic acid be projected, it dissolves readily at a low temperature, and a white, transparent saline mass is formed. If this mass be put into a bulb of difficultly fusible glass, and hydrogen gas which has been dried by chloride of calcium be passed over it, the saline mass soon becomes of a fine lavender blue at the surface; if the residue be afterwards dissolved in water, the phosphate dissolves, and oxide of titanium of a fine blue colour remains; this oxide was left in contact with water during two months, without producing any diminution of its fine colour; but when heated to redness in contact with the air, it loses its colour, and is converted into titanic acid. This blue compound is not acted upon by hydrochloric acid at common temperatures; but when heated with this acid it loses its colour, and is converted into titanic acid. Zinc, tin, and iron immersed into this solution, reproduce the blue oxide.

6thly. If pure zinc be placed at the bottom of a porcelain crucible, and then above it a mixture of titanic acid and biphosphate of soda, or what is still better, if these substances fused together, be strongly heated in a crucible for several hours, a blue saline mass is obtained, sometimes of a violet tint, which dissolves in water, leaving a residue, the blue substance, mixed with a little oxide of zinc, which is not dissolved by the alkaline phosphate.

7thly. If instead of zinc, tin or iron be employed, the results are similar; but by the two last methods the blue oxide obtained is, however, not of so fine a colour as when the titanic acid is reduced by hydrogen.

CHELIDONIA AND PIRROPINA.

M. Potex has lately discovered two alkalis in the root of the *Chelidonium majus*; to the former he has given the name of *chelidonia*, and to the latter *pirropina*. To prepare these, the dried root of the Chelidonium is to be powdered and twice boiled in spirit; these infusions being distilled, they are poured into a cucurbit, and an equal quantity of distilled water being added, the alcohol is to be separated by another distillation. The residue is to be poured into a capsule; by perfect cooling a soft resin separates; the filtered liquor is to be precipitated by carbonate of soda, which throws down the two alkalis. Filtration is to be performed again, and the precipitate, after washing with cold water, is to be dissolved in boiling alcohol; the solution is to be filtered and allowed to cool. In this operation the greater part of the chelidonia crystallizes, and by evaporation more is obtained. The crystals are to be washed with alcohol to separate extractive. The mother-water and the washings are to be evaporated by a gentle heat, and then the pirropina crystallizes with a little chelidonia; these crystals are in yellow laminæ, and a portion is deposited on the sides of the vessel in blackish discs.

The properties of chelidonia are, that it crystallizes partly in transparent tables and partly in cubes, and varieties. In the state of crystals it dissolves with difficulty in hot alcohol and in æther; it crystallizes on their cooling. The concentrated acids, even when heated, act slowly on this substance; nitric acid gives it a yellow colour, and sulphuric acid blackens it. The diluted acids form colourless salts with it, which readily crystallize and have an astringent and very bitter taste. Chelidonia dissolves readily in the fat and volatile oils when heated; the solutions have a bitter taste.

The alcoholic solution has an alkaline reaction. If tincture of galls or subacetate of lead be added to a solution of acetate of chelidonia, abundant white precipitates are obtained. Tincture of iodine forms a kermes-coloured precipitate, chromate of potash a deep yellow, chloride of gold a dirty reddish yellow, and the alkalis give white precipitates.

The properties of pirropina are, that it forms stellated crystals, which are aggregated prisms; they are colourless and transparent, and on cooling they lose their transparency and become slightly brown. The acids, when cold, act upon it but slightly, but when heated they dissolve it, and become of a golden yellow or reddish colour; a powerful acid imparts a fine flame-red colour to the crystals. Its compounds are generally but little soluble in cold water, and have a slightly bitter taste, but are acrid and penetrating. Pirropina dissolves with difficulty in cold æther and alcohol, but readily when they are boiling. The fat and volatile oils dissolve it when hot. It fuses when heated, burns and yields ammoniacal vapours. Tincture of iodine precipitates acetate of pirropina of a crimson colour; chloride of gold yellowish brown, chromate of potash deep brown; tartar emetic, chloride of iron, proto-nitrate of mercury, bichloride of mercury, and nitrate of silver, all give yellowish white precipitates.

The alkalies form a white precipitate; tincture of galls and subacetate of lead produce no effect; the alcoholic solution has no alkaline reaction.—*Journal de Chim. Médicale,* Août, 1840.

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